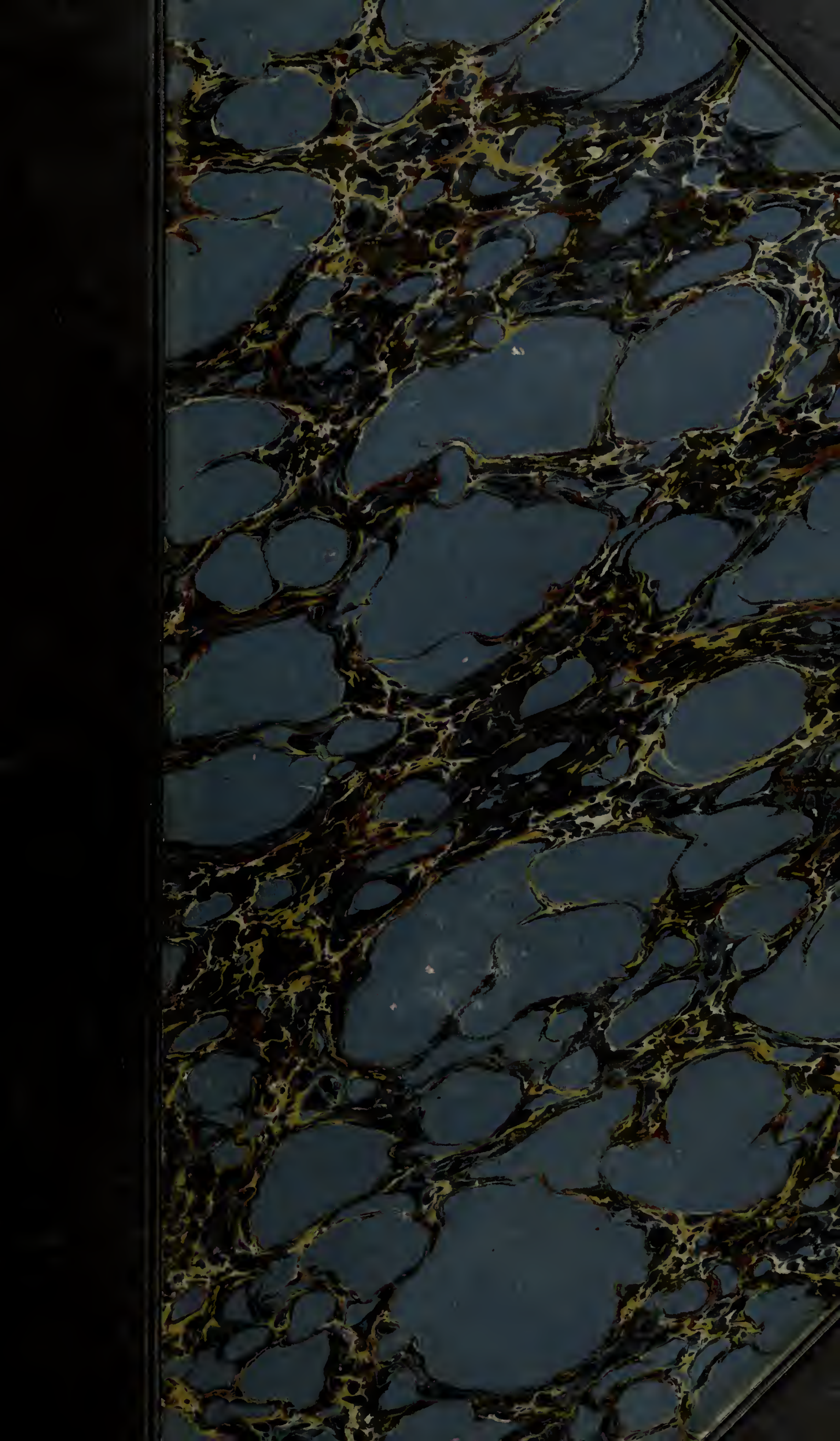

THE

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.L.S. G.S. Astr.S. Nat.H. Mosc. &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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CONTENTS OF VOL. XI.

NUMBERS LXIV. and LXV.—JULY, 1837.

NUMBER LXVI.—AUGUST.

NUMBER LXVII.—SEPTEMBER.

NUMBER LXVIII.—OCTOBER.

NUMBER LXIX.—NOVEMBER.

NUMBER LXX.—DECEMBER.

PLATES.

I. A Plate illustrative of Mr. BROOKE's Paper on the Crystallographical Identity of Phacolite and the Irish bipyramidal Levyne with Chabasie; and of the Rev. J. B. READE's Paper on Structure in the Ashes of Plants.

II. A Plate illustrative of the Rev. T. KNOX's Paper on his new Rain-Gauge, and of Mr. W. C. WILLIAMSON's Paper on the Affinity of certain Fossil Scales of Fish to those of the recent *Salmonidæ*.

ERRATA.

P. 447, l. 9 from the bottom, *for* "which the air solidifies," *read* "which avidity the air satisfies."

P. 460, l. 5 from the bottom, *for* "If a wire having a moist sponge be attached," *read* "If a wire have a moist sponge attached," &c. (See p. 567.)

P. 492, l. 4, *for* "a function of those coefficients," *read* "a function of those constants."

P. 516, l. 16, *for* "carbonic acid," *read* "carbonic oxide."

THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

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JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1837.

I. *On the Electro-magnetic conducting Power of Wires of different Qualities and Dimensions, and an Inquiry into the Efficiency of the Galvanometer for determining the Laws of its Variation.* By Peter Barlow, *F.R.S., Cor. Mem. Inst. France, &c. &c.*[*]

IN the Bakerian Lecture for 1833[†], Mr. Christie has given the details of a long and interesting series of experiments on the magneto-electric conducting power of wires of different lengths and diameters; from the results of which he infers that the law of conduction in this case is, that it varies directly as the square of the diameter, and inversely as the length of the wire; and in a subsequent part of the same article, by a comparison of the experiments of different authors, he infers that the same law has place in electro-magnetic conduction[‡]. My object in this paper is, to explain the anomalies that have been observed, and to inquire whether the deflections produced by the galvanometer ought to be considered as proper measures of the conducting power of the wire without reference to the power of the battery.

It is very desirable that we should be able to reduce the laws of electro-magnetic action to mathematical principles, and at a very early stage in the progress of this science I un-

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[*] Communicated by the Author.

[†] This paper was written in 1834, with an intention of publishing it at that time; it has however been lying in my drawer ever since, and as the subject is again brought forward, it may not, perhaps, be uninteresting to some of the readers of the Philosophical Magazine.

[‡] [An abstract of Mr. Christie's Bakerian Lecture will be found in Lond. and Edinb. Phil. Mag. vol. iii. p. 141, and some remarks upon it by Dr. Ritchie, in vol. iv. p. 208.—Edit.]

dertook the investigation of several of these laws, such as the general character and direction of the magnetic force, the law of its power as depending on the distance and relative position of the compass and conductor, and on the law as depending on the length of the conductor immediately acting on the compass, and some others. My paper on this subject was read at a meeting of the Royal Society in May 1823, and was afterwards published in the second edition of my Essay on Magnetic Attractions.

In the experiments above referred to, made with a view of determining the law as depending on the length of the wire, the whole length of the conductor remained nearly the same, and the question was only respecting the change of angle depending upon the length of that part of the conductor which acted directly on the needle; but in 1824, in continuation of these inquiries, I undertook to investigate the laws of the conducting power, or rather, perhaps, the laws of electro-magnetic intensity, as depending upon the actual length and diameters of the conducting wires. These experiments were published early in 1825 in the Edinburgh Philosophical Journal.

It had been ascertained at that time, that a diminution of deflecting power exhibited itself on the needle by any considerable lengthening of the conducting wire, and my first object was, if possible, to determine the law of this diminution, as also what the law was in wires of different diameters. The same experiments which I had proposed for this purpose would likewise, I imagined, determine another important question, namely, whether the effect was produced by the transmission of one fluid from the positive to the negative side of the battery; or whether two fluids rushed simultaneously from each pole; or lastly, whether the effect was due to the transmission of any fluid whatever. My reasoning stood thus: If the effect is due to a single fluid passing from the positive to the negative side of the battery, and the diminishing effect, by lengthening the wire, is due to a dissipation of the fluid in its progress, a compass placed near the positive pole ought to be more deflected than one near the negative pole; or if two fluids rush simultaneously from both poles, the compasses at these poles ought to be more strongly deflected than one placed in the centre, supposing, as I had done, that the diminished effect was the result of dissipation. I possessed at that time a powerful galvanic battery, consisting of 20 pairs of zinc and copper plates, 10 inches square, arranged after the manner of Dr. Hare's calorimotor; those of the latter being in connection with the copper lining of the battery, amounting to 16 square feet; so that I had about 30 square feet of zinc, and 46 square feet of copper, compressed within a small compass and acting as single plates.

With this power at my disposal I was anxious if possible to make the experiments upon such a scale as should leave nothing doubtful in the results. I accordingly procured about 900 feet of small copper bell wire, in one length. This was arranged in the manner hereafter described, and experiments carefully made upon it with three compasses at twenty different lengths, varying from 838 feet to 98 feet, and the results carefully read and registered. The three compasses were placed, one at each extremity of the wire and the other exactly in the middle of its length.

Before I proceed to state these results it may be well to refer to one marked peculiarity in them which is independent of the laws in question, viz. that with very trifling anomalies the three compasses placed as above were instantaneously and equally deflected, proving that the whole wire was in the same state of electric tension, and that the diminished effect by lengthening the wire was not, as I had at first supposed, due to dissipation during its transmission. The same result was afterwards independently obtained by M. Becquerel and communicated to the Royal Academy of Sciences of Paris, and is now I believe considered as an established law of electro-magnetic action.

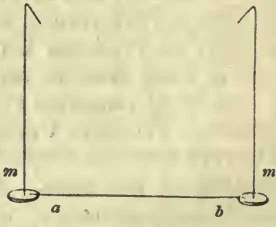
M. Becquerel also pursued a similar inquiry into the laws of conduction as depending on the lengths and diameters of the conductors; and here unfortunately are found discrepancies between the two results, which it is necessary to examine, particularly as Professor Cumming seems to have obtained results in many cases at variance with both.

Confining ourselves at present to that law which relates to diameter, Professor Cumming in different comparisons found it to vary from the simple first power of the diameter to nearly the cube of the same. M. Becquerel finds it to vary as the square of the diameter; while in my experiments, beyond very small limits, I found the deflections wholly independent of the diameter. Indeed if the deflection depended upon the square or any power of the diameter, it would follow that we might in any case supply a deficiency of galvanic power by only enlarging the diameter of our conductor, a doctrine which will not be maintained; and yet this is a necessary consequence if we admit the deflections to be a measure of the conducting power without limits; and if it has limits, on what do they depend?—is it absolute dimension, or has it reference to the intensity of the battery? This is certainly a question which requires investigation; for whatever it may be that establishes thes limits, beyond them the galvanometer must become an inefficient instrument.

In my experiments I employed twelve copper wires, each two feet long, varying in weight from 17 grains to 1590 grains, and twelve brass wires varying from 38 grs. to 3770 grs.; the squares of the diameters of course varying in the same ratio. One would naturally think, therefore, had such a law had place, it must have been rendered sufficiently obvious in these experiments; but so much was it the contrary, that I could perceive no increase of power in any of the wires which exceeded in weight 180 grs. for a length of two feet.

These results are therefore so decidedly at variance with the law above stated, that I shall be excused bestowing a few lines in describing the manner in which the experiments were conducted. This was as follows:

Two stout wires about $\frac{1}{4}$ of an inch in diameter were bent into the form shown in the diagram, their ends *m m'* being bent down about an inch, properly amalgamated, and inserted into two wooden cups containing mercury, these cups being about 20 inches apart. Two notches were cut in the sides of the cups *m m'*, in which were laid the specimens to be experimented on, these being placed so that the wire *m m'* was exactly in the magnetic meridian, and therefore parallel to the needle placed between them but considerably below the wire. These specimens it should be observed, were all also bent down an inch at each end and amalgamated, to be, like the poles of the battery, inserted in the mercury. The notches in the sides of the cups *m m'* were for receiving each successive specimen, so that no change should take place throughout in the relative position of the wire and compass.



The only remaining point to guard against was the variable power of the battery; and to provide against this, a standard wire, weighing 470 grains, was inserted in the cups, and the deviation produced by it observed and registered before and after that produced by each successive specimen; the mean of the former deflections was then considered as indicating the mean power of the battery for that experiment, and it was thus easy afterwards to reduce all the deflections to those which would have been produced by a constant power of the battery.

These observations and reductions are stated in the following table; and in the last column I have given the deviations which ought to have been observed supposing the power of the wires to vary as the square of the diameters.

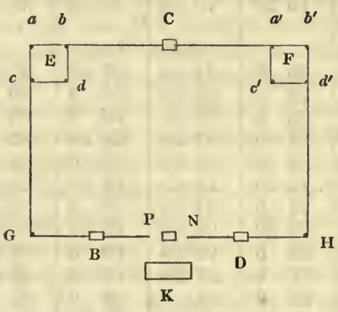
Table I.

Weight of Specimen.	Deflection by Standard before each Experiment.	Deflection by Standard after each Experiment.	Mean of the two preceding Columns.	Observed Deflection of each Specimen.	Observed Deflection reduced to mean standard 26°.	Deflections which ought to have been observed if the power had varied as D^2.
			Copper	Wires.		
17	39° 0′	37° 0′	38° 0′	25° 0′	16° 13′	16° 13′
49	35 0	33 0	34 0	31 0	23 29	37 59
59	33 0	31 0	32 0	28 0	22 32	45 17
70	31 0	28 30	29 45	28 0	24 24	50 9
95	28 30	27 30	28 0	26 0	24 6	58 25
140	27 30	26 30	27 0	26 0	25 1	67 20
180	26 30	24 0	25 15	25 30	26 15	72 1
250	22 30	23 0	22 45	23 0	26 16	76 51
290	23 0	21 0	22 0	22 0	26 0	78 36
580	20 0	21 0	20 30	21 0	26 35	83 3
1350	21 0	20 0	20 30	20 0	25 23	87 32
1590	20 0	19 30	19 45	19 30	25 11	87 54
			Brass	Wires.		
38	33 0	30 0	31 30	26 30	21 38	21 38
44	30 0	29 0	29 30	24 0	20 59	24 40
80	29 0	28 0	28 30	26 0	20 8	37 52
100	28 0	27 0	27 30	23 30	22 9	46 14
150	27 0	26 0	26 30	25 0	24 30	57 27
250	26 0	25 30	25 45	25 30	25 44	69 2
470	25 30	24 0	24 45	24 0	25 13	78 29
680	24 0	23 30	23 45	23 30	25 44	81 59
1330	23 0	22 0	22 30	22 0	25 26	85 53
1580	22 0	21 0	21 30	22 0	26 34	86 32
1890	21 0	21 0	21 0	22 0	27 10	87 6
3770	21 0	21 0	21 0	21 30	26 35	88 33

With such a series of results, obtained on a large scale, with a powerful battery, and with every necessary precaution to ensure their accuracy, it is impossible, using the deflections as the measure of the conducting power, to arrive at the conclusion, that the power of wires of different diameters and of the same length varies directly as the squares of their diameters, or as any power of the diameter whatever.

A similar discrepancy has place in the law which has reference to the length, M. Becquerel making the power vary inversely as the length, while my experiments make it vary in the inverse ratio of the square root of the length, or at least very nearly in that ratio. I am aware that assuming this law, viz. $l^{-\frac{1}{2}}$, the errors between the computed and observed variations average about a degree, and in one case out of twenty exceed two degrees, and for this reason I have only stated my result as an approximation; but admitting the power to vary inversely as the length, the average error amounts to nearly 20°, where the whole observed angle does not amount to 16°; and on this account I am again obliged to employ a few lines to explain the manner in which these experiments were conducted in order to trace that error to its source.

It is not easy to dispose of 838 feet of wire in such a way that a needle shall not be disturbed by parts of it not intended to act, and thereby the law of its action become misstated. I had experienced this difficulty in some preliminary experiments, which I attempted to make in a room with a much less length of wire; it is requisite therefore that I should explain the precautions taken to avoid this source of inaccuracy. On a grass plat in my garden I formed a rectangular figure represented in the diagram by G *a b′* H, driving into the ground strong stakes at G and H, and at *a* and *b′* short square open frames, 10 feet in periphery and 3 feet 6 inches high, (as shown by *a b c d*, *a′ b′ c′ d′*,) resembling the square frames or fences used for young-planted trees. This rectangle was so planted that G H, *a b′* were in the true magnetic meridian, and of course its other two sides at right angles to the same.



The wire being made fast at P, was turned round the prop G, whence it passed to the frame E, about which it was made to pass in 37 spiral threads from the bottom upwards; it then passed from *b* to *a′*, where it was made to descend in 37 spirals from above downwards; it then proceeded from *d′* to H, where it was turned round as at G to N. At P and N were placed the two cups of mercury as described in the last experiments, which latter were also connected with the battery K, as already explained.

The lengths of the several parts were P G = 15½ feet.
G *c* = 14½
37½ turns on frame E = 375
b a .... = 28
37½ turns on F ... = 375
D H .... = 14½
N H .... = 15½

Total = 838 feet.

The three compasses on which the observations were made were situated as shown at A, C, D; the standard compass for measuring the power of the battery was placed at A, and its deflection taken prior to each experiment, as described in the former case, and two separate observations were made on each length of wire by three observers, one at each compass *. These observations and those on the standard compass being made, the battery was raised out of the acid, and the wire shortened 40 feet by unwinding two turns from each frame: it was then again lowered, and similar observations repeated, and so on, till the length had been reduced from 838 feet to 98 feet. The compasses and wire were continued at the same distance at each station, till the wire had been shortened to 398; its action was then so strong that I was enabled after every observation at the usual distance, to obtain another double observation with the compass at 1½ inch distance; so that from 398 feet to 98 feet I obtained two distinct series, answering to two different distances. The comparative results were very nearly the same in both, but for the present comparison I shall only use the longer series, and state the mean of every two corresponding observations. Each individual observation may be seen in the work already quoted.

These mean results and comparisons are given in the following table. The 1st column contains the length of the conductor; the 2nd, the deflection of the standard compass; the 3rd, the mean deflection of the three other compasses; the 4th, these deflections reduced to a constant standard deflection of 21°; the 5th, the computed deflections assuming the law to be inversely as the square root of the length; the 6th, the errors arising from this assumption; the 7th, the computed deflections, assuming the law to be inversely as the length; and the 8th, the errors arising from that assumption.

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* It is proper to observe, that at the time I made these experiments, June 1824, I had attending me for instruction relative to my correcting plate several of the junior civil officers of the Dockyards, formerly students in the Royal Naval College, Portsmouth, to whose assistance I was much indebted not only in the experiments, but in all the previous arrangements.

Length of conductor.	Deflection by standard compass A.	Mean deflection of compasses B, C, D.	Deflection reduced to a standard of 21°.	Computed deflections and errors, assuming $\tan \Delta . \sqrt{L} = \text{constant}$*.		Computed deflection and errors, assuming $\tan \Delta . L = \text{constant}$*.	
				Defloctions.	Errors.	Deflections.	Errors.
Feet.							
838	21°	4° 55	4° 55′	6° 9′	+1 14	4° 29′	−0° 26′
798	24	6 18	5 26	6 18	+0 52	4 43	−0 43
758	25	8 12	6 46	6 28	−0 18	4 58	−1 48
718	25	8 50	7 17	6 39	−0 38	5 15	−2 02
678	26	9 10	7 4	6 50	−0 14	5 33	−1 31
638	25½	10 32	8 31	7 3	−1 28	5 53	−2 38
598	26	10 20	8 10	7 17	−0 53	6 17	−1 53
558	26	10 30	8 17	7 32	−0 45	6 43	−1 34
518	29	12 10	8 30	7 49	−0 41	7 14	−1 16
478	30	13 0	8 44	8 8	−0 36	7 50	−0 54
438	30	14 10	9 31	8 29	−1 2	8 32	−0 59
398	30¾	14 25	9 25	8 54	−0 31	9 23	−0 2
358	31½	15 10	9 38	9 22	−0 16	10 25	+0 47
318	34	17 0	9 52	9 56	+0 4	11 41	+1 49
278	34	17 15	10 1	10 37	+0 36	13 19	+3 18
238	35	18 20	10 18	11 27	+1 9	15 27	+5 9
198	33¾	18 55	11 8	12 31	+1 23	18 23	+7 15
158	34½	21 25	12 21	13 57	+1 36	22 37	+10 16
118	33½	23 5	13 53	16 3	+2 10	29 9	+15 16
98	32¼	24 40	15 37	17 30	+1 53	33 52	+18 15

On examining the errors arising from the assumption that the deflection varies inversely as the square root of the length, we find them amount on an average to about a degree, the maximum exceeding 2°; and Mr. Christie properly observes, that with such errors we cannot admit the law of the square roots. These errors however, employing the law of the inverse of the length, would amount to 18°; and it is, therefore, very desirable to trace the cause of the discrepancy to its source.

There can be no doubt that M. Becquerel's experiments were carefully conducted, and the means which I employed are stated above; there ought therefore to be found some primary cause for the disagreement, which is, after all, I suspect, not very remote. In order to lead the way to such an investigation, I shall offer a few suggestions which have occurred to me, leaving the bearing they may have on the subject to the consideration of others.

In the first place, I doubt much whether the deflection of a compass needle, either from the action of a single wire or from

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* The constants employed in these computations have been obtained by finding the value of all the $\tan \Delta \sqrt{L}$'s and taking the mean, and the value of all the $\tan \Delta . L$'s, and taking the mean of these.

that of what is called the galvanometer, is any certain measure of the relative conducting power of a wire, without reference to the intensity and productive power of the battery employed, whether our inquiries relate to their length, diameters, or natural qualities. To take the case of wires of the same kind and same length, it may perhaps be possible, where the productive power is great and the intensity inconsiderable, to employ a wire so small that it shall not be able to carry off the whole of the fluid the battery is competent to supply; and when this is the case a larger wire may be advantageously used, and if it does not greatly exceed the other, the power of conduction and the indications of the galvanometer may be consistent; but after employing a wire capable of conducting away the fluid as fast as it can be supplied by the battery, it will be useless to expect to produce a greater effect on the galvanometer by employing a greater wire.

Should this view of the subject be correct, it must be admitted that the indications of the galvanometer are no certain measures of the conducting power of wires, unless reference be had also to the intensity and productive power.

To take, by way of illustration, a more tangible, but somewhat analogous case, viz., the conducting power of metals for caloric. I would suppose a spirit lamp of given power and intensity to have one end of a wire placed in it, and the other end in a vessel of water with a thermometer; there is, I conceive, in this case no doubt that with small wires the thermometer would indicate their relative conducting powers with some degree of accuracy, but that it would fail entirely with very large wires capable of conducting away the heat faster than it can be generated.

I may also observe that as some metals are known to be better conductors of electricity than others, it is reasonable to conclude that a smaller wire of a better conducting metal will be as effective in carrying off the electricity of a battery as a larger wire of a worse conductor, and hence, probably, the discrepancies observed in the results of experiments on the relative conducting power of different metals. Suppose, for example, a copper wire to be employed, so small as not to be able to carry off the supply of the battery, then the same-sized silver wire will carry off more, and within certain limits the galvanometer may indicate something like their comparative powers of conduction; but if larger wires were employed a different result might be obtained, the justness of the indication depending, according to this view of the case, upon the circumstances whether both or either of the wires are larger than is requisite to conduct away the supply.

The following table shows the comparative conducting powers of different metals according to the determination of Sir Humphry Davy and M. Becquerel, the power of copper in each being called 100.

Relative conducting power of different Metals according to Sir Humphry Davy.		Relative conducting power of different Metals according to M. Becquerel.	
Copper	100	Copper	100
Silver	109	Silver	73
Gold	73	Gold	93
Lead	69	Lead	8·3
Platinum	18	Platinum	16·4
Iron	14·5	Iron	15·8

The names of these authors are a sufficient guarantee that each has reported his results correctly, and there must be therefore some cause for the discrepancies involved in their estimations which it is very desirable to discover; for till this is done we cannot be said to be in possession of any law of conduction in electricity derived from one source only, and much less are we enabled to assign the comparative conducting powers when derived from different sources.

According to the idea I have advanced, the galvanometer only measures the conducting power of a metal while the wire is so small as to be insufficient to carry off the generated electricity; beyond that point it only measures the intensity of the battery. Now this intensity certainly varies with the length of the conductor. Sir Humphry Davy, it appears, considered the law to be inversely as the length, and M. Becquerel states this law distinctly; while the experiments I have referred to make it nearly inversely as the square roots of the lengths. So that while M. Becquerel, by quadrupling the length of his wires, reduced his deflections to about one fourth, mine were only reduced to about a half, and with nine times the length to about one third.

My battery, as I have shown, had a large generating surface; what the other batteries may have been I cannot tell; but if, according to the view I have taken, my battery generated the fluid faster than the conductor could carry it off, and the others did not, we see at once why the intensity in my instrument did not exhibit the same reduction; but if from the nature and magnitude of these batteries this cannot be supposed, then this explanation fails and some other must be sought for.

When we reflect that besides this great discrepancy in the law of the length, we have a still greater in that which relates to diameter, and another nearly as large between the conducting power of different metals as given by various authors, including men of the first rank in the science, we must conclude that there is some inaccuracy, and that there are probably other circumstances influencing the results which are not at present understood. If the explanation I have attempted should be thought satisfactory I shall feel much gratified; and if not, and it should only lead to inquiries that shall elicit a satisfactory explanation, my object will have been attained.

P.S. My attention during the last three or four years having been withdrawn from electro-magnetic experiments, I was not aware till this paper had been set up that the subject of these discrepancies had been so fully investigated by M. Lenz, in an article in Taylor's SCIENTIFIC MEMOIRS, Part II. p. 311. The paper, as I have stated, was written in 1834, and my attention was only recalled to it by an accidental conversation with a foreigner, to whom, it would appear, Lenz's Memoir was equally unknown *.

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* [The paper by Lenz here referred to by Mr. Barlow is entitled "*On the Laws of the Conducting Powers of Wires of different Lengths and Diameters for Electricity.*" In it are successively reviewed the researches and reasoning on the subject, of Ritchie, Davy, Becquerel, Pouillet, Barlow, Cumming, and Christie; together with a formula derived from the theory of the galvanic battery by Ohm, which theory, M. Lenz remarks, "being only published in German, it is unknown both in France and in England." "This theory," however, he states, "explains perfectly the difference between Barlow's results and those of other natural philosophers who have occupied themselves with this subject, as well as the doubts of Ritchie." After his examination of the results obtained by the physicists above mentioned M. Lenz observes, "We perceive, then, by the above, that all the contradictory results of Barlow's, Cumming's, and Ritchie's experiments, in opposition to the law established by other philosophers, are reduced to a mere nothing by an accurate appreciation of the mode in which they performed their experiments;" and he states that "the axiom *that the conductibility of wires of the same substance is inversely as their lengths and directly as their sections,*" has been conclusively established by Ohm and Fechner. He then describes the results which he has himself obtained by the induced electro-dynamic current, and which are entirely in agreement with that axiom. The proposition "that in equally good conducting wires of the same substance the lengths are proportional to the masses, that is to say, to the sections," appears, it may be added, to have been first demonstrated by Davy. No. IV. of SCIENTIFIC MEMOIRS, to be published this month, will contain another paper by Lenz, On the laws according to which the magnet acts on a spiral, and on the influence of the distance of the convolutions of spirals on the production of the electromotive power in them.—EDIT.]

II. *On the Crystallographical Identity of Phacolite and the Irish bipyramidal Levyne with Chabasie.* By H. J. Brooke, *Esq., F.R.S., &c.*[*]

[With Figures: Plate I.]

SOME specimens of a mineral in small and nearly transparent bipyramidal crystals have been received in this country from Bohemia ticketed "Phacolite," and other specimens in larger crystals of nearly the same form have been sent from Ireland under the name of "Levyne." It does not appear by whom these specimens have been so named, but I find on examination of them that the crystals are alike, and that they correspond in primary form and angular measurement with Chabasie; the common twin crystals however of this substance consist of only *two* simple crystals intersecting each other, while those of Phacolite may be most easily explained by supposing them to consist of *four.*

As a more perfect knowledge of these composite forms will be useful to mineralogy, I am induced to request a place for the accompanying figures and remarks in the Philosophical Magazine.

Fig. 1. is a simple crystal of Chabasie, from the combination of two of which, the fundamental crystal, as it may be termed, of Phacolite, No. 2, is produced. The planes o have not been before observed on Chabasie. They result from the law $\overset{2}{B}$ of Haüy, the planes n corresponding to $\overset{1}{B}$, r to $\overset{1}{E}$, and u to $\overset{1}{D}$.

Fig. 2. shows the combination of two of these crystals, the planes o coinciding in surface so as to form one plane in the twin crystal, and filling up the re-entering angle in the common twin crystals of Chabasie.

Fig. 3. Two intersecting crystals which are supposed to cross each other, in the manner represented in the figure, *within* the crystals represented by fig. 4.

Fig. 4. The figure of Phacolite, the letters on the several planes indicating its relation to the preceding figures; with this difference however, that the planes corresponding to the P planes of fig. 4 on the actual crystals of Phacolite, are not single as drawn in the figure, but consist of a number of minute similar planes, which recede, as it were, behind each other so as to produce the appearance of rough single planes, nearly parallel to the planes n on which they rest.

Assuming P on P, fig. 1, to be 94° 46′, and denoting the

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[*] Communicated by the Author.

Chabasie.

Fig. 1.

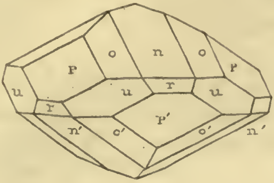


Fig. 3.

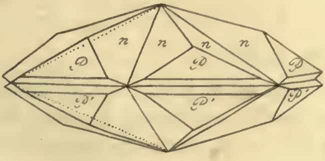


Fig. 2.

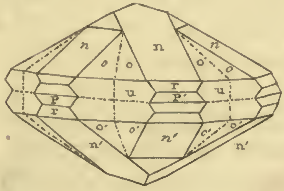
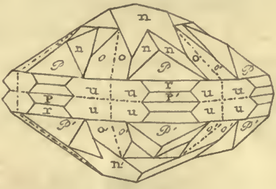
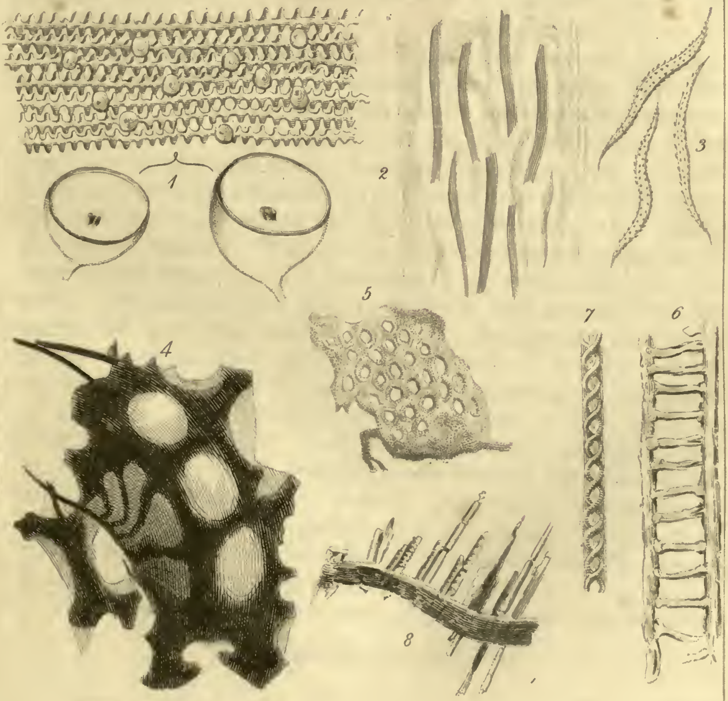


Fig. 4.



Ashes of Plants.



F. Brookes Sc.

axis by x, the following angles will be found very nearly correct:

x on P = 38° 24′
– – o = 53 56
– – n = 57 45
n on o = 162 53

H. J. B.

P.S. In No. 61 of this Journal, p. 278, the inclination of the plane P of Chabasie on the axis is given as 38° 34′ instead of 38° 24′, whence the inclination of P on g is 11° 36′ instead of 11° 26′.

The term *doubly oblique* prism in Prof. Johnston's paper on Baryto-calcite, inserted in Phil. Mag. for May, pp. 373, 374, and 375, should have been *oblique rhombic*, as correctly given in line 20 from the bottom of page 375.

III. *On the Existence of Structure in the Ashes of Plants and their Analogy to the Osseous System in Animals. By the Rev.* J. B. Reade, *M.A.**

THE broad assertion that a plant differs from an animal is so obviously true, that while points of difference rapidly present themselves, it appears to be a matter of no easy accomplishment to discover points of resemblance. Man is not more readily distinguished from other animals by his divinely bestowed power of *looking before and after*, than the generality of animals stand high above all vegetable creation by their powers of voluntary motion and perception. Yet there are links in the great chain of organized bodies so almost inseparably connected as to compel the naturalist to retire from his examination baffled at the question, Is it a plant, or is it an animal? The mere existence of organization is all that can be detected, and therefore it is impossible to determine with accuracy, whether the growth and structure of the individual have been accomplished by the agency of animal or of vegetable life. But though in some cases the shades of difference between the products of these two great kingdoms of nature so melt into each other as almost to lose their distinctness, there are in nearly all cases certain analogies both of parts and functions, and these not fanciful but real, which a close examination may detect. And hence it becomes a task as easy as it is delightful, to trace, in the uniformity of design and the infinite variety of modification in the execution, the hand of the Great Master-builder.

Functions of circulation and respiration, as well as func-

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* Communicated by the Author.

tions of nutrition and reproduction, are shared in common by all organized bodies; and it is through the medium of similar organs that the vital principle carries on these functions, both in the animal and in the vegetable kingdoms. Accordingly we find a muscular system in the former, a corresponding cellular system in the latter, and a vascular system in both. In carrying out these analogies further, it is not uncommon to find the stem and branches represented as a frame-work or *skeleton* for the support of the parts necessary to life. But we have already included the chief mass of these portions of vegetable structure in the vascular and cellular systems; and surely it would be unphilosophical to make the same parts subservient to the illustration of different analogies. While, however, we reject the stem and branches as the skeleton, we are not driven from the analogy; for plants as well as animals have an *osseous system*; and it is my design in the present paper to point out its construction and locality.

Having been requested by Mr. R. Rigg,—an able analytical chemist, whose valuable researches into the composition of vegetable products will ere long be made public,—to examine the ashes of plants with the microscope, I procured a platinum spoon and a large spirit-lamp as my working apparatus. Portions of plants were then submitted to an intense heat until the carbonaceous parts were entirely dissipated, and only a few apparently white ashes remained. The specimens thus incinerated consisted chiefly of grasses, together with barley, wheat, &c., and in all of them I have been able to discover, by means of the microscope, a most beautiful, and in many a most elaborate, structure. That this detection of structure in the ashes of plants is altogether new, I must infer from the silence of our best writers on the subject of physiological botany. The fact, had it been known, would have appeared far too interesting and important to be dismissed without special notice. The commonly conceived opinion is, to use the words of Professor Henslow, that carbon fixed under the form of a nutritive material is elaborated for the development of *all parts* of vegetable structure, and that those earthy, saline, and metallic ingredients which are found in the ashes of plants *being accidentally introduced*, cannot with any certainty be looked upon as products of vegetation, or as ever constituting essential elements of organization*.

Now, since the presence or absence of organization is direct evidence of the presence or absence of life, the first thing which strikes the mind under this newly discovered feature in

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* Cabinet Cyclopædia, Principles of Botany, pp. 176, 177, 224, &c.

the ashes of plants is, that combustion does not in this case, as we have hitherto supposed, supply us with brute matter merely, but that it leaves behind a purely vegetable product, a product far from being dissimilar in its nature to *the bones of animals*, and having its particles undoubtedly arranged by the agency of a living principle. Yet I confess that these are somewhat startling novelties; indeed, so much so, that I almost shrink from bringing before the naturalist a statement, which, to say the least, will be at first received with suspicion. The facts, however, he may easily verify for himself, and I can only believe that an examination similar to my own will conduct him to a similar conclusion.

It is almost superfluous to observe that bones contain, in addition to animal matter, salts of lime and soda, together with traces of silica and metallic oxides. The ashes of plants also, as is equally well known, are composed of earthy, saline, and metallic ingredients. We have here, therefore, two products, the one animal, and the other vegetable, differing chiefly in the proportions of similar elements. If it be asked how these elements are distributed in plants, whether in accidental accumulations or uniformly dispersed throughout their volume, all we know of creating intelligence urges us to say, that certainly the dispersion will be uniform, or at least systematic. We cannot, therefore, be surprised to learn that such an arrangement is actually detected after combustion, though it may be gratifying to know that combustion does not disturb so as to conceal it.

What I wish then more especially to insist upon with respect to the ashes of plants is *structure*,—the similar conformation of similar parts, whether those parts be stems, leaves, or the appendages of flowers and seeds. The variety is evidently a variety of purpose and plan, compelling us to reject at once every supposition of the operation of causes without design. The inability to comprehend the use of this construction is no argument against the subtlety of the mechanism. The bare existence of structure is of itself proof sufficient of the active presence of a living principle, and therefore of a contrivance accommodated to some end, and suited to some office. That end and office, in the present case, may be to give consistence and support, or there may be some mysterious connection even with the healthy existence of the plant. For did we find the deposition of matter, like *silica* along the angles of the *Equisetum hyemale*, occurring in small masses, or as lumps, like tabasheer between the joints of the bamboo, we might with justice suppose, that what seems to be so casually introduced might be withheld, or if possible removed, without interfering with the process of vegetation. But since the residual matter which combustion separates is as it were carefully arranged, in certain definite forms, throughout the entire plant, those forms varying uniformly in different parts of the same plant but preserving many similar characters in similar parts of different plants, we cannot suppose that there is no connection between structure like this and the general œconomy of vegetation, or that so concealed but curious a contrivance has no share and interest in the functions of vegetable life.

We may also further infer that there is a *chemical union* of the earthy, saline, and metallic ingredients which the ashes of plants contain. If these ashes were wholly destitute of structure, we might with justice suppose that they contained their elements in mechanical combination merely, each particle being a pure portion of a separate element. But the fact of organization compels us to conclude that, in each and every particle of the incombustible residuum, every element is combined under the operation of a natural chemistry. And hence, under this impression, we can pronounce the ashes of plants to be a purely vegetable product, equally with the nutritive products, starch, sugar, and gum.

Whether the physiologist will condemn as fanciful and vague any idea of analogy between the bones of animals and this systematic distribution of incombustible matter in plants; or whether,—bearing in mind that created things differ in magnitude preeminently,—he will be disposed to confirm such speculations; these are points which I cannot decide. Of this, however, I feel confident, that every lover of the microscope will be glad to place in his cabinet a series of objects which, to say the least, will call forth his admiration, if they do not also awaken a suspicion that he is examining structure which has been obedient to some rule, and is therefore conducive to some effect.

Peckham, April 27, 1837.

Note.—The above observations may possibly tend to throw some light on the natural process of the silicification of wood. By the agency of an intense heat the surrounding siliceous matter may be liquefied and the carbon and gaseous products of the wood dispelled, while the essential characters of the fibrous and cellular structure are undisturbed. The unconsumed portions, which alone constitute the true vegetable frame-work, are then, as it were, *mounted* in the fluid silica. This property of retaining its form notwithstanding the action of heat, which seems to be a characteristic of fibre, suggested to me the probability of detecting structure in the ashes of coal; and, upon examination, I find that the white ashes of "slaty coal" furnish most beautiful examples of vegetable remains. We have thus additional evidence that the *basis* of vegetable structure is independent of carbon.

Explanation of the Figures. (*Plate* I.)

Figs. 1, 2, 3. Skeletons of portions of recent plants.
- 1. Part of husk of Oat, with separate drawings of the cups, which are attached at nearly uniform intervals along the siliceous columns.
- 2. Part of leaf of the Iris.
- 3. Hair of leaf of *Cornus alba* (Common Dogwood).

Figs. 4, 5, 6, 7, 8. Siliceous skeletons of portions of plants occurring abundantly in the white ashes of coal.
- 4, 5. Cellular structure.
- 6. Annular ducts with transverse bars.
- 7. Spiral fibre.
- 8. Fibre *in situ.*

Magnifying power about three hundred linear. The parallel siliceous lines of the Oat, occurring in some cases at intervals of 1–4000dth of an inch, form a very delicate *natural micrometer.*

P.S. Since writing this paper, I have been indebted to Mr. Brown's kindness for the perusal of Struve's Inaugural Dissertation, "De Silicia in Plantis nonnullis." It is the author's object to show that pure sillica forms the skeleton of three species of *Equisetum*, and also of the *Spongia lacustris* and *Calamus Rhodan.* I am gratified by finding the following remark: "Sub aëris libero aditu ustis, restat sceleton, totam plantæ formam accurate servans, partibus animalium osseis quam maxime comparandum." p. 12.

My attention has also been directed to Mr. Lyell's observations on Göppert's Memoir on the Process of Lapidification, Phil. Mag., May 1837, p. 408, and Ehrenberg's Memoirs on Fossil Infusoria, Scientific Memoirs, vol. i. part iii.

IV. *Notes relative to the Geology of a Portion of the District of Holguin in the Island of Cuba, and the Mineral Region on the North-east Coast, from the Observations of himself and* THOMAS G. CLEMSON, *Esq.* *By* RICHARD COWLING TAYLOR, *Esq.*, *F.G.S.*, *&c.**

WE have prepared a detailed description of the mineral region in the vicinity of Gibara, and particularly as to its copper lodes; but as some delay will unavoidably take place in its publication, I have arranged a portion of our notes as a preliminary communication to the Philosophical Magazine.

A considerable portion of the year 1836 was devoted to an examination of the north-east part of this island, a mineral region which, so far as I can learn, has never been visited for scientific purposes, and till recently has never been investigated for the practical objects of mining.

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* Communicated by the Author.

The area of which I propose now to give a sketch is situated between the city of Holguin and the sea, and forms a mountainous belt or zone parallel with the northern coast of Cuba. That part of it, at least that part which is within the limits of this investigation, in which copper lodes of value have been proved, appears to be only two or three miles in breadth. The centre of this mineral range is about eight miles, in a straight line from the *Embarcadero* or landing-place of the Gibara river; from hence that stream is navigable for lighters four or five miles to the bay and port of Gibara. The known copper lodes are almost entirely limited to the Savanas, which it may probably be not uninteresting first to describe.

Natural Features of the Savanas.—This term, in the island of Cuba, implies an elevated hilly range, for the most part clear of wood, excepting the *Corojo* palms, the Palmettos of two or three species, and some occasional patches of low thorny bushes, aloes, and beautifully flowering shrubs, peculiar to these sites. The surface is everywhere thickly strewed with detritus of the prevailing serpentine rocks of the district, and is covered with a coarse description of grass, rejected by cattle, and which is commonly fired in every spring, either for the improvement of the scanty pasturage, or from time to time, to facilitate the search after copper veins.

Innumerable small streams and ravines, whose beds are dry during the greater part of the year, wind amidst the Savanas in the most intricate manner; descending to larger streams on their way to the main outlet at the bay of Gibara, or to that of Barriay and others which indent the north-eastern coast of Cuba. Their courses in the elevated country are distinguishable by the superior luxuriance of the vegetation along their banks. These streams, with their borders of rich and woodland soil, form the boundaries of the numerous distinct Savanas, each of which bears its separate name.

Smaller detached areas of open and unimproved land, resembling the Savanas, appear on the hills which rise from the midst of the surrounding woods. They are known by the denomination of *Saos*, and are kept open by occasionally firing the coarse herbage. The rocks which appear on their surfaces are of the same character as those on the Savanas, and sometimes indicate mineral traces.

Still smaller open and elevated areas are called *Saoitos*, and partake of the common character, having a few straggling palmettos and mahogany trees dispersed over their rocky surfaces.

The Savana region, whose extreme limits are imperfectly defined, and of which but a comparatively small portion has at present been ascertained to contain veins of copper, is remarkable for the undulations of its surface, and consists of a countless series of rounded hills, which rise from one hundred to four or five hundred feet above the bed of the principal streams. Some of these hills are prolonged in the form of ridges, stretching several miles in a direction varying between north-east and east; their sides penetrated deeply, at inconsiderable intervals, by innumerable lateral ravines. Other eminences are so surrounded by watercourses and ravines as to appear in regular oval or circular forms, as if constructed by the hand of man, like the ancient mounds and earth-works of Europe, on a gigantic scale. Some of them are a few hundred yards only in diameter, and rise from two to four hundred feet in elevation, and slope on all sides with a remarkable uniformity. To speak of these conformations of the surface, chiefly derived from the slow operation of drainage, as mere undulations will scarcely convey an adequate impression of the superficial character of this region. It is seldom that so extensive an expanse of country is seen, which, without presenting any precipitous faces of naked rock, maintains over its entire area a surface so uneven. Standing upon one of these eminences in the midst of the savanas, and looking over those countless barren and rounded hills, the only scene to which they may be compared, yet on a very inferior scale as to magnitude, will be the billows of an agitated ocean.

These circumstances of external configuration are not unimportant to the miner, who will perceive the facilities they offer to his operations, and how readily these ridges and hills, containing the mineral veins, can be reached, and worked, and drained, to a great depth, from the adjacent ravines.

White Limestone Mountains.—Whilst describing the peculiar features which characterize this part of the island, and which are mainly attributable to geological phænomena, we may not pass unnoticed those remarkable mountains of limestone or marble, which not only approach the borders of the savanas, but even rise in the midst of them.

As we approach the bay of Gibara from the sea, the aspect of the coast and country inland is bold and striking. Mountains of strange forms, pinnacles and isolated bluffs, and elevated saddle-shaped masses, steep and bare of vegetation on their faces, range along the coast at the distance of a few miles in the interior.

We cross them, and amidst them, and pass to the savanas in their rear, towards the south. From hence, as they stretch, at intervals, to the east and to the west, a scene unusually striking and geologically interesting presents itself.

From the midst of the barren savanas and the lower wooded plains arise those lofty detached mountains of compact marble, whose singulár forms, and whose white, precipitous, waterworn sides, contrast so remarkably with the rounded, sunburnt hills of the savanas, and give such a peculiarity to the contour of this coast, and furnish to the mariner such conspicuous landmarks. At a little distance, and even at several miles from the base of these mountains, their precipitous faces appear vertically and distinctly striated, like clusters of enormous columns, hundreds of feet in height. We at first attributed this appearance to the possibility of a section of vertical strata being thus presented. But on closer examination we found no such traces of stratification: all sides appeared to possess the same singular, strongly marked vertical lines; and we saw that this remarkable columnar appearance was derived from the erosion of the perpendicular face of the hard rock, wherever exposed to the atmosphere, into deep vertical grooves or flutings, on a large scale.

On the mountain of *La Silla* this phænomenon is beautifully exhibited; and when from its summit we looked down upon the numerous spurs of this mountain, and upon its surrounding masses, we had the singular prospect of apparently an immense assemblage of groups of enormous crystals of white rock, distributed over a space more than a mile broad and two or three miles long, shooting perpendicularly upwards from the woods below, and contrasting strongly with the dark green foliage of a tropical forest.

From the bases of the *Toro loco*, the *Llavason*, and the *Siera alte* mountains, particularly the former, an equally grand and singular view is presented, resembling snow-white basaltic pillars, clear of all vegetation on their sides, except here and there an aloe rooted in some crevice. At this distance the illusion was equally beautiful; when looking to the irregular outline of their crests, it seemed as if the entire mountains, a thousand feet in height, formed one enormous group of vast crystals.

Intermediate Valleys and Plains of rich Alluvial Soil. — In close contiguity to, and intermixed with many of the savanas, are extensive tracts of low and comparatively level alluvial land, the richest, the most luxuriant, and the most prolific, perhaps, in the world. A very small portion of this land is otherwise than in its primitive state of nature, and is covered with timber of the most valuable properties. When cleared, its fertility is apparently inexhaustible, requiring no manure, and it is capable of yielding two or three crops in one year.

This singular intermixture, this close approximation of absolute barrenness and redundant fertility, is not the least striking among the peculiarities of the region under consideration. We have chosen here to advert to it, because it cannot fail to be seen from this circumstance, that in an œconomical point of view it bears materially upon the convenience, and consequently upon the local value, of sites hereafter designed to be scenes of a busy population connected with the mines, which though seated within an area of positive sterility will derive incalculable benefits from their proximity to one of the utmost fecundity.

Rocks of the Savanas.—Our notice of the rocks and nature of the ores of this region will be brief, and will be reserved in detail for another place. The rocks of the mineral district may be divided into two classes: those which contain much diallage and are more or less crystalline in their structure; and those in which that mineral is wanting or is less prevalent; and they all may be arranged under the head of serpentine rocks.

They are sometimes distinct, and, again, are mixed in all proportions. The surface rock, as well as that which has been extracted from the shafts and levels but which has been exposed for a while, is more or less in a disintegrated form; of a soft unctuous touch, and easily reduced to powder. As the constituents of this rock happen to vary, it changes into one of a petrosiliceous nature, is hard, and resists the disintegrating nature of the atmospheric agents. We have previously made mention of this class of rocks in our account of the bituminous veins within them, in the vicinity of the Havana*.

Copper Ores.—The surface ore, or the mineral substance containing copper, that is found at the outcrops, or upon the back of the veins within a few feet of the surface of the ground, differs materially in its physical characters and in its chemical composition from that ore which is found to predominate in the same lode at greater depths. The term surface ore is here applied to amorphous or informal masses of mineral, of different colours, containing more or less metallic matter.

Frequently at the outcrop of veins containing cupriferous ores, the term surface ore is applied to a heterogeneous mixture of several distinct mineral species, of which copper forms one of the constituents, associated with other minerals of little or no value. These ores are generally red, brown, black, green, and the different hues that grow out of an indiscriminate mixture of those colours; the most prominent of these species being the oxides, sulphurets, silicates, and carbonates of copper, with iron, &c. As we descend upon the vein the

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* See Lond. and Edinb. Phil. Mag., vol. x. p. 161.—Edit.

ore assumes a different character. The copper is then found in combination with sulphur and more or less iron; the mineral having a foliated structure, which it owes to an intimate mixture with the foliated magnesian rock, its gangue. At the depth of ninety feet, as we observed in the Buena Isabela mine, that structure is lost, and a more compact and permanent form is assumed. Occasionally the foliated character is maintained, but it is not so marked as nearer the surface*. This ore is raised from the depth quoted in masses of from fifty to three hundred pounds weight each, and free from gangue; but masses have been detached, by blasting, of the estimated weight of one thousand pounds.

Native copper is met with, particularly at the mine of *San Fernando*, on the upper portion of the lodes, and descending to the depth of thirty yards. This occurs in masses of from ten to two hundred or more pounds weight.

Chromate of Iron of great purity occurs in beds and irregular veins in the serpentine rocks at several places in this district. At one point masses containing many cubical yards project several feet above the general surface of the savana.

Discovery and Progress of the Mines in the Savana Region.—It does not appear that any knowledge of the actual existence of lodes of copper on the north-east side of the island of Cuba prevailed before the year 1830. Soon after this time, however, a couple of Mexican working miners were employed to explore for gold amongst the hills and open savanas within the district of Holguin. It was during their ineffectual researches for the more precious metal in this quarter that the first copper veins were discovered; and subsequently the denouncement of *San Fernando*, containing three veins, was commenced, and entrusted to the management of a Mexican manager, by John Bedopia, Esq., an English resident on the island. We have taken the liberty of mentioning this gentleman personally, because to his individual enterprise we are in a great measure indebted for determining the existence of mineral veins within this district.

The mine of *San Fernando* has continued to be worked by negro labour, although but slowly, and under all the disadvantages of the old Mexican system and incompetent management. The ore is a sulphuret, of a bronze green colour, rich in copper, and intermixed with rich gray ore, and, to the depth of the first 30 yards, with native copper.

In the same vicinity have been subsequently made by the same proprietors the denouncements of *Socorro*, *San Antonio*, *San Juan*, *Mina Inocentes* and *San Olivo*.

* T. G. C.

In the mean while, during the active search for gold by numerous individuals, discovery was made of the vein denounced under the appellation of *San Augustin*, and which now comprises the four veins of *Prosperidad*, *Santa Isabel*, *San Augustin*, and *San Nicolas*. The ore of *San Augustin* is as rich as any in the district, so far as has been examined, varying from 23·30 to 51·60 per cent.

The denouncements of *La Buena Isabela* and *Perseverancia* commenced being mined in 1834. A few English miners were employed in the former in the year 1835, and after proving the vein, the works were suspended on account of some temporary difficulties on the part of the owners.

Stimulated by the success which attended the mining of the *Cobre* veins in the vicinity of *St. Iago de Cuba*, the researches on the north side of the island were continued on a limited scale by a few individuals. During the last two or three years, notwithstanding no mining undertaking had been conducted so far as to bring in a single dollar, great activity was exhibited in searching all the savanas through a great range of country. These explorations, however eagerly prosecuted, have up to the present moment led to no other new denouncement than that of *Savana Veija*. Indications and traces have been observed at detached positions, but among these no works, except of the most trivial and superficial nature, have been proceeded with.

All the denouncements made up to the time we are now writing are comprised within an area of only five miles in length by two in breadth. That of *Savana Veija* is among the most promising. The principal vein was discovered in 1835, but copper had been traced at one or two points on this savana three or four years earlier. There appear to be seven or eight good veins here, which are imperfectly proved. No capital has hitherto been employed in this undertaking; and in fact this may be said of the entire region, with the exception of the *San Fernando* and the Good Isabella mines, and even in them it has been expended to a limited extent only.

Assays of the ores from the various denouncements within this region have been made in abundance, with a view to the ascertainment of their quality. These results, however satisfactory, it is scarcely necessary to communicate in detail here.

White Limestone of Holguin District.—I add a few additional notes relative to this rock.

Having examined numerous mountains, hills, and belts of this beautiful rock, and traced their connection with the adjacent formations, we were led to the opinion that it is of the same geological age as the serpentines, the greenstones, the diorites, and the euphotides which occupy so large an area in the island of Cuba.

In the vicinity of *Savana Veija* are limestone hills, composed of very thin layers or laminæ, dipping to the south from 50° to 75°. This rock is white, occasionally tinged with green, and contains numerous interposed beds, varying from half an inch to 4 inches thickness, of red and flesh-coloured crystalline limestone. In other situations we observed that this limestone is either cream-coloured, or with various delicate tints of yellow, green, or pink.

The Gibara river is crossed repeatedly by bands of this limestone, which are traversed by a network of quartz veins. In these positions the rock exhibits evidence of having been shattered and broken, the fragments being reunited in a siliceous cement, and so distorted that the original arrangement of the laminæ or seams of the limestone is obscure and almost obliterated. In general all these traces of original stratification are absent, which inclines us to the opinion that we see the mass only in a modified form, and that it has been subjected to the same influence which has changed the adjacent rocks, and modified the quartz into a substance resembling porcelain, and converted the serpentine almost into a vitreous slag.

We examined two small conical hills of unstratified white limestone near the *Sao Gibara*, which seem to be surrounded by greenstone. We may mention here that we have observed greenstone at the base of most of these limestone mountains; among others in that of *La Silla*, where the greenstone seemed confounded or intermixed with the limestone. We shall advert to this further on, as a fair illustration of a class of mountains which characterizes so strongly the eastern parts of Cuba. The summits of these and other hills of similar character are broken into large masses, and exhibit extensive fissures, affording hiding-places to the numerous wild dogs which infest the country.

The structure of this beautiful marble is extremely fine and compact, too much so, I am informed, to admit of its adaptation to external building purposes, as I had anticipated; but for finished and more delicate work in the interior, and for the ornamental departments of architecture and sculpture, it appears well adapted. Its fracture is conchoidal; its colour commonly white or cream-colour, or slightly tinted, and its texture inclined to waxey. The specific gravity is great. Blocks of almost any magnitude may be obtained without a flaw. When struck with a hammer the loose fragments emit a sonorous ringing sound. Upon the mountains all the exposed surfaces are honeycombed and have sharp projecting points as in the coral rock of the coast.

Upon the skirts of most of these mountains are tufaceous deposits, covering the surface; which calcareous tufa has been derived from the decomposition of the rock, and is brought down by the numerous springs which descend the hills. This soft tufa incloses various extraneous substances, and contains beautiful impressions of leaves and vegetables.

Organic Remains.—In this rock traces of organic remains are so extremely rare, that during months of examination but two or three specimens came under our observation. They were madrepores, the structure of which had been partly obscured by crystallization, or by the change which the mass of limestone had undergone. We have not observed the slightest trace of a shell in this formation. It is possible that it may have originally contained organized fossils which have been obliterated by the modification to which the rock has evidently been exposed. No mineral veins or substances were detected in it.

From the foregoing description it would appear that this rock or marble is neither the white limestone with tertiary shells, described by Mr. De la Beche as abounding in Jamaica; nor the compact lithographic limestone, sometimes containing Pectens, Cardites, Terebratulæ, and madrepores, described by M. de Humboldt at the west end of the island of Cuba, under the name of *Calcaire* [Jurassique?] *de Guines*, which we ourselves have had some opportunity of examining in the vicinity of Havana. We conceive that it is more ancient than those rocks; and that it is contemporary with the Euphotides and metalliferous serpentines of this region.

It is a prevailing character of the Holguin and Gibara limestone that it contains large masses of carbonate of lime of a much later origin. These are accounted for, on the supposition that they were open fissures, cavities, and even large caverns, which in process of time have been wholly filled by stalagmitical infiltration. All these later deposits are of a brick-red colour, remarkably fetid, and embrace vast quantities of casts of land shells, occasionally intermixed with marine univalves, and with a few small bones, apparently of the great Indian rat, one of the very few indigenous quadrupeds of the island, and now inhabiting the same mountains.

La Silla.—This singularly-shaped mountain of white limestone is about two miles long and one in breadth. Its sides, towards the summit, are bare and perpendicular, so that only at one or two points is it practicable to attain the top, by the assistance of the luxuriant foliage of certain creeping shrubs, or an occasional tree growing amongst the recesses and crevices.

The barometer being injured in the ascent, no correct calculation was made of the height of *La Silla*, which we estimated at from one thousand to twelve hundred feet above the sea, here distant about eight miles.

Contrary to our expectation, we found that the summit was limited to a narrow ridge; a mere vertical wall of honeycombed limestone, on which it was practicable only in a few places to obtain sufficient space for standing, assisted by the shrubs that were rooted in the crevices.

We have adverted to the singularly beautiful appearance of these rocks when viewed either from the crest or from below; which effect was produced by the vertical wearing or erosion of the white marble in grooves, giving them the aspect of enormous columns and of gigantic groups of crystals.

The upper surfaces of this rock are deeply honeycombed; that is to say, they have numerous sharp projecting points, on which it is dangerous to walk, and have also innumerable circular holes two or three inches in depth and one inch or more in diameter, perfectly smooth and regular in their interior, as if bored with an auger.

No stratification can be perceived in this enormous mass of limestone. Certain lines of separation there are, which may be traced at various angles, as well as vertical fissures, but none of these were the result of stratification. Large blocks are commonly seen piled on the sides near the summits of these mountains. They are remarkably sonorous when struck, ringing under the blow of the hammer like a bell.

Land-shell Limestone of La Silla.—We have adverted to the barrenness of the white marble in organic remains, and we obtained from hence only a single specimen, a madrepore, which was somewhat modified in form.

We have now to describe a deposit, rich in fossil shells, of a novel and remarkable character.

On the surface and on the sides of *La Silla* is a great abundance of subsided masses of stalagmitical rock, derived from the limestone, and filled with hollow casts of an immense assemblage of univalve shells, which at the first glance I thought were tertiary. On examination these shells were observed to be peculiar to a separate rock of a brick-red colour, but the position of that rock was not immediately discovered. This red earthy stone was also crowded with small spherical bodies; black, smooth, polished, with minute rounded and kidney-shaped pebbles, from the size of mustard-seed to that of a small pea. They were unequally distributed, and in some specimens but few can be observed. The shells were in almost every instance enveloped in crystallized carbonate of lime.

During subsequent researches we ascertained that the red rock was not interstratified with the white limestone, but occupied the spaces formed by ancient open fissures, sometimes a few inches only in thickness; but in one instance, within fifteen feet only of the summit, it was exhibited of the breadth of fifty feet, and of a thickness varying from ten to thirty. This mass was evidently composed of a numerous series of deposits, having different tints, and being more or less earthy or crystallized, emitting an offensive odour when broken.

It was not until after two or three visits to this mountain that we traced satisfactorily the extent of these insulated shelly deposits, acquired a knowledge of their origin, and perceived the process by which they were consolidated. It became evident, on comparison, that these univalves, which were as abundant as in any deposit we have seen, were referrible to *land genera*, such as exist in profusion among the rocks of the same mountain, amounting to eight or nine species at least. The inhabitants of these shells retire into the open fissures and caves, and in some instances are probably carried into them; and the dead and unoccupied shells lie in vast numbers in such places, in every instance mixed with the red earth, similar in colour to the more earthy portion of the consolidated rock. These caves are resorted to by multitudes of bats, and beneath the holes in the roof where they most congregated I observed heaps of the dung of these animals, of a bright red colour, as if derived from some seeds or berries upon which they had fed. It is possible that a portion of this red colour of the earth and of the consolidated rock was derived from this source, and probably the globular bodies to which I have referred originated in the same way.

By the stalagmitical process the shells, together with other extraneous substances within its influence, become enveloped in crystallized carbonate of lime, and the operation of infiltration may be seen slowly but uniformly proceeding, until caves and fissures of considerable size are wholly filled up with the new mass, and are consolidated with the older limestone. Sometimes the mass contains brecciated fragments of this white limestone. It approaches in colour to the osseous breccia of Gibraltar. We had little hope of discovering bones of large animals in the caves of an island which contained on its discovery no quadruped larger than the Indian rat. But we met with fragments of bones which we referred to the *Hutia*, or large Indian rat of the island, which is nearly the size of the raccoon. The *Hutia* is the largest if not the only native quadruped of Cuba, and we observed it on the very spot where we conceived we had recognised its fossil remains. Its flesh is sought by the negroes as a favourite food.

On first consideration it appeared almost incredible that land shells should accumulate in such multitudes as we find them, packed together in layers irregularly within the red stalagmitical limestone. But having witnessed in the caves of *La Silla* the myriads of dead shells there assembled and lying in heaps upon the floor of red earth, we perceived that the process was then going on before our eyes, and that by the infiltration and crystallization of carbonate of lime, these shells were entombed, consolidated, and the whole mass converted to a beautiful marble. This process continues until some of the caves are completely filled up, and the recent mass becomes an integral part of the original rock.

In the interior chambers of the caves, which were the furthest removed from light, the only living shell I observed was a *Clausillia*, which adhered to the walls in great numbers.

I may observe that no traces of the original shells were noticed in the newly-formed rock; the matter of the shell seems to be absorbed during the change it undergoes, and its conversion to solid rock.

There yet remains an interesting fact to be pointed out. Amongst the numerous land-shells which the red shelly modern rock exhibits may be detected, occasionally a univalve which is decidedly marine, and the same circumstance is observable among the dead land-shells in the caves. It was this occurrence of marine shells that occasioned our hesitation, and prevented us from earlier deciding on the real origin and character of the new red shelly limestone. We perceived that perfectly fresh or recent marine univalves were by no means uncommon even upon the highest crest of the mountain of *La Silla.* This mystery was solved when we found that the active agents in the transportation, and in the admixture of sea and land exuviæ, were the soldier crabs, which abound here, and which inhabit the littoral shells almost wholly, wherever they are found, borrowing those habitations for their temporary purposes, and discarding them when too small for their convenience.

The soldier crabs (genus *Pagurus*) at certain seasons resort to the sea-shore, where we have seen them in great quantities. They return from their pilgrimage, carrying or rather dragging the shell of some marine univalve for many a weary mile; and thus, like the pilgrims of the olden time, each bearing his shell to denote the character and extent of his wanderings, they proceed for miles into the interior. Thus, at the distance of eight or ten miles from the nearest shore, we trace them to the very summit of a precipitous mountain, almost twelve hundred feet high. When these borrowed habitations become too confined for the accommodation of their tenants, the crabs desert them, and seek for larger shells, leaving the others mingled with the terrestrial shells, as we observed. The marine shells which had been thus conveyed from the sea to the top of *La Silla* were

Trochus, two or three species.
Turbo, two or three species, particularly *T. muricatus*, Lam.
Littorina, one.
Monodonta, one.

No doubt other genera and species were also transported thither by the same agents, but those above mentioned we can testify to.

Of the land shells, which exist equally in a fossil state on the mountain of *La Silla*, and were seen abundantly in their living state in the same locality, we have collected the following, of which Mr. Isaac Lea has kindly furnished us with the specific names.

Cyclostoma sulcata, var.	Lamarck.
Rupa mumia	Lam.
Carocolla marginata	Lam.
Carocolla ——— ?	
Helix microstoma	Lam.
Helicogena auricoma	
Helix muscarum	Lea.
Helix purpuragula?	Lea.
Clausilia ——— ?	

Caves of La Silla.—At about a hundred and fifty feet below the summit of the mountain of this name is an extensive suite of caves. Those we passed through, six in number, extended above three hundred feet to the south, and others stretch off to the north, in the compact white limestone to which we have referred. These caves swarmed with bats, snails, spiders, tarantulas, scorpions, and other vermin, and the large snake called the *Majus*. They are also the resort of hogs, which run wild in the woods, and bring hither a well-known pest in the insect termed the Jigger.

The entrance to the cave is at the bottom of a perpendicular cliff of limestone. The interior is not unlike to an Anglo-Norman crypt, having a heavy groined roof and pillars of continually increasing stalactite. This tendency to encrust the walls or sides contracts the areas of the chambers, to some of which the apertures are nearly closed up. All these chambers have nearly level floors, covered to an unknown thickness with red earth or mould, on which is thickly strewed the dung of the thousands of bats which congregate here, and myriads of snail-shells. I think the dung of the bats alone would be in sufficient quantity to account for the red earth, and that the colouring matter of this earth and of the rock into which it passes is vegetable and not mineral, as the examinations to which it has been submitted appear to determine. We would have ascertained the depth of this soil on the floor of the cave, but the annoyances from the causes alluded to rendered our stay there almost impracticable. We ascertained quite enough, however, to feel assured that in this soil, to which the bats have largely contributed and have coloured by some vegetable exuviæ,—in the multitude of dead and decomposing shells, for which this cave seemed the charnel-house, the tomb of millions,—in the gradual conversion of this mass to the state of solid rock, we saw the origin of those beds of shelly carbonate of lime which at first sight seemed almost inexplicable.

Admixture of Terrestrial, Marine, and Freshwater Shells.—The shores of the bay of Gibara supply to the geologist an instructive instance of this association of shells of different habits. There are several streams which empty themselves into this bay, and which in times of floods, during the rainy season, bring down an immense number of dead land-shells from the high lands of the interior. We here noticed extensive banks or deposits of terrestrial and marine shells, as on the mountain of *La Silla*; but the agency is reversed. In the one case we saw that the marine shells were conveyed by those active agents the soldier crabs even to the highest crest of *La Silla*. In the other the land shells have been brought down from the rocky recesses of *La Silla*, and are deposited in multitudes with those of the sea, on the margin of the estuary. Were any great geological catastrophe to occur, by which these accumulations would be buried, and be subjected to the examination in future times of some inquiring naturalist, he would see repeated here the phænomena which have been observed in more than one position in remote parts of the globe: with these he would find also a full proportion of freshwater shells, but of one genus and species only, the inhabitants of the Cuba streams, the *Neritina virginea* of Lamarck; of which great quantities are brought down by the freshes and deposited on the beach with the terrestrial and sea shells, and with the small oysters which cluster so thickly on the pendent branches of the mangroves of the creeks.

Coral Rock of different Ages on the Shores of the Island of Cuba.—In our examination of the rocks which approach the north coast of Cuba, we have seen nothing to countenance the hypothesis of a gradual transition from the crystalline white limestone we have described in this article, to the fragmentary coral rocks which appear on some parts of the coast, and thence to the modern reef of living corals which encompasses the Indian islands. Some such passage we have observed on the borders of the sea near Matansas, the Havana, and the Moro Castle; but the compact limestone of the first class is not in those positions, and a newer lithographic rock interposes.

On the west side of the Bay of Gibara the white limestone is observed, declining at first at an angle of 45°, and then decreasing to 20° towards the sea. Upon and near the base of this slope rest ancient beds of aggregate coral rock, reaching to about twenty feet above the present high-water line. This rock is indurated and externally honeycombed like the white compact limestone of the mountains.

Three or four miles to the west of the bay the shore is bordered with a reef of living corals, having an intermediate space of shoal water called the *Baxo*, nearly half a mile broad, between it and the beach. This shoal has been described in detail by Mr. R. C. Taylor in Loudon's Mag. of Nat. Hist., vol. ix. p. 449, &c.

High up on the beach may be seen a more ancient reef, forming a solid ledge of aggregate rock, for the most part composed of corals, shells, and coral sand or mud, now consolidated into a hard rock or cliff, some twenty or thirty feet high, against which the surf beats violently at high water. This is another proof of a change of level on this coast. The old reef, of which we spoke, after continuing for a mile or two as a cliff whose base is washed by the waves, passes obliquely inland, and now has a hill covered with a thick wood of wild fig, sea-grape, and a few aloes and palmettos between it and the sea. This rock is also honeycombed, its surface being full of holes and sharp points. In the mass, which consists of various madrepores and cabbage-formed corals of great size, we observed spines of Echini, and numerous univalve and bivalve shells, having their cavities wholly filled with indurated coral, sand, or mud; the whole forming a perfect illustration of the consolidation of an old rock containing organic remains, some of the oolites, for instance, the coral rag, or the Farringdon coral beds.

From this old reef we collected a series of characteristic specimens, all of which are common in the West Indian seas. One of the distinguishing characters of this old coral rock is that it is for the most part made up of a branching species, which appears to have existed at that time in great profusion, but which we have failed to discover living in the vicinity on the present reef. This ramose coral of the beach, when worn by the action of the waves, reminded me of forms which were nearly similar, which I remember to have seen at the base of the chalk at Hunstanton Cliff in Norfolk, and to which I made reference more than fourteen years ago in an article in the Philosophical Magazine, First Series, vol. lxi. p. 82.

Our notice of these different aged reefs would be incomplete did we stop here. We have remarked that there is a difference of level, amounting to full twenty feet, perhaps thirty, between the outer and the inner reef. Now as the coral insects do not live above high-water mark, and indeed are seldom seen living above the extreme low-water level, and commonly are several feet below it, it would appear that this ancient inner reef, which passes inland, was produced under different circumstances of relative elevation of the sea and land; and that either the sea has been depressed to a depth corresponding with the existing reefs of the coast, or, what is more probable, that the land has been elevated since the construction of the old reefs, not only here but at more distant points.

We have also to note that the Zoophytes differ decidedly in the two reefs, and therefore the circumstances which promoted the growth of genera and species in one case, were changed or were absent in the other.

Among the *Mollusca* also of the old reef the greater part, (although common to the seas of these latitudes, like the corals,) consist of different species to those now living in the vicinity of the recent reef, or that are thrown up on the present beach; and we looked in vain for some which exist in great profusion, and may be seen living at low water, at the base of the old coral reef.

We conceived that a further confirmation of the supposed change of level suggested itself in the prevalence of extensive fissures in the old coral bed, where it slopes towards the sea at low water. These cracks, which seem to imply displacement and disturbance, may be traced separately many hundred feet, commonly running parallel with the coast in an east and west direction.

Putting these facts together, one is led to conclude that other agents have been in action besides the mere erosion of the ocean waves, or an occasional and temporary elevation of its waters; and although we look to a much less remote date for these operations, we are perhaps not widely wrong in ascribing them to some such causes as have produced such remarkable changes of position in, and modified the composition of, every rock on this side of the island of Cuba. This influence, towards the west, has manifested itself by the injection of petroleum and bituminous matter, not only in large quantities into the fissures of the stratified rocks, but into the solid rocks themselves, and even filling cells in the veins of chalcedony by which they are traversed. In the quarter of which we have attempted to draw the foregoing sketch, this influence is seen in the partial vitrification of the serpentine and allied rocks; in the admixtnre or proximity of igneous rocks; in the conversion of masses of quartz into a species of porcelain; in the kneading into the most contorted and fantastic forms the old stratified beds; in the obliteration, to a great extent, of the planes of stratification of the limestone; in the destruction of the traces of organic remains which there is reason to conceive existed in that formation; and in the conversion of the whole series into a compact, unstratified, and apparently homogeneous mass.

Philadelphia, March 1837. R. C. T.

V. *On the Fossil Jaw of a gigantic Quadrumanous Animal allied to the genera Semnopithecus and Cynocephalus.* By *Lieuts.* W. E. Baker *and* H. M. Durand, *Engineers.*[*]

LYELL, when combating the inconclusive evidence advanced in support of the theory of the progressive development of organic life, notices the absence of remains of quadrumanous species in a fossil state, and the hypothesis which this circumstance has by some geologists been considered to countenance. He, however, draws attention to the fact, that the animals which are found in subaqueous deposits are in general such as frequent marshes, rivers, or the borders of lakes, and that such as live in trees are very rarely discovered; he adds, moreover, that considerable progress must be made in ascertaining the contemporary Pachydermata before it can be anticipated that skeletons of the quadrumanous tribes should occur. Considering the great number of relics assignable to the *Pachydermata*, *Ruminantia*, and *Feræ*, which the Sub-Himálayan field has produced, it is not therefore sur-

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[*] From the Journal of the Asiatic Society of Bengal, vol. v. p. 739 *et seq* This paper forms one of a series, by the same authors, on the Sub-Himálayan Fossil Remains of the Dádúpur collection. Dr. Falconer and Capt. Cautley's Memoir on the *Sivatherium giganteum*, another Sub-Himálayan Fossil, will be found in Lond. and Edinb. Phil. Mag. vol. ix. p. 193 *et seq.*

prising that at length the half jaw of a quadrumanous animal should be brought to light: the circumstance, however, being interesting in several respects, we have not deferred its communication until further research should put us in possession of more perfect specimens; the chances are against the probability of more being brought in for some time—in the interval it may be as well at once to add to the Sub-Himálayan list of fossils one species belonging to the order of the Quadrumana.

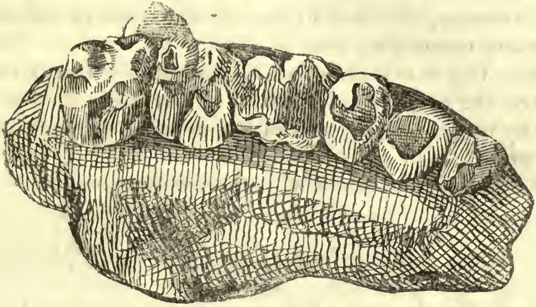
The specimen in question was found in the hills near to the Sutlej, and it appears from the attached matrix to have been derived from a stratum very similar in composition to the one described as occurring at the Maginund deposit. The fragment consists of the right half of an upper jaw; the molars as to number are complete; but the first has lost some of its exterior enamel: and the fifth has likewise had a portion of the enamel from its hind side chipped off. The second and third molars are a good deal worn, and the state of the fourth and fifth such as to indicate that the animal was perfectly adult. The canine is small, but much mutilated, its insertion into the jaw and its section being all that is distinct.

From the inspection of the molar teeth, the order to which the animal belonged is sufficiently evident; but there is enough of the orbit remaining to afford additional and very satisfactory proof; the lower part of the orbit and the start of the zygomatic arch being very distinct, would alone remove all doubt from the subject; the orbits of the Quadrumana being peculiar and not easily to be confounded with those of other animals.

On comparison with the delineations of the dentition of this order of animals given by F. Cuvier, the fossil bears some resemblance to the genus *Semnopithecus*; the section of the canine and the form and size of the false molars are very similar to the exemplar taken by F. Cuvier from a head of the species [*Semnop.*] *Maurus*, a species found in Java: had the drawing been taken from the [*S.*] *Entellus*, a species which inhabits India, the comparison would in this instance have been more satisfactory; the *Maurus* being chosen as the type, and no mention made of other difference except length of canines, the various species may be supposed to present no material departure from the type in form of molars. The third molar in the fossil is so much worn as not to admit of being compared with drawings from unworn teeth; the fourth is like that of the *Maurus*, but the fifth does not resemble the analogous molars of any of the existing species as represented by F. Cuvier, for the fossil tooth possesses a small interstitial point of enamel at the inner side, which does not appear to have place in any of those delineated. The incisors are absent, but the intermaxillary is clearly distinguishable.

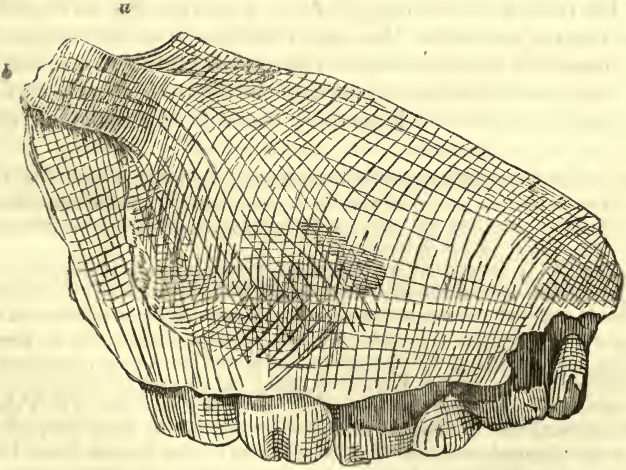
Were it not for the size of the canine and the fifth molar, the specimen presents some resemblance to the genus *Macacus*, given as the type of the genera *Macacus* and *Cynocephalus;* the smallness of the canine and the large size of the molars cause the fossil to approach more nearly to the *Semnopithecus* than to the *Macacus;* the difference is, however, great between the two, for the [*S.*] *Entellus* is said to attain the length of three and a half feet, whereas the length of the fossil animal, if

Fig. 1. *nat. dim.*



Quadrumana.

Fig. 2.



the space occupied by the molars and their size be deemed sufficient ground for a conjecture, must have been equal to that of the *Pithecus Satyrus*:—the space taken up by the molars is 2·15 inches. This circumstance, and the differences before pointed out, clearly separate the fossil from the species belonging to the genera *Cynocephalus* or *Semnopithecus*. The specimen is imperfect, but it indicates the existence of a gigantic species of quadrumanous animals contemporaneously with the *Pachydermata* of the Sub-Himálayas, and thus supplies what has hitherto been a desideratum in palæontology—proof of the existence, in a fossil state, of the type of organization most nearly resembling that of man.

NOTE.—Fig. 2 is a little foreshortened in order to show the bottom of the orbit at *a*, which in an accurate profile view is hidden by the ascending part of the orbit, the section of which is seen at *b*.

Both figures were taken with the camera lucida.

VI. *Chemical Examination of the Colouring Matter of the Green-sand Formation. By the late* EDWARD TURNER, *M.D., F.R.S.L. & E., Professor of Chemistry in University College, London.*[*]

THE colouring matter of green-sand sometimes appears in the rock of its ordinary green tint, and sometimes in grains of so deep a green that they seem black. The former generally occurs in sand, or where the sandstone is porous, and in this state an ochreous appearance is often observed, due to the green particles being partially decomposed, and their iron

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[*] From the Transactions of the Geological Society, vol. iv. part ii. p. 108. Dr. Fitton, in whose paper on the strata below the chalk the above communication by the late Dr. Turner is inserted, notices in the following terms the subject to which it relates:

"(10.) The green matter, which abounds in this stratum [the upper green sand] near Wissant, on the opposite coast of France, has been examined by M. Berthier [*]; who found it to consist principally of silica and protoxide of iron, with ten per cent. of potash. For the purpose of comparing the green-sands of different places and formations, my friend Dr. Turner, Professor of Chemistry in the London University, was good enough to examine some specimens from the upper and lower green-sands of Folkstone, of the Vale of Wardour, and the Boulonnois, and also particles of the same kind which abound in the sand and concretions beneath the Portland stone in the Boulonnois, and in England. I subjoin the result of this examination, whence it appears that in all these cases the green matter is of the same nature. A slight examination of the green particles, which Sir John Herschel had previously the goodness to make for me, intimated the same results."

"[*] Cuvier and Brongniart, *Environs de Paris*, 2nd edit. 1822, p. 249. See also *Annales des Mines*, iv. 1819, p. 623; and v. 1820, p. 197."

having passed into a higher state of oxidation; whereas the black-looking grains are met with in highly calcareous sandstone, where the texture is too firm to admit of the percolation of water. From either kind of rock the green matter may be obtained by washing with water and subsidence, since the colouring matter subsides less readily than grains of quartz, and more readily than calcareous and argillaceous substances. For the purpose of analysis it is best procured from those calcareous sandstones where the cement predominates, as in the neighbourhood of Hythe and Folkstone in Kent. On reducing such samples to powder, washing away the finer particles with pure water, and separating any adhering carbonates by dilute muriatic acid, the colouring matter is left, mixed only with small grains of quartz. It then always appears in the form of earthy particles of a deep green tint.

The green matter, when not previously weathered, is very feebly attacked by concentrated acids, even by the nitro-muriatic. It gives out water when heated, and becomes brown from its iron passing into the state of peroxide. As it has been supposed to owe its green colour to the presence of phosphoric acid, it was carefully examined, with the view of detecting that acid, if present. It was accordingly fused with carbonate of soda, the alkaline filtered solution neutralized by nitric acid, and evaporated to dryness, and the neutral solution tested by nitrate of silver and nitrate of lead. Of two samples of green-sand, thus examined, one was found to be quite free from phosphoric acid, and traces only were detected in the other. The former was also free from lime, and the latter contained but a small portion. It is hence obvious, that neither lime nor phosphoric acid are essential constituents of the colouring matter of green-sand, and their presence must be regarded as casual.

In order to determine the chemical constitution of the colouring matter, I collected some green particles from the calcareous sand of Eastware-bay, near Folkstone, removing all foreign matter as far as possible, by washing with water and dilute acid. The only impurity which I could detect after this treatment consisted of small grains of quartz, the quantity of which varied in different samples.

A portion of green particles thus purified, very free from oxidation, and dried at 212° Fahr., lost 7·0 per cent. of water when heated to redness.

Another portion of the same sample was fused with carbonate of soda and the earthy ingredients subsequently separated and weighed in the manner usual in such analyses.

A third portion was heated with carbonate of baryta, and examined for potash, traces of which were readily found. According to the total result, the green particles consist of

		[M. Berthier's analysis of the green particles from near Havre gave the following proportions:—	
Silica	48·5	Silica	50·0
Black oxide of iron	22·0	Protoxide of iron	21·0
Alumina	17·0	Alumina	7·0
Magnesia	3·8		
Water	7·0	Water	11·0
Potash	traces	Potash	10·0
	98·3		99·0.]

It is superfluous to speculate on the precise atomic constitution of the green particles, since they were not obtained in a state of perfect purity. The ingredients which appear to be essential, both from the quantity in which they occur, and their constancy in the colouring matter of green-sand from different localities, are silica, alumina, oxide of iron, magnesia, and water. I should hence consider the green matter as a hydrated silicate of alumina, magnesia, and black oxide of iron, and as being, in all probability, the true green earth, or earthy chlorite of mineralogists. The analyses of chlorite hitherto published are so discordant as to prove, either that different compounds have been examined under the same name, or that the specimens under examination were very impure. The essential ingredients, however, appear to have been the same as in the subject of my analysis.

Though the foregoing description applies more immediately to the colouring matter of the green-sand from the vicinity of Folkstone, I have obtained similar results on examining that from Hythe and several other places. Indeed, from the examination of many samples of green-sand collected by Dr. Fitton from various localities in England and France, I believe the colouring matter to be precisely the same in all.

VII. *Remarks on an Error of* M. Fourier *in his* Analyse des Equations. *By the Rev.* R. Murphy, *M.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE first part of M. Fourier's posthumous work on the analysis of determinate equations was published in 1831, the editor being M. Navier, of the Academy of Sciences of the

Institute: I am not aware that any succeeding part of this work has been published since that date.

The first part commences with a synoptic exposition of the whole work, and from this *Exposé*, which is rather diffuse, but clear in its language, and which contains the announcement of several theorems, the demonstrations of which are reserved for the succeeding parts, there can be no doubt that the author has committed some grave errors in the application of recurring series to the solution of numerical equations. M. Navier's attention may, through this medium, be directed to expunge or correct those parts of the unpublished manuscript referred to from p. 71 to 75 of the *Exposé*.

(1.) Let a recurring series A, B, C, D, E, F... be formed by the known method of Bernoulli and Euler, the successive quotients of which, viz. $\frac{B}{A}$, $\frac{C}{B}$, $\frac{D}{C}$, &c. converge to the first (when real, greatest) root of a proposed equation; from this let a second series be formed, of which the terms are $AD-BC$, $BE-CD$, $CF-DE$, &c.; the successive quotients of this series, says M. Fourier, converges towards the *sum* of the two first roots. This is incorrect, for when these quotients are convergent, they give the product instead of the sum of the two first roots.

To prove this, let α, β, γ, &c. be the roots of the proposed equation, α, β being the two first, that is, greatest, abstracting from sign, when real, and when conjugate imaginary quantities, such that $\alpha\beta > \gamma^2$, &c.

Let u_x represent the general term of the first recurring series, and v_x of the second, formed as above indicated, and let C, C′, C″, &c., c, c', &c. represent quantities invariable with x,

then $\quad v_x = u_x u_{x+3} - u_{x+1} u_{x+2}$

and $\quad u_x = C\alpha^x + C'\beta^x + C''\gamma^x +$, &c.

Hence $\quad v_x = c(\alpha\beta)^x + c'(\alpha\gamma)^x + c''(\beta\gamma)^x +$, &c.

$$= P_x(\alpha\beta)^x,$$

that is, making for abridgment

$$P_x = c + c'\left(\frac{\gamma}{\beta}\right)^x + c''.\left(\frac{\gamma}{\alpha}\right)^x +, \text{\&c.};$$

and as x increases it is clear that P_x converges to c, and therefore the limit of $\frac{P_{x+1}}{P_x}$ is unity, and consequently that of $\frac{b_{x+1}}{b_x}$ is $\alpha\beta$ and not $\alpha+\beta$.

Example. Given $x^2 - 2x - 3 = 0$; roots -1 and 3.

The first series is recurring; take 1, 2 as the first two terms and 2, 3 as the constants of relation.

First series 1, 2, 7, 20, 61, 182, &c.

Second derived series 6, −18, 54, −162, &c.

The successive quotients of the first series converge to the greatest root 3, and those of the second series are exactly the product −3 of the two roots, and not the sum which would be +2.

(2.) A, B, C, D, E, &c. being as above the primitive recurring series, if another be formed such as $AC-B^2$, $BD-C^2$, &c., the successive quotients converge to the product of the first two roots (p. 72.). This is correct, and may easily be proved as in the preceding case.

(3.) From the primitive series, Fourier adds, 3 other *recurring* series may be deduced by rules we have announced [in the MS. of sixth book]. The first will give by its successive converging quotients the sum of the 3 first roots, the second will determine the sum of their products two by two, and the third their continued product. (*ib.*)

The first two announcements here contained must be wrong; the third is most probably right; for if the successive quotients of a converging series gave $\alpha+\beta+\gamma$, its general term would be $v_x = C_1(\alpha+\beta+\gamma)^x + C_2(\alpha+\beta+\delta)^x +$, &c. Such a series *cannot* be derived in the above manner from $u_x = C\alpha^x + C'\beta^x + C''\gamma^x$, &c.; and if it could it would not give the three first roots, but the three roots of greatest sum, which may be very different; for the same reason $\alpha\beta+\alpha\gamma+\beta\gamma$ is not to be thus obtained from a recurring series deduced from the primitive, but $\alpha\beta\gamma$ may be easily so found, and the value belong to the three first roots, for their product must stand also first amongst the products, though their sum may not amongst the sums.

The knowledge of $\alpha+\beta$ as well as $\alpha\beta$ would, no doubt, give at once the real and the imaginary parts of a conjugate pair of impossible roots; but Fourier's method does not obtain the first, as has been shown, and therefore his observations (p. 74.), "et, ce qui est remarquable, on connaîtra pour chaque racine imaginaire, la partie réelle, de cette racine, et le coefficient de l'imaginaire. Voilà l'usage le plus étendu que l'on puisse faire de la méthode de séries récurrentes," &c.; and (p. 75.) "Les propriétés que nous venons d'énoncer sont *incomparablement* plus générales que celles qui ont été connues des inventeurs, et des auteurs qui ont traité depuis la même question," must be received with considerable deductions.

2, Bateman's Buildings, Soho Square,
London, May 20, 1837.

ROBERT MURPHY.

VIII. *Investigation of Formulæ for the Summation of certain Classes of Infinite Series.* *By* J. R. YOUNG, *Professor of Mathematics in Belfast College.*

[Continued from vol. x. p. 124, and concluded.]

LET us take A of the form

$$\frac{1}{n^{\kappa}(n+p)(n+2p)\ldots(n+mp)},$$

then, by the general relation referred to, we may substitute for it the expression

$$\frac{1}{mp}\left\{\frac{1}{n^{\kappa}(n+p)(n+2p)\ldots[n+(m-1)p]}\right.$$
$$\left.-\frac{1}{n^{\kappa-1}(n+p)(n+2p)\ldots(n+mp)}\right\}.$$

Now, by the same relation, we may for the first expression within the brackets substitute

$$\frac{1}{(m-1)p}\left\{\frac{1}{n^{\kappa}(n+p)(n+2p)\ldots[n+(m-2)p]}\right.$$
$$\left.-\frac{1}{n^{\kappa-1}(n+p)(n+2p)\ldots[n+(m-1)p]}\right\}$$

and for the first of these we may make a like substitution; so that by proceeding in this manner we shall at length arrive at

$$\frac{1}{2p}\left\{\frac{1}{n^{\kappa}(n+p)}-\frac{1}{n^{\kappa-1}(n+p)(n+2p)}\right\}$$

and finally at

$$\frac{1}{p}\left\{\frac{1}{n^{\kappa}}-\frac{1}{n^{\kappa-1}(n+p)}\right\},$$

whence a rule may be easily deduced for the summation of a series of the proposed form, provided certain subordinate series can be summed. As an illustration of this suppose $\kappa=2$, and put for abridgement

$$\frac{1}{n^2}+\frac{1}{(n+p)^2}+\frac{1}{(n+2p)^2}+\&c.=s$$

$$\frac{1}{n(n+p)}+\frac{1}{(n+p)(n+2p)}+\frac{1}{(n+2p)(n+3p)}+\&c.=S_1$$

$$\frac{1}{n(n+p)(n+2p)}+\frac{1}{(n+p)(n+2p)(n+3p)}+\&c.=S_2$$

$$\vdots$$

$$\frac{1}{n\,(n+p)\ldots(n+mp)}+\frac{1}{(n+p)\quad[n+(m+1)p]}+\&\text{c.}=\mathrm{S}_m,$$

then we have the following rule for the summation of the infinite series:

$$\frac{1}{n^2\,(n+p)\ldots\ldots(n+mp)}+\frac{1}{(n+p)^2\,(n+2p)\ldots[n+(m+1)p]}+\&\text{c.}$$

From *s* take S_1 and divide the result by p; from the quotient take S_2 and divide the result by $2p$; from the quotient take S_3 and divide the result by $3p$; and so on, till the divisor becomes $m\,p$, which will furnish a quotient equal to the sum of the proposed series.

As to the values of the subtractive quantities, S_1, S_2, S_3, &c., they are at once obtained from the fundamental relation with which we set out. Thus,

$$\begin{aligned}
\mathrm{S}_1 &= \frac{1}{p\,n}\\
\mathrm{S}_2 &= \frac{1}{2p\,n\,(n+p)}=\frac{\mathrm{S}_1}{2\,(n+p)}\\
\mathrm{S}_3 &= \frac{1}{3p\,n\,(n+p)\,(n+2p)}=\frac{2\,\mathrm{S}_2}{3\,(n+2p)}\\
&\vdots\\
\mathrm{S}_m &= \frac{(m-1)\,\mathrm{S}_{m-1}}{m\,[n+(m-1)\,p]},
\end{aligned}$$

As an example let it be required to find the sum of the infinite series

$$\frac{1}{1^2\,.\,2\,.\,3\,.\,4}+\frac{1}{2^2\,.\,3\,.\,4\,.\,5}+\frac{1}{3^2\,.\,4\,.\,5\,.\,6}+\&\text{c.}$$

in which $n=1$, $p=1$ and $m=3$.

The values of S_1, S_2, S_3, are, in this case,

$$\mathrm{S}_1=1,\quad \mathrm{S}_2=\frac{1}{4},\quad \mathrm{S}_3=\frac{1}{18},$$

hence, arranging these in a row and prefixing the subtractive sign, the operation by the rule will be as follows:

$$\begin{array}{llll}
-1 & -\frac{1}{4} & -\frac{1}{18} & \\
\frac{\pi^2}{6} & \frac{\pi^2}{6}-1 & \frac{\pi^2}{12}-\frac{5}{8} & \frac{\pi^2}{36}-\frac{49}{216}=\text{Sum.}\\
\hline
\frac{\pi^2}{6}-1 & \frac{\pi^2}{6}-\frac{5}{4} & \frac{\pi^2}{12}-\frac{49}{72} &
\end{array}$$

If the series to be summed were

$$\frac{1}{1^2 . 3 . 5 . 7} + \frac{1}{3^2 . 5 . 7 . 9} + \&c.,$$

in which $p = 2$, the process would be this, viz.

$$\begin{array}{llll}
-\frac{1}{2} & -\frac{1}{12} & -\frac{1}{90} & \\
\frac{\pi^2}{8} & \frac{\pi^2}{16} - \frac{1}{4} & \frac{\pi^2}{64} - \frac{1}{12} & \frac{\pi^2}{384} - \frac{17}{1080} = \text{Sum.} \\
\hline
\frac{\pi^2}{8} - \frac{1}{2} & \frac{\pi^2}{16} - \frac{1}{3} & \frac{\pi^2}{64} - \frac{17}{180} &
\end{array}$$

It would be easy, by imitating the steps of the preceding investigation, to deduce a rule for the summation of the infinite series

$$\frac{1}{n(n+p) \ldots (n+mp)^2} + \frac{1}{(n+p) \ldots [n+(m+1)p]^2} + \&c.$$

This rule would differ from the foregoing in the following particulars, viz. instead of S_1, S_2, S_3, &c. we shall have to employ

$$S_1 + \frac{1}{n^2}, \quad S_2 + \frac{1}{n(n+p)^2}, \quad S_3 + \frac{1}{n(n+p)(n+2p)^2}, \quad \&c.,$$

and these, instead of being subtracted as before from the several quantities placed under them, must themselves be diminished by those quantities.

It is clear that when n and p are each unity, as in the first example above, the values which here supply the place of S_1, S_2, &c. will be the doubles of these quantities.

As an example, let the series

$$\frac{1}{1 . 2 . 3 . 4^2} + \frac{1}{2 . 3 . 4 . 5^2} +, \&c.$$

be proposed, in which $n = 1$, $p = 1$, $m = 3$,

$$\begin{array}{llll}
2 & \frac{1}{2} & \frac{1}{9} & \\
\frac{\pi^2}{6} & 2 - \frac{\pi^2}{6} & \frac{\pi^2}{12} - \frac{3}{4} & \frac{31}{108} - \frac{\pi^2}{36} = \text{Sum.} \\
\hline
2 - \frac{\pi^2}{6} & \frac{\pi^2}{6} - \frac{3}{2} & \frac{31}{36} - \frac{\pi^2}{12} &
\end{array}$$

By comparing the several steps of this process with those in the working of the first example, we are led to conclude that the aggregate of two series, like this last and that of the first example, will always be accurately expressed in a finite fraction, provided the factors in the denominator be even in number, and that the difference of the two series will also be a finite fraction, if the number of these factors be odd. In all other cases the sum and difference will involve π^2. Thus the difference between the two series

$$\frac{1}{1^2 . 2 . 3} + \frac{1}{2^2 . 3 . 4} + \frac{1}{3^2 . 4 . 5} + \&c. = \frac{\pi^2}{12} - \frac{5}{8}$$

and

$$\frac{1}{1 . 2 . 3^2} + \frac{1}{2 . 3 . 4^2} + \frac{1}{3 . 4 . 5^2} + \&c. = \frac{\pi^2}{12} - \frac{3}{4}$$

is $\frac{1}{8}$; so that actually subtracting and dividing by 2, we have

$$\frac{1}{1^2 . 2 . 3^2} + \frac{1}{2^2 . 3 . 4^2} + \frac{1}{3^2 . 4 . 5^2} + \&c. = \frac{1}{16};$$

this series is therefore the square of the series

$$\frac{1}{1 . 2 . 3} + \frac{1}{2 . 3 . 4} + \frac{1}{3 . 4 . 5} + \&c.$$

It thus appears that every series of the form

$$\frac{1}{1^2 . 2 . 3 \ldots (m-1)\, m^2} + \frac{1}{2^2 . 3 . 4 \ldots m\, (m+1)^2} + \&c.$$

is accurately summable when m is odd, but not when m is even.

Similar remarks obviously apply to the series

$$\frac{1}{1^2 . 3 . 5 \ldots (m-2)\, m^2} + \frac{1}{3^2 . 5 . 7 \ldots m\, (m+2)^2} + \&c.,$$

the sum of which is a determinable fraction only when the denominator of each term consists of an odd number of factors. In other cases the sum involves π^2.

Although in the foregoing illustrations the several terms of each series are all connected together by the sign *plus*, yet a glance at the general investigation will serve to show that the several deductions hold when the terms are alternately *plus* and *minus*.

IX. *On the Right Rhombic Baryto-Calcite, with reference to Prof.* Johnston's *Paper in the Phil. Mag. for May* 1837. *By* THOMAS THOMSON, *M.D., F.R.S. L. & E., Regius Professor of Chemistry, Glasgow.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE Number of the Philosophical Magazine for May, which I received yesterday, contains a paper by Mr. Johnston of Durham, entitled "On the composition of the right rhombic Baryto-Calcite, the Bicalcareo-carbonate of Baryta of Dr. Thomson." This paper makes it necessary for me to send you a few observations, and to request their insertion in the next Number of the Philosophical Magazine.

The mineral which constitutes the subject of Mr. Johnston's paper came into my hands in the year 1834, being in a collection exposed here for sale by a mineral dealer from Alston. It was new to me; I therefore analysed it, and concluded from my experiments that it was a compound of 1 atom of carbonate of barytes and 2 atoms of carbonate of lime. I was not aware when I published an account of this mineral in May 1835 that Mr. Johnston had some months before noticed it in a paper containing some ingenious speculations about isomorphism (L. & E. Phil. Mag., vol. vi. p. 1.), till my attention was called to his paper by my nephew, Dr. R. D. Thomson of London. I had read the paper before my System of Mineralogy went to press. But as the result of Mr. Johnston's analysis was different from mine, and as my faith in the theory of isomorphism and dimorphism was not very strong, I thought the best thing I could do was not to refer to Mr. Johnston at all, as I had not complete evidence that his mineral was the same as mine.

Last winter I received a letter from Mr. Johnston informing me that he had repeated his analysis and found it correct, and requesting me to repeat mine. At that period of our session my time was so fully occupied with college business that it was impossible to pay the requisite attention to the necessary steps of an analysis. I therefore wrote him that at present I could not spare time for experimenting, but that I would make a point of repeating the analysis as soon as our session was over.

Accordingly, when the month of May arrived, I immediately set about it. While engaged in it I was informed by a friend that a paper by Mr. Johnston had been read to the Royal Society of Edinburgh, in which he maintained the accuracy of his own analysis and the inaccuracy of mine. I presume the paper in the Philosophical Magazine for May (vol. x. p. 373.) is the same that had been previously read in Edinburgh.

The analysis of a double carbonate of lime and barytes is very easy. Mine was made during the winter session, in a laboratory where a number of practical students were constantly engaged in experimenting. It was possible therefore, as I stated to Mr. Johnston, that some accident vitiating my results might have taken place without my knowledge. I determined in the first place the constituents of the mineral, and then ascertained the quantity of carbonate of barytes from the nitrate left when the dry salt was digested in alcohol. Here the analysis stopped. For I was not able for more than a fortnight to analyse the nitrate of lime, and it was overturned accidentally by one of the students. But from the known weight of the mineral dissolved and the quantity of nitrate of barytes obtained I inferred the composition of the mineral.

I have now finished three successive analyses of this mineral. For the greater security they were made in a separate room which my practical students seldom entered, and I watched over all the steps of the analysis in person. I shall here state the steps of the process, that Mr. Johnston may repeat it if he should feel inclined.

The first analysis was made upon 22 grains, the second on 20·1 grains, and the third on 15·8 grains of the mineral. It will be sufficient if I give a detail of the third analysis, because I consider it as the most accurate.

1. The 15·8 grains of the mineral were put into a flask along with dilute nitric acid, and digested on the sand-bath. They dissolved with the exception of 0·33 grain of sulphate of barytes. On testing the nitric acid employed I found in it a sensible quantity of sulphuric acid. Hence I conclude that this sulphuric acid was the cause of the appearance of the present sulphate. In the first analysis the sulphate of barytes undissolved weighed 0·2 grain, and in the second only 0·08 grain. These differences were doubtless connected with the quantity of nitric acid employed to dissolve the mineral.

2. The nitric acid solution was filtered, evaporated to dryness, and redissolved in water. The solution was opal-coloured, and deposited when left at rest 0·27 grain of a white matter, which proved when examined by the blowpipe to be sulphate of barytes. This, added to the preceding quantity, makes 0·6 grain of sulphate of barytes, equivalent to 0·5 grain of carbonate of barytes.

3. The aqueous solution of the nitrates was evaporated to dryness, and the residual salt being put into a flask was digested in a quantity of alcohol of the specific gravity 0·800.

The flask was corked, the alcohol was heated to the boiling point, and then left to digest for some hours on the salt. It was then drawn off by a sucker, an additional portion of the same alcohol was poured on the undissolved salt, and the digestion continued as before. A third portion of the same alcohol was finally added and the whole was thrown upon a filter to collect the undissolved nitrate of barytes. After being dried in a heat of 300° this nitrate weighed 12·05 grains, equivalent to 9·08 grains of carbonate of barytes.

4. All the alcoholic liquids were collected together in a small retort, and the alcohol distilled off till only a few drops of liquid remained in the retort. It was taken out of the retort, evaporated to dryness, and digested again in alcohol of 0·800. The whole dissolved with the exception of a dirty yellowish black powder, which was separated, and weighed after ignition 0·3 grain. When examined before the blowpipe it proved to be oxide of manganese, not quite free from iron.

5. The alcoholic solution was distilled as before. It was then evaporated to dryness, and the salt that remained heated in a platinum crucible till the nitric acid was decomposed. The residue was digested in weak nitric acid and the undissolved portion collected on a filter, ignited, and weighed. It amounted to 0·67 grain, and was red oxide of manganese, not quite free from iron. This with the preceding 0·3 grain makes 0·97 red oxide of manganese, equivalent to 1·45 grain of carbonate of manganese.

In my first analysis I separated the manganese by caustic ammonia, but I could not in this way free the nitrate of lime from all traces of manganese. In the second analysis I employed with the same object sulphohydrate of ammonia; but this method only succeeded imperfectly. Even the process employed in the third analysis did not render the lime quite colourless, though the manganese and iron remaining were very small in quantity.

6. The nitrate of lime was converted into sulphate, ignited and weighed.

The results of the analysis are

Carbonate of barytes	9·58	or 60·63
Carbonate of lime	4·77	or 30·19
Carbonate of manganese.........	1·45	or 9·18
	15·80	100·00

These numbers are equivalent to

3·92 atoms carbonate of barytes,
3·83 atoms carbonate of lime,
1 atom carbonate of manganese, with a little carbonate of iron.

Doubtless the true composition is

4 atoms carbonate of barytes,
4 atoms carbonate of lime,
1 atom carbonate of manganese and iron.

Thus it appears that neither Mr. Johnston nor myself had given a correct analysis of the mineral. It is a triple instead of a double salt. I failed in discovering the manganese, in consequence of having neglected to examine the calcareous residue: Mr. Johnston failed in consequence of dissolving the mineral in muriatic instead of nitric acid. This last acid should always be used in examining the earthy carbonates, because it enables us to see by the colour when any iron or manganese is present.

A name derived from the constituents of so complicated a mineral would be unwieldy. Perhaps the term *bromlite*, derived from its best known locality, would be as unexceptionable as any. I am, &c.,

THOMAS THOMSON.

Glasgow, June 3, 1837.

X. *Remarks on the present State of Botanical Classification.*

By Sir EDW. FF. BROMHEAD, *Bart., M.A., F.R.S.L.&E.*[*]

IT seems to be agreed among botanists that the natural families must be arranged upon some new system; and it is also understood that the first attempt must consist in forming natural alliances or assemblages of such as are in immediate undisputed affinity with each other. Each family must be related to every other family within the alliance, and I have elsewhere explained the artifice by which a first approximation may be made.

The question of linear arrangement is not necessarily connected with this stage of the inquiry; if such be the order of nature, the primary alliances will throw themselves into sequence almost spontaneously; if not, some other principle will shortly show itself. In the mean time, to give the future result fair play, we should endeavour to place the families within our alliances in their natural order of transition, as we endeavour to arrange genera within families and species within genera. The tendency to circulation will no doubt always offer difficulties; it would almost seem as if the progress of development were represented by a thread wound spirally round a rod, on which the point immediately above another appears

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[*] Communicated by the Author

to be the nearest, though distant by a whole round of the spiral.

The extent of these alliances does not yet rest upon any fixed principle. Linnæus and Adanson throw plants into fifty-eight assemblages, and we must consider a botanical family of that day as representing the alliance of our own time. Bartling makes sixty classes or alliances. The annexed table makes sixty alliances also, averaging each about five families. Those of Dr. Lindley are more numerous, averaging rather less than three families; he seems to have looked for consolidation in his groups, and precision in the alliances. The alliances of the table sometimes correspond with Dr. Lindley's groups, as in *Curvembryosæ*, *Aggregosæ*, *Glumosæ*, *Spadicosæ*, but they more generally correspond with his alliances; the smaller alliances of Dr. Lindley must however continue under any scientific classification as suballiances.

In the revised table, the improved nomenclature of Dr. Lindley is of course usually adopted; his work on the Natural System must henceforth be a standard of reference everywhere; and there is much convenience in the definite manner by which such reference points out the tribes and genera intended to be included. His happy manner of designating the ALLIANCES by a termination in *ales* is also adopted. In amending the nomenclature, it were to be further wished that botanists would regularly designate *all* the families from generic names, throwing aside names from species or obsolete genera, and all characteristic or arbitrary or mutilated names. Such a regular system might, by a slight modification of the termination, lead hereafter to a convenient mode of indicating the position of each family in the general system.

The limits of families are often at present more a matter of taste than of definite distinction: Arnott has in doubtful cases made good use of suborders as superior to tribes and sections. The average points to five families for each alliance, and it may be as well to lean towards that number; nothing would be more easy than to reduce the whole to that standard; we sometimes have the choice of more than one combination, or sometimes a more easy adaptation to a ternary or senary division. Numerical symmetry is, however, a suspicious circumstance in natural history, though it is possible that an antecedent, normal, and succeeding state may occasionally be distinctly marked.

In the first approximations for forming the table, the characters were studiously kept out of sight. Every one must feel, on trial, the bias against evidence, where it is required to modify a scheme for the admission of something additional, and also the tendency to cut off families at the limits of an alliance for the sake of rounding the character. After the first grouping the investigation of characters will, of course, be found useful; it will settle the limits of an alliance, where a family has stood ambiguously between two; it will negatively settle the place of many doubtful families; it will shake heterogeneous alliances, and confirm such as are sound.

The characters must be formed on the *whole structure;* it may in a few instances be possible to point out an assemblage of families by some happy peculiarity, but such is not the plan of nature, in which even species require an enumeration of many parts. The utmost extent of simplification will be a statement of the NORMAL STRUCTURE (the Nixus of Dr. Lindley), followed by remarks on the limits of deviation and equivalent changes. The error in this case has arisen from the received notion, that every species passes imperceptibly into some other; such, however, may not be the case,—a change of one particular part generally implies, in natural history, some corresponding change of every other part, so that families may differ more abruptly than genera, and alliances more abruptly than families. In some families the differences of adjoining species are much greater than in others.

A collection of characters is necessary also for ascertaining the *nature* of the properties which usually extend over a succession of families; the properties which most conveniently distinguish genera are often of little value to distinguish families, and those which separate adjoining families will probably have little further range. In this research it is mortifying to find a property running through the greater part of an alliance, and often passed by in the remainder as being unimportant within the usual limits of classification. The characters may too frequently amount to nothing, or indicate merely that there is not a deviation from general normal structure; but such were some of the characters of Jussieu. They may often be negative, but such are more definite than any other, and may hereafter be pushed to any extent, so as to form differential characters between any two alliances whatsoever.

Results apparently trivial should be admitted, where they offer themselves, as it is impossible to foresee what may serve to illustrate the general progress of structure. Where a property occurs through an alliance, with comparatively trifling exception, it should be given, and the exception stated; the place of the excepted family may be shaken, or some remarkable equivalence of structure may be discovered.

Bartling very properly gives the lead to the structure of the stem, foliation, and inflorescence, as Linnæus does in the elegant sketches of his Botanical Philosophy. Those properties are not often used in distinguishing genera, and are comparatively neglected by many late writers; they will probably hereafter rise to importance, and it may be found useful to refer to the older writers, who laid so much stress on foliation.

In giving the characters it is necessary to use a modified language of a greater breadth of expression than usual.

I have collected some materials for ascertaining the founders of the different alliances in the table; and the first publisher must be so considered, even were it thought to be the result of an indistinct happy accident. Few writers can take the range of a whole science and instinctively select from the mass whatever is well founded. Writers may see the truth placed before them, but seeing it mixed up with objectionable matter, they may throw it aside, and after long inquiry and many qualifications, may find on reference the final result of their labours to be the same. Science herein differs from literature, that the truth *must* bring two writers to the same point; but it is also true that we may let the result of our reading digest with the mass of our ideas, until we mistake for the produce of our own minds what we met with elsewhere. Frequently, by a kind of compulsion, after long research the views of others are adopted. The annexed table has thus been most materially improved, and was in some parts new cast, from the many new affinities and assemblages discovered by the acuteness of Bartling and Lindley.

Of the 60 alliances in the table, (in the next page) it is highly satisfactory to find that nearly 50 have been substantially indicated by the first names which botany can produce:—Agardh, Bartling, Brown, Cæsalpinus, Decandolle, Hedwig, A. L. de Jussieu, Ad. de Jussieu, Lindley, Linnæus, Morison, Ray, Reichenbach, Richard, Rudolphi, Schultes, St. Hilaire, Wallenbergh. The particulars of their discoveries, and the characters of the botanical formations and alliances which I have prepared, would extend these remarks much too far.

[See the "*Intelligence* and *Miscellaneous Articles*" of the present number for some remarks on the introductory passages of the foregoing paper, prepared for insertion as a note, but for which there is not room in this place. Edit.]

THE ULVACEOUS RACE*.

[Fucaceae,[1] chondraceae,[1] ULVACEAE,[1] nostocaceae,[1] diatomaceae,]

(†) Confervaceae,[1] CHARACEAE, equisetaceae, (sigillariaceae,)
Ophioglossaceae, danæaceae-OSMUNDACEAE-gleicheniaceae, polypodiaceae,
Cycadaceae, EPHEDRACEÆ, casuarinaceae,

† Myricaceæ, platanaceae-(moraceæ,[2]) ULMACEÆ-(fothergilleae,) (empetraceae,) stilaginaceae, scepaceæ,
Trewiaceæ, hensloviaceæ-batideæ, (†)URTICACEAE, cannabineae, datiscaceae,
Lacistemaceae, chloranthaceae, garryaceae, (nageiaceæ[2])-PIPERACEAE-saururaceae, podostemaceae,

Ceratophylleae, hippurideae,[3] callitrichaceae, HALORAGEAE, trapaceæ,
Circæeae-†ŒNOTHERACEAE-montinieae, [†lythraceæ, (†)rhizophoraceae, vochyaceae, †combretaceae,
Memecylaceae, melastomaceae, alangiaceae, lecythidaceae-barringtonieæ,] †MYRTACEÆ-puniceae,

Pyraceae, amygdaleae, †chrysobalanaceae-†sanguisorbeae, (neillieae[4])-quillaiae-spiræae, (†)ROSACEÆ-†potentilleæ,
(†) SAXIFRAGACEÆ-cunoniaceae-baueraceae, aristoteleæ-philadelphaceae, (†galacineae,[4]) escalloniaceae, bruniaceae,
Ribesiaceae, [†melocactaceae, CUCURBITACEAE, begoniaceae, loasaceae,]
[Fouquieraceae, crassulaceae, mesembryanthaceæ, PORTULACACEAE, vivianieæ,[7]-†silenaceæ-†alsinaceae,]

(†) Illecebraceæ, †CHENOPODIACEAE,[9] †phytolaccaceae,[9] †polygonaceae, mirabiliaceae-salvadoraceæ,

Staticeae-plumbaginaceae, POLEMONIACEAE, cobæaceae, diapensiaceæ, hydroleaceae,
Hydrophyllaceae, BORAGINACEAE, heliotropiceae, ehretiaceae, cordiaceae,
Nolanaceae, [(†)verbasceae-(†)digitaleæ-salpiglossideae, SOLANACEAE-(cestraceæ,)] convolvulaceae, cuscutaceae,

Leiphaimeæ,[2] [(retziaceæ,) menyantheae, †GENTIANACEAE, spigeliaceae,
(†)APOCYNACEÆ, asclepiadaceae, carisseæ-rauwolfieæ,] potaliaceae-loganiaceae, lygodysodeaceae,
†Cinchonaceae, opercularineae, GALIACEÆ, lonicereae-sambuceae, apiaceæ,

(†) Araliaceae, loranthaceae, (†)hamamelaceae, CORNACEAE-hederaceae,[6] †vitaceae,
GERANIACEÆ, surianaceæ, limnanthaceae, tropæoleae-balsaminaceae, †oxalidaceae,
[Reaumuriaceae-(tamaricaceae,) elatinaceae-linaceae, CISTACEAE,] resedaceæ, polygalaceæ-krameriaceæ,
Tremandraceæ, cappareae-cleomeae, BRASSICACEAE, fumarieae, papaveraceae,
[NYMPHÆACEAE, nelumbiaceae, (†)cephalotaceae, hydropeltideae-podophylleae, pæonieae,[7]
Cimicifugeæ[7]-clematideae-†ranunculaceae, SARRACENIACEAE,] aristolochiaceae, nepenthaceae, (†cytinaceae,[9])

(†) Pistiaceae, †hydrocharaceae, ALISMACEAE, butomaceae, pontederaceae,
Commelinaceae-philydraceae, xyridaceae-restiaceae-desvauxiaceae, cyperaceae, AVENACEÆ, cocoaceae,
Cyclanthaceae, pandanaceae-TYPHACEAE, (†)acoraceae-araceae-ambrosinieæ, triglochinaceae, †naiadaceae,

* The symbols and references are explained in p. 54.

THE USNEACEOUS RACE.

††Fungi,...(Byssaceæ,) [calyciaceæ,[2] graphidaceæ,[2] USNEACEAE,[2]] endocarpaceæ,[2]

Riccieae-marchantieæ, JUNGERMANNIACEAE, andræaceae-bryaceæ,
Salviniaceae-marsileaceae, isoëteae[6]-†LYCOPODIACEAE, lepidodendreæ,[2]
(Salisburiaceae)-†taxaceae, CUPRESSINAE, †pinaceæ-araucarieæ,

Liquidambraceae, salicaceae, BETULACEAE, carpineae[2]-†corylaceae, juglandaceae,
Anacardiaceae-spondiaceae, burseraceae, chailletiaceae, nitrariaceae-(neuradeae,) RHAMNACEAE,
Coriariaceae, (†)EUPHORBIACEAE, stackhousiaceae, celastraceae-staphyleaceae-†hippocrateae, erythroxyleae,

Malpighiaceae, (†)aceraceae, ÆSCULACEÆ, millingtonieæ-sapindaceae, caryocaraceae,
†Clusiaceæ, marcgraaviaceae, HYPERICACEAE, (ochranthaceæ)-carpodonteae, †camelliaceae,
Rhodolænaceae, humiriaceae, (hugoniaceæ)-(canelleæ,) meliaceae-cedrelaceae, LIMONIACEAE,
Amyridaceae, †connaraceae, mimoseae-detarieae, swartzieae-†FABACEAE, geoffroyeae-cæsalpinieae,
Moringaceae, [wormskioldiæ[2]-†frankeniaceae-sauvagesiae, (†)parnassieae,[7] droseraceae, †VIOLACEAE,
†PASSIFLORACEAE-malesherbiaceae, turneraceae, caricaceae, belvisiaceae, patrisieae-†flacourtiaceæ,
†Bixaceae, pangiaceae, samydaceae, HOMALIACEÆ,] aquilariaceae,

†Daphnaceæ-penæaceae, †proteaceae, ELAEAGNACEAE, nyssaceae[5]-†santalaceae-anthoboleæ, olacaceae,

Oleaceae-jasminaceae, columelliaceae-gesneraceae, pinguiculaceæ, ACANTHACEAE, cyrtandraceæ-bignoniaceae-pedaliaceae,
Stilbaceæ, selaginaceae, myoporaceae, †verbenaceae, LAMIACEAE-ocimoideae,
Buddleieæ-buchnereæ, †gratioleæ, (†veroniceæ,) hemimerideæ, antirrhineae-gerardieae-†RHINANTHEAE, orobanchaceae,
Monotropaceae-(†)pyrolaceæ, †ERICACEAE, epacridaceae, andromedeæ, vacciniaceae,
CAMPANULACEAE-lobeliaceae, stylidiaceae, pongatiaceæ, goodeniaceae-scaevolaceae, brunoniaceae,
Asteraceæ-cichoraceæ-mutisiaceæ-CYNARACEÆ,[9] valerianaceae, calyceraceae, dipsacaceae-globulariaceae, †plantaginaceae,

†Primulaceae-†MYRSINACEAE, achrasaceae, (†)styraceæ-diospyraceae, ilicaceæ-brexiaceae, pittosporaceae,
†Zygophyllaceae, †RUTACEÆ, †xanthoxylaceae, simarubaceae, ochnaceae,
Dipterocarpaceae, elaeocarpaceae-(†)tiliaceae, byttnerieæ-dombeyae-bombaceae, MALVACEAE, (†)sterculiaceae,
Myristicaceae, hernandiaceae, illigeraceae, cassythaceae, LAURACEAE,
Atherospermaceae, monimiaceae, calycanthaceae, illicieae-MAGNOLIACEÆ, †dilleniaceae,
Schizandreae, †anonaceae, (†)berberaceae, lardizabaleæ, MENISPERMACEAE, * * *

Smilaceae-dioscoreaceae-†roxburghiaceae, †ASPARAGACEÆ,[9] (†)juncaceae-gilliesiaceae, †hemerocallaceæ,[9] (†)melanthiaceæ,
Iridaceae, apostasiaceae, ORCHIDACEÆ-vanillaceae, zingiberaceæ-marantaceae-musaceae, †amaryllidaceae,
Hypoxideae-agaveæ, †BROMELIACEAE, wachendorfieae[7]-†hæmodoraceae, burmanniaceae, taccaceae,...†Balanophoraccae,[9]

MEMORANDA.

A series of Families in immediate and continuous affinity with each other, is called an ALLIANCE, and is indicated by a termination in *ales*:—Ex. Osmundales are the Ferns.

Parallel Alliances are called FORMATIONS*, and are indicated by a termination *osæ*:—Ex. Lamiosæ include Lamiales and Boraginales, the Nucamentosae of Dr. Lindley.

Series of successive alliances are called UNIONS:—Ex. The Passifloral Union includes Violales, Passiflorales, and Homaliales, nearly the Parietosae of Dr. Lindley.

[ ] Indicates that the order of succession among the families so included is not settled.

( ) Indicates that the evidence for the station is more conflicting than usual.

† Indicates that the Family or Tribe may be compound. Tribes are occasionally inserted to show transitions.

The Nomenclature of Dr. Lindley's System is usually taken as the standard.

(1) Agardh's Fucoideæ, florideæ, ulvaceæ, nostochinæ, diatomeæ, confervoideæ.
(2) Here separated.
(3) Link.
(4) D. Don.
(5) Jussieu.
(6) Bartling.
(7) Arnott.
(8)
(9) Chenopodiaceae here includes Scleranthaceae-amarantaceæ-†chenopodiaceæ.
Phytolaccaceæ includes Tetragoniaceae-phytolaccaceæ-petiveriaceæ.
Cytinaceae includes Rafflesiaceæ.
Cynaraceæ—followed by (Xeranthemeæ)-calendulaceæ-arctotideæ.
Asparagaceae includes Convallarinæ-parideæ-asparageæ-aloinæ-anthericeæ.
Hemerocallaceae includes †Scilleæ-hemerocallideæ-tulipeæ.
Balanophoraceae includes Cynomoriaceæ.

XI. *Observations on the Deduction of the Dew-point from the Indications of the Wet-bulb Thermometer, and on the Detection of minute quantities of foreign Matters diffused in the Atmosphere; with Notices of Apparatus. By* JOHN PRIDEAUX, *member of the Plymouth Institution; in a letter to* Mr. Brayley.

DEAR SIR,

THE wet-bulb thermometer, preferable to other hygrometers from the permanence, directness, and simplicity of its action, has the inconvenience of its indications being not only subject to necessary calculations, but also, hitherto, to considerable discrepancy in the principles on which these computations are made; and to corresponding differences in the resulting dew-points, even supported by irreconcileable experiments. The pages of the Philosophical Magazine have been so largely occupied in the discussion of these principles, that it is not my intention to go much into them on the present occasion; on which I will offer little more than a comparison between Dr. Apjohn's table in vol. vii. p. 471, with that of Dr. Mason in *Thomson's Records*, vol. iv. p. 109.

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* A term used by Reichenbach.

In Dr. Apjohn's I omit the barometrical column, which Dr. Mason finds to be needless (p. 101), and that of the calculated dew-points, in which the error is generally on one side, occasioned, as the author supposes, by an error in one of the elements; and I substitute, for the sake of comparison with Mason's, a column (5) of differences between the dew-point and dry bulb thermometer; having for the same purpose changed the order of arrangement to that of these differences.

In Mason's table such numbers only are taken as come into comparison with Apjohn's; and to facilitate this comparison, three columns (6, 7, 8) interpolated, illustrating his method of obtaining the dew-point, viz. doubling the difference between the wet and dry bulb (col. 8), + a correction for wind (col. 7), and deducting the product (col. 10), from the indication of the dry-bulb thermometer.

APJOHN.				Added Columns.				MASON.	
Dry-bulb Thermometer.	Wet-bulb Thermometer.	Difference, Wet and Dry-bulb.	Dew-point.	Difference, Dew-point and Dry-bulb.	Correction for Wind, subtracted from col. 8.	Correction for Current of Wind.	Double Difference, Wet and Dry-bulb.	Difference, Wet and Dry-bulb.	Difference, Dew-point and Dry-bulb
1	2	3	4	5	6	7	8	9	10
68·	60 3	7·7	55·	13·	12·5	2·5	15·	7·5	17·5
71·3	63·	8·3	57·5	13·8	13·33	2·67	16·	8·	18·67
81·	62·2	8·8	56·5	14·5	14·17	2·83	17·	8·5	19·83
72·	62·	10·	55·1	16·9	16·67	3·33	20·	10·	23·33
69·	58·9	10·1	51·5	17·5					
69·	58·6	10·4	51·3	17 7	17·5	3·5	21·	10·5	24·5
71·7	60·	11·7	51 2	20·5	19·17	3·83	23·	11·5	26·83
72·	60·	12·	51 3	20·7	20·	4·	24·	12·	28·
77·	65·	12·	57 2	19·8					
75·2	63·2	12·	55·	20 2					
73·	60·3	12·7	51·3	21·7	20 83	4·17	25·	12·5	29 17
76·	63·3	12·7	54·7	21·3					
77·5	64·5	13·	56·3	21·2	21·67	4·33	26·	13·	30 33
76·	61·5	14·5	51·3	24·7	24·17	4·83	29·	14·5	33·83
78·	62·2	15·8	51·3	26 7	26·67	5·33	32·	16·	37·33
79·	62·	16 4	50·9	28 1					
83·	66·5	16·5	56 8	26·2	27·5	5·5	33·	16·5	38·5
83·	65·8	17·2	54·5	28·5	28·33	5·67	34·	17·	39·67
84·6	67·	17·6	56·	28·6	29·17	5·83	35·	17·5	40·83
82·2	64 3	17·9	50 9	31·3					
92·	69·	23·	54 1	37·9	30·	6·	36·	18·	42·
91·8	68·6	23·2	54·1	37·7	38·34	7·66	46·	23·	53·66
99·5	67·	23·5	50·8	39·7	39·17	7·83	47·	23·5	54·83
98·5	71·5	27·	55·5	43·	45·	9·	54·	27·	63·

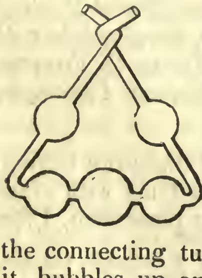
Now it is remarkable that if instead of *adding* this correction (col. 7), which was found by experiment, we *deduct* it from the double difference (col. 8), we get (the numbers in col. 6, corresponding with) the differences between Apjohn's dry-bulb and dew-point (col. 5); and so close is this correspondence when the slight differences in the leading numbers (cols. 3 and 9) are taken into the accompt, that we can hardly persuade ourselves that it is accidental.

Hence Apjohn's experiments fix the dew-point at double the descent of the wet-bulb—the effect of current; and an upward current, in some proportion to the rate of evaporation, must obtain over an evaporating surface. But Mason's number to be deducted, which takes the effect of current in +, is both deduced from and supported by direct experiments.

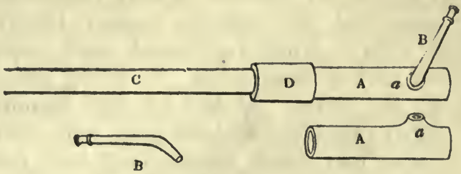
It further follows from the premises, and is apparent in the table (cols. 5, 6, 8, 10), that the mean of their respective differences between the dry-bulb and dew-point is simply double the difference between the dry and wet-bulb; a relation previously deduced from numerous experiments by August, Bohnenberger, and others. Berzelius in giving us this information (*Tr. de Chim.*, viii., art. HYGROMETRE) has not referred to the works in which these experiments are detailed: if you have the means of hunting them out, and giving such abstracts as will enable British meteorologists to judge of the comparative degree of confidence to which they are entitled, it may render an important service to meteorology. The best instruments are subject to the influence of various circumstances in taking the dew-point; and the experiments of Apjohn, in which most of these interferences are obviated, are objected to by Dr. Hudson, in a paper immediately accompanying their publication.

In the mean while it must be remembered that Dr. Apjohn's dew-point is that at which the air, by passing through water, *took up* moisture, and should be under rather than over saturated, so that it would, of course, not deposit any at that temperature. Dr. Mason's, on the other hand, was that at which a metallic surface became dewed; the air depositing moisture, and being of course supersaturated. Between these two a few degrees of temperature would intervene; the mean of which, the true point of saturation, would be the same as above deduced from the tables. It therefore becomes a question whether this simple deduction may not bring us nearer the truth, on the long run, in the present state of our knowledge on the subject, than more elaborate calculations or even observations with our best instruments, considering the temporary character of such observations, and the influence of circumstances to which they are liable.

I have been led into these inquiries, in considering the present difficulty of ascertaining chemically the causes of differences in the atmosphere, produced by the diffusion through it of infectious and other matters in very minute proportions. It would seem that the aqueous vapour likewise diffused brings down with it when condensed, such miasmata, &c., as were either merely suspended, or, being dissolved, have much affinity for water. In fact we find the glass of windows and other non-absorbent surfaces here incrusted with salt, for three or four miles inland, after long southerly and westerly winds; our stream of water from Dartmoor always acquires a slight saline impregnation before it reaches the town; and in malaria and other unwholesome climates the stranger is particularly cautioned to avoid the evening dews. This *natural* dew, if collected in sufficient quantity, would probably be most fully impregnated; but when this cannot be done, a common glass carboy, filled with a cheap refrigerating mixture and suspended over a funnel in a gentle current of air on a dewy evening, will collect a sufficient quantity for experiment. Rain is too dilute, and the clouds from which it falls too migratory to answer the purpose. For matters little attracted by water, reagents of more energetic affinities may be suspended in shallow vessels, kept cold, when requisite, by refrigerating mixtures beneath. But when the air is very dry, when it is necessary to concentrate upon a very small quantity of reagent the foreign matter in a large quantity of air, or to know the quantity of air submitted to its action, the test may be contained in a tube like that of Liebig for condensing carbonic acid, &c., in organic analysis, (described below,) and the air blown through from a pair of small cylindrical bellows, the cubical contents of which are known; or for greater accuracy, by a condensing syringe, containing an exact number of cubic inches; though the bellows are more conveniently portable. To thoroughly extract the foreign matter, the air may be repeatedly passed through the tube by receiving it at the end in a bladder furnished with stop cock, &c., having a second bladder, which can either replace it when full, and removed to the entering end, or it may be returned from one to the other, through a tube with a two-way cock, to save the trouble of screwing and unscrewing.



Liebig's condensing tube for carbonic acid, &c., in organic analysis.—For carbonic acid the three lower bulbs are filled just above the connecting tubes with solution of potass, through which it bubbles up and keeps the liquid in agitation; the two bulbs in the legs are safety bulbs. For ammonia, &c., the appropriate reagents are, of course, substituted.



Modification of Gahn's Blowpipe.—I employ a modification of Gahn's blowpipe which is, perhaps, more convenient in use, and more clean and agreeable to handle. A is a piece of brass tube closed at the end, and having a boss near the end perforated to admit the beak at *a*. B is the beak, fitted to *a* by grinding, and bent at an angle of 135°, which I find more convenient than a right angle for directing the jet upward or downward by a little turn of the beak without disturbing the support, and for *minutiæ* in operating. It can be turned in any direction. C is a piece of glass barometer tube, fitted into A by binding round with fine thread, and fixed with a little plaster of Paris: thus A becomes the condensing chamber; and when moisture is collected there in so large quantity as to affect the beak, (but which will never take place in a single operation, however long,) the beak is withdrawn and the moisture blown out through *a*. D is a bit of hard wood turned, and cemented round C; where it serves as a stop to its entering A, and as a good holdfast in operating; the smallness of the tube in many blowpipes giving the fingers some difficulty in commanding them, which occasions the preference felt by so many operators for the blowpipe with a condensing bulb in the middle.

I am, dear Sir, yours very truly,

Plymouth, April 19, 1837. J. PRIDEAUX.

XII. *Account of some Experiments made in different Parts of Europe, on Terrestrial Magnetic Intensity, particularly with reference to the Effect of Height. By* JAMES D. FORBES, *Esq., F.R.SS. L. & E., &c., Professor of Natural Philosophy in the University of Edinburgh.**

1. THE Council of the Royal Society of Edinburgh having, on my application in 1832, entrusted me with Hansteen's magnetic intensity apparatus, in their possession, I feel

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* From the Transactions of the Royal Society of Edinburgh, vol. xiv., having been read before that society December 19th, 1836.

it to be my duty to communicate to the Society the results then and subsequently obtained with it.

2. The instrument consists of a mahogany box 5 inches long, 4 broad, and 2 deep, with sides and top of glass, having also a wooden tube, screwing into the top, for containing a silk-worm's fibre about 5 inches long, by which the magnetic needle is suspended so as to place itself horizontally, and after being caused to deviate from its point of rest, the time of any given number of oscillations in a horizontal plane is measured,—whilst a graduated circle in the bottom of the box indicates its arc of vibration.

3. The needles which accompanied the instrument, when originally sent from Norway, are two in number, one a cylinder 3 inches long and 0·1 inch in diameter, is marked on its case "No. 1." The other is shorter, thicker, and heavier, and from its form has always been called the "Flat" needle. These were the needles used with this apparatus by Mr. Dunlop, in the experiments made in Scotland at Sir T. M. Brisbane's expense, and published in vol. xii. of the Society's Transactions. A reference to that volume will show clearly Professor Hansteen's and Mr. Dunlop's method of observation, which, essentially, I have always followed.

4. If we assume the magnetism of a needle to remain invariable, the intensity of the earth's magnetism at different places, or at the same place at different times, will be (on the principle of the pendulum) inversely as the square of the time required to perform a given number of vibrations in infinitely small arcs, under the different circumstances. But various adjustments have to be attended to, and corrections applied.

5. Nothing more portable or more simple than the instrument in its present form can be desired. These requisites are no doubt obtained at the expense of some accuracy. Mr. Snow Harris has shown* that the influence of the surrounding air upon the needle gives rise to considerable errors, especially when the needle is so small and light, as in Hansteen's apparatus. But greatly to enlarge the needle, and to connect an air-pump with the apparatus, is nearly equivalent to depriving the traveller of its use altogether. Hansteen's instrument was the constant companion of my pedestrian excursions, and had it been in any other form, the present observations would probably never have been made. Besides all this, there are sources of error arising from imperfectly known and irregular variations of the earth's intensity, and equally or more important ones from changes of magnetic intensity in

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* Edinburgh Transactions, vol. xii. p. 1.—See also the Observations of Professor Bache; American Phil. Trans., vol. v.

the needle itself, which the improvements in question do not affect. Until by a regular and long-continued series of observations, such as those likely to be undertaken at Greenwich, magnetism shall be reduced to more of a science than it is at present, we must beware of pretending to illusory accuracy in a traveller's detached experiments. Those about to be detailed in this paper, will sufficiently indicate the degree of comparability of observations made with Hansteen's instrument, such as it is, and which is by far the best test of their real value. It has certainly rather exceeded than fallen short of my expectations.

§ 1. *Adjustments and Method of observing.*

6. Hansteen's instrument contains no provision for securing the horizontality of the needle, which is of considerable importance. The needles have, indeed, sliding collars of suspension, which may be altered with change of dip, but the box has no adjusting levels. I have always* used a small spirit level for adjusting the bottom of the box, and then, as carefully as I could, made the needle hang parallel to it, but the adjustment was troublesome and unsatisfactory.

7. The needle being levelled and allowed to come to rest, it was drawn out of its position of rest †, but always in a horizontal plane, by the approach of a piece of iron or steel (usually a penknife), and the process repeated until the semiarc of vibration exceeded 20°, if 300 vibrations were to be observed, or 10° if 100 only were observed, as was more usually the case. This last deviation from Professor Hansteen's practice was not adopted without due consideration. The large commencing arc necessary in order that the vibrations might be distinguishable at the close of 300, increased greatly the errors pointed out by Mr. Harris. Moreover, there seemed less chance of error in combining several series of 100 vibrations taken in succession, than in using a single series of 300, which, from the time it occupies, is more liable to be interrupted and rendered useless by a gust of wind, or a momentary relaxation of the painful attention required to be exerted by an unassisted observer. Besides, the mere error of the *observed time*, depending on the eye and ear of the observer, will not exceed even in 100 vibrations the uncertainty arising from causes impossible to eliminate,—indeed falls

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* I cannot answer however, for two or three of the first observations hereafter to be quoted.

† As the torsion of the silk fibre must have some influence, it is not unimportant to remark, that the same thread which was adapted to the instrument in August 1832, has been used ever since.

much short of it: yet this is the only error which we diminish by increasing the vibrations in a series to 300.

8. When the semi-arc of vibration had diminished to 20°, or to 10° (as 300, or 100 vibrations were to be observed), the counting of vibrations commenced,—the hour, minute, second, and decimal, of the beginning or 0th vibration being noted, and the second and decimal only for each succeeding 10th vibration, until 360 vibrations (in the first case) or 160 (in the second) were observed. These seconds of time are arranged in columns, so that the times of the 0th, 100th, 200th, 300th vibration, run along the same horizontal line as do the 10th, 110th, 210th, 310th, &c. The time of the 0th being then subtracted from the time of the 300th (or 100th), we have one value of the time of 300 (or 100) vibrations. The 10th, from the 310th (or the 110th) gives a second value, and so on to the 60th and 360th (or 160th), which gives in all seven values of time of 300 (or of 100) vibrations; the mean of which seven values is taken (the minutes being of course supplied), and the hour, minute, and second of conclusion. The thermometer (inclosed in the box) is consulted at the beginning and end, and its indications registered. The rate of diminution of the semi-arc of vibration is also observed, its continued bisection being indicated opposite to the instant at which it occurs in a column parallel to those already named. The rate of the chronometer is likewise to be determined. An example will best illustrate all this.

MAGNETIC INTENSITY.

Place, Greenhill, near Edinburgh. — Ther. Reaum.
Date, 7th May, 1833. — Beginning, $5^{h}\ 3^{m}\ 41^{s}{\cdot}6$ — 18°·0
Needle, Flat. — End,.... 25 34·5 — 17°·3
Mean, 17·65

Arc.	Sec.	Arc.	Sec.	Arc.	Sec.	Sec.	300 Vibrations.	
							m	s
20°	41·6		47·8		52·0	56·7	18	15·1
	18·3		24·0		28·3	32·5		14·2
	54·7	10°	0·4		5·3	9·2		14·5
	31·0		36·5		40·8	45·7		14·7
	8·5		14·0	5°	17·7	22·0		13·5
	45·0		49·8		54·2	58·2		13·2
	21·7		26·0		30·7	34·5		12·8
	58·3		3·3		7·7	Mean, $18^{m}\,14^{s}{\cdot}00$		
	35·0		39·0		43·6			
	11·1		15·8		19·8	$= 1094^{s}{\cdot}00$		

Rate + 3^{s}

9. My method of observing was to keep the chronometer at the ear till the instant that the termination of a vibration was observed, then to count five beats of the balance (corresponding to two seconds), which affords time to bring the dial-plate into view, and the seconds entered in the Table are those read off at that time, namely two seconds later than the absolute times. Thus the impracticable attempt to observe two things at once by the eye is avoided. For this and some other suggestions, I am indebted to my friend Captain P. P. King, R.N. The observations are registered in lithographed forms bound into volumes.

10. In the choice of stations I have been extremely particular, often at great personal inconvenience*. Places remote from any trace of habitation have most usually been selected, and in no case have intensity observations been made in a house. The specialties of the sites will be noticed in the following Tables. I have invariably removed all masses of iron from my person; and in my later experiments even took the precaution of carrying thin shoes, in order that the heavily nailed ones which I usually wear, might be removed to a distance. The chronometer, too, has generally been held at some distance from the apparatus. But some direct experiments lead me to believe that the influence of the two last-mentioned sources of error is insensible.

§ 2. *Corrections applicable to the Observations.*

11. When the mean of seven values of 100 or of 300 vibrations has been taken, as above explained, a variety of important corrections remain to be applied.

12. I. *Rate of Chronometer.*—The following rule due to Professor Hansteen is simple and accurate:—"The logarithm to five decimal places of the observed time is taken, unity is to be *added* to the fifth decimal place for every *two* seconds *per diem* that the watch goes *slow;* and unity *subtracted* for every *two* seconds that the watch goes *fast*." The demonstration is too simple to require notice. The following is a table of corrections:—

TABLE I.

	Log. Additive.		Log. Additive.
Rate + 0sec	0·00000	Rate − 0sec	0·00000
2	0·99999	2	0·00001
4	0·99998	4	0·00002
6	0·99997	6	0·00003
8	0·99996	8	0·00004
10	0·99995	10	0·00005

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* None but those who have been engaged in observations of the very same description, where the eye, the ear, and the memory are all actively employed, can have an idea of the difficulty of always finding sites free from the interruptions of curiosity, or natural obstacles.

There is another chronometric correction worth mentioning, arising from the necessarily imperfect division of the seconds' circle of an enamelled dial-plate. In my watch this amounts to a sensible quantity, and has often given an apparent discrepancy to the partial results of a series for which I was not prepared. Upon investigation, I find, however, that the effect upon the *mean* will always be so insignificant as to be hardly worth notice.

13. II. *Arc.*—A correction due to the motion of the magnetic pendulum in circular arcs, cannot be considered as a constant quantity, and therefore not affecting relative results, 1. Because the rate of diminution of arc varies considerably in different experiments, and is directly deduced from the observed law of diminution of arc; and 2. Because we sometimes have to compare observations of 100 vibrations having an initial semi-arc of 10°, with 300 vibrations beginning at 20°. The latter case having alone been considered by Hansteen, I reinvestigated the theory of the correction, and confirmed his numbers.

14. It is assumed that the arc diminishes geometrically in consequence of resistance, the time increasing arithmetically. The best observations I have made confirm the truth of this general admission. Again, we have to recollect that, in consequence of the degradation of the arcs, the reduction to infinitely small arcs for the vibrations between the 0th and the 300th, will be greater than between the 10th and 310th, &c., and that the *mean* of all the corrections (taking this variation into account) must be applied. The law of the diminution of arc, or the factor representing the ratio of the arc of one vibration to that of the immediately preceding one, will be at once deduced from observing after how many vibrations the arc is halved. Let m be that number, then if r be the factor in question, $r^m = \frac{1}{2}$; whence $r = {}^m\sqrt{\frac{1}{2}}$, which is known; and therefore m, together with the initial semi-arc of vibration, may be used as the arguments for entering the following table of corrections*: —

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* *Investigation.*—Let α be the initial semi-arc of vibration (*taken in parts of radius*), and r the ratio of its diminution by resistance in a single vibration.

Then, for the	1st,	2nd,	3rd,	4th, ... nth vibration,
The arcs will be	α,	$\alpha\, r$,	$\alpha\, r^3$,	$\alpha\, r^2$, ... $\alpha\, r^{n-1}$

And, by mechanics (Poisson, art. 184.), the times occupied by these vibrations will be (the time of an infinitely small vibration being unity),

$$1 + \frac{\alpha^2}{16}, \quad 1 + \frac{\alpha^2\, r^2}{16}, \quad 1 + \frac{\alpha^2\, r^4}{16}, \quad 1 + \frac{\alpha^2\, r^6}{16} \ldots 1 + \frac{\alpha^2\, r^{2(n-1)}}{16}$$

And the *mean* duration of the n vibrations is,

TABLE II.

Initial Semi-Arc = $\alpha = 10°$.		Initial Semi-Arc = $\alpha = 20°$.	
No. of Vibrations observed = $n = 100$.		No. of Observations observed = $n = 300$.	
m	Additive Log.	m	Additive Log.
70	9·99978	70	9·99969
80	9·99975	80	9·99960
90	9·99972	90	9·99953
100	9·99970	100	9·99946
110	9·99967	110	9·99939
120	9·99965	120	9·99933
130	9·99962	130	9·99926

I have every reason to think this correction to be accurate on the whole: the agreement of the two modes of observation being in general very close.

15. III. *Temperature.*—This extremely important correction it is very difficult to determine. Without an accurate estimation of it, it would be vain to attempt to decide whether or not the magnetic energy varies with height; because at great elevations the temperature being always diminished, the inten-

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$$M = 1 + \frac{\alpha^2}{16} \cdot \frac{1 + r^2 + r^4 + r^6 \ldots r^{2(n-1)}}{n} = 1 + \frac{\alpha^2}{16} \cdot \frac{1 - r^{2n}}{(1 - r^2)n}$$

Hence the mean duration of n vibrations,

From the 0th to the nth is $1 + \frac{\alpha^2}{16} \cdot \frac{1 - r^{2n}}{(1 - r^2 n)} = 1 + \left(\frac{\alpha}{4}\right)^2 . \mathrm{A}$

——— 10th to the $(n+10\text{th})$ is .... $= 1 + \left(\frac{\alpha}{4}\right)^2 r^{10 \times 2} . \mathrm{A}$

(because the initial arc instead of α is αr^{10})

——— 20th to the $(n+20\text{th})$ is .... $= 1 + \left(\frac{\alpha}{4}\right)^2 r^{20 \times 2} . \mathrm{A}$

——— 60th to the $(n+60\text{th})$ is .... $= 1 + \left(\frac{\alpha}{4}\right)^2 r^{60 \times 2} . \mathrm{A}$

And the mean value of these deviations is

$$1 + \left(\frac{\alpha}{4}\right)^2 . \mathrm{A} . \frac{1 + r^{20} + r^{40} \ldots + r^{120}}{7} = 1 + \left(\frac{\alpha}{4}\right)^2 . \mathrm{A} . \frac{1 - r^{140}}{7\,(1 - r^{20})}$$

The concluding factor may be called B; and substituting the value of r from the text, or $\left(\frac{1}{2}\right)^{\frac{1}{m}}$, we have

$$\mathrm{A} = \frac{1 - \left(\frac{1}{2}\right)^{\frac{2n}{m}}}{\left(1 - \left(\frac{1}{2}\right)^{\frac{2}{m}}\right) n} \qquad \mathrm{B} = \frac{1 - \left(\frac{1}{2}\right)^{\frac{140}{m}}}{7\left(1 - \left(\frac{1}{2}\right)^{\frac{20}{m}}\right)}$$

(the last factor being independent of n), and we have

$$\text{Corrected time} = \frac{\text{Observed Mean Time}}{\left(1 + \frac{\alpha}{4}\right)^2 . \mathrm{A} . \mathrm{B}},$$

whence the Tables are computed.

sity would appear too great (the magnetic energy in iron being increased by cold and diminished by heat). I therefore endeavoured to compare the intensity of the needles employed within the range of temperatures usually observed. The apparatus employed was of this kind. The needle was first allowed to take the temperature of a heated room and vibrated. Then everything else remaining the same (and of course any local attraction which might exist being unaltered) the apparatus was placed in a cylindrical glass jar, with ice in the bottom, placed in a dish of ice, and covered with a glass plate also covered with ice. A steady temperature, but little above the freezing point, was thus attained, and the oscillation again observed. These experiments were repeated many times. One series was undertaken at Geneva in October 1832, another at Edinburgh on four different days of August 1834. Those for the needle, No. 1, were conducted with the most scrupulous care, nearly 5000 vibrations having been counted for this purpose alone. One set was discarded as differing too much from the others, and the remainder agreed very closely, although made under such different circumstances, and at such different times. The result adopted for needle No. 1 gave an increase in time of ·00045 for a diminution of temperature of 1° Reaumur, and *vice versâ*; for the flat needle (determined from two concordant series, both observed at Geneva on different days,) ·00030. From these results the following tables were calculated, giving the reduction in each case to 0° of Reaumur (which being the scale attached to the instrument, was always observed in these experiments). This seems preferable to referring to any other arbitrary temperature, upon which observers do not generally agree.

Table III.

Additive corrections applicable to *five* place Logarithms of the *Time*, for the effect of Temperature.

NEEDLE, No. I.						FLAT NEEDLE.					
Temp. Reaum.	Correction.	Temp. Reaum.	Correction.	Proportional parts.		Temp. Reaum.	Correction.	Temp. Reaum.	Correction.	Proportional parts.	
					Corr.						Corr.
1°	9·99980	11°	9·99785	°·1	−2	1°	9·99987	11°	9·99859	°·1	−1
2	9·99961	12	9·99766	·2	4	2	9·99974	12	9·99846	·2	3
3	9·99941	13	9·99746	·3	6	3	9·99962	13	9·99834	·3	4
4	9·99922	14	9·99726	·4	8	4	9·99949	14	9·99821	·4	5
5	9·99902	15	9·99707	·5	10	5	9·99936	15	9·99808	·5	6
6	9·99883	16	9·99687	·6	12	6	9·99923	16	9·99795	·6	8
7	9·99863	17	9·99668	·7	14	7	9·99910	17	9·99782	·7	9
8	9·99844	18	9·99648	·8	16	8	9·99898	18	9·99770	·8	10
9	9·99824	19	9·99629	·9	18	9	9·99885	19	9·99757	·9	12
10	9·99805	20	9·99609			10	9·99872	20	0·99744		

16. I am disposed to think that the correction for temperature is always open to a certain degree of doubt. Perhaps the condition of magnetism in the needle is not necessarily that due to the temperature it possesses at the moment, but rather to a temperature it had formerly. I think I have in some cases perceived indications of this. The needle No. 1, which is more slender than the "flat," seemed to be more steady in its indications than the other, and as I have always placed more reliance upon its indications, so the effect of temperature was determined with most care.

17. IV. *Variations in the Earth's Magnetic Intensity.*—These variations must affect observations of the relative intensity at two places, if the observations be not simultaneous. These variations are either (1.) *secular*, showing a progressive change from year to year; (2.) *periodical*, that is, subject to short periods of variation and regular, as at different seasons of the year, and at different hours of the day; or, (3.) *accidental*, arising from the aurora borealis, or from unknown causes*. The numerical laws of these three may be said to be almost equally unknown; the variations of the second class have indeed been studied by Hansteen, Christie, Dove, and others, but the results are not sufficiently accordant to permit me to apply any of them to my observations. As, however, the epochs are always recorded, this correction may be applied at a future time, and in a more advanced state of science.

[To be continued.]

XIII. *Additional Remarks on the former Extent of the Persian Gulf, and on the Distinction between Babel and Babylon.*

By CHARLES T. BEKE, Esq., F.S.A.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

FROM the length of time which has elapsed since the insertion of my last communication†, I am led to conclude that the controversy which during the last three years has occupied so many pages of your valuable journal is now at an end. Before, however, allowing the subject to be entirely dismissed,

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* My friend Professor Necker of Geneva has pointed out to me one of the first recorded observations of the influence of the aurora upon the magnetic needle, the more interesting because the coincidence was unnoticed (apparently) by the observer himself. In Saussure's *Voyages dans les Alpes*, vol. iv. p. 300, that enterprising traveller notices an auroral appearance, observed from the Col du Geant on the 12th of July 1788, and in another part of the same volume (p. 308), records, amongst his magnetical observations, the unsettled state of the needle during the whole of that evening.

† See Lond. and Edinb. Phil. Mag. for July 1836, vol. ix. p. 34, *et seq.*

I am desirous of adding a few references and observations which have since occurred to me.

With respect to the changes which have taken place in the course of the Euphrates, the authority of the Nubian geographer, Ebn Idrisi, is important. This writer, after showing that one branch of the Euphrates joined the Tigris, by which vessels were brought down from Samosata to Bagdad, says that the other branch, skirting the desert, divided itself into several arms, of which one passed Tsarsar, another Alcatur, a third Sura, and a fourth Kufa, *and that all these arms terminated in the lakes**, which lakes, as described in my former papers, will have been formed, to the destruction of the course of the river, by the same process of change which has also subsequently annihilated them.

This change, as I have before stated, consists as well in an alteration of the courses of the rivers as in an advance of the land at the head of the gulf. And as upon this latter point Mr. Carter regards the express statement of Pliny as a mere "notion," adding that "certainly serious doubts may well arise of the authenticity of the passage" in which it occurs†, I am glad to be able to refer to so valuable an authority as that of the Rev. G. C. Renouard, who, far from doubting the authenticity of the passage or regarding the fact recorded in it as a *notion* of the writer, says: "*Nor can any doubt be entertained as to the continual augmentation and change of this coast*, when we learn from Pliny (vi. 31) that Charax, at first a maritime town, only 10 stadia (1¼ mile) from the sea, was distant 50 miles from it according to Juba, and as much as 120 miles in his own time, (in the first century of our era,) *as he had heard from persons well-acquainted with the place.*‡"

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* "Euphrates labitur deinde a Samosat, atque illinc ferre incipit naves usque ad Baghdad. A Samosat postea excurrit per meridiem declinans ad orientem...ad Hitz, ad Enbar; indeque defluit ad amnem Isa, ad Baghdad. Jacet autem Baghdad secus Tigrim. Reliqua vero pars Euphratis fluens a Rahaba, e tergo deserti, in varia dividitur brachia, quorum unum pergit ad Tsarsar, aliud ad Alcatur, aliud etiam ad Sura, quartum denique ad Kufam, et omnia illa brachia varios in lacus sese immergunt." p. v. clim. iv. cited in Hase's *Regnum Davidis*, p. 154.

† See Lond. and Edinb. Phil. Mag., vol. vii. p. 197.

‡ *Encycl. Metrop.* 4th div. vol. x. p. 256. I must not, however, omit to cite on the opposite side of the question the authority of a writer of no little renown, namely, Professor Heeren, who, in a treatise on the former shape of the Persian Gulf, after making the voyage of Nearchus to correspond with the present eastern coast of that gulf, by using a stadium of eight to the mile,—not less closely (it is worthy of remark) than Dr. Vincent has done by employing a stadium *of just half the length*,—gives it as his opinion that the northern coast, in the time of Nearchus, probably extended much further to the south than it does in the present day! His words are,

On the subject of the distinction between the Babel of the book of Genesis and the Babylon of Nebuchadnezzar, I have, in conclusion, to adduce the authority of Diodorus Siculus, who informs us that, at the time of the conquest of Babylonia by the Assyrians under Ninus, *Babylon itself was not in existence,* although the country contained several *other* cities of importance[*]. This statement is entirely corroborative of the conclusion drawn from the various other authorities already cited by me, namely, that the Babylon which was known to the prophets, and which existed in the times of the writers of profane history, was a totally distinct city from, and one of a far more recent date than the Babel which was erected in the plain of Shinar previously to the dispersion of mankind.

I am, Gentlemen, your most obedient servant,
Leipzig, March 23rd, 1837. CHARLES T. BEKE.

XIV. *On some of the Phænomena and Laws of Action of Voltaic Electricity, and on the Construction of Voltaic Batteries. By* CHRISTOPHER BINKS. *Addressed to* J. Frederic Daniell, *Esq., F.R.S., Professor of Chemistry in King's College, London.*[†]

DEAR SIR,

IN your letter addressed to Dr. Faraday[‡], describing your constant voltaic battery, you speak of seeking to reduce the zinc to a minimum comparatively with the surface of copper employed in the same mass of electrolyte; but it does not seem to have been your intention to determine precisely what was the proper relative proportion of the two metals. Subsequently to this remark in your letter, Mr. Mullins[§] has expressed his belief that the surface of zinc ordinarily brought into action in such combinations is much greater than is needed: but neither has he stated the precise results of experiment upon this point.

In the same communication, and in allusion to his new battery, Mr. Mullins speaks of his having discovered the existence of a new principle of action in voltaic combinations. He remarks, "In this battery the power is immense in proportion

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"Admodum tamen probabile videri, continentem quondam magis versus austrum procurrisse, partemque ejus non exiguam procedente tempore aquis esse submersam, non dissimulaverim." *Comment. Societ. Reg. Scient. Götting.* tom. xiii. *Cl. Hist.* p. 138. See also an abstract of the treatise printed in his *Historische Werke,* vol. iii. p. 337, Göttingen, 1821.

[*] *Κατ' εκεινους δε τους χρονους ἡ μεν νυν ουσα Βαβυλων ουκ εκτισμενη, κατα δε την Βαβυλωνιαν ὑπηρχον αλλαι πολεις αξιολογοι.* Lib. 11, c. i.

[†] Communicated by Professor Daniell.

[‡] Philosophical Transactions, 1836. [§] Phil. Mag. for Oct., 1836. p. 285.

to the quantity of metals used, and arises, I conceive, from the application of a principle which I believe is quite novel in the construction of voltaic batteries, namely, that of *diminishing* the metallic surfaces as the fluid in its onward passage *accumulates;* thus acquiring *increased* force in proportion to the smallness of the substance to which it is restricted." It was in the absence of the information that would have been derived from the promised communication from Mr. Mullins, or of precise information from any other source, that the following experiments were begun, having for their immediate object to determine what proportions should be preserved between the zinc and copper in any voltaic combination, and to trace the existence and nature of the principle of action alluded to in the above extract from the paper of Mr. Mullins.

Before the invention of your constant voltaic battery we possessed no instrument upon whose regularity of action we could depend, for either the establishing (by even an approximation to the truth) or the demonstrating of many of the laws of voltaic electricity. The battery of Wollaston, or modifications of it, either as an elementary or a compound one, had generally been employed in such investigations. But you have shown that there is an uncertainty and variableness in its operation, of so great an amount as should surely preclude its employment in experiments intended to decide upon the fundamental laws of the science.

It is, perhaps, to this circumstance mainly, or as much so as to that of our possessing no common standards of comparison in the majority of instances, that we owe the conflicting statements of the fundamental laws of the phænomena of galvanism which pervade the various treatises upon it. Of such conflicting statements we have one recent instance, among very many, in which the previously admitted law of the conductibility of wires of different lengths is called in question by E. Lenz *, who substitutes in its place another law, of which the expression is, that "their conductibilities are in an inverse ratio to their lengths," in contradiction to that formerly held of their conducting power being "inversely as the square root of their lengths." I would submit that many other such laws are not yet decided, since it has not been shown generally in what way provision was made to guard against the irregular operation of the exciting battery employed, or that experimenters generally were aware that such irregularities prevailed, or at least, prevailed to so great an extent as has now been shown.

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* Scientific Memoirs, part ii. p. 320. [See also p. 11 of the present Number.—Edit.]

Let a sheet of common rolled zinc, carefully selected for its apparent uniformity of surface and thickness, be amalgamated in such a way as to secure the greatest uniformity in the distribution of the mercury over its surface. Let plates be cut from this sheet, exactly of the same size, and then associated with corresponding copper plates; and however well this may have been done, and however exactly alike the plates and every attendant circumstance may be, it will be found that no two of the couples will give the same results in the same time, when arranged as simple galvanic circles and acted on by acids in the usual way. Whilst one zinc plate will lose 10 grains in a certain time, another compared with it and apparently exactly similar will lose perhaps only 6 grains, or, on the other hand, as much as 15 grains. Out of innumerable instances I have never been able to select two *exactly* alike, and in the closest approach to perfect similarity in the amount of action of any two which I have found there has still been between them a difference of $\frac{1}{20}$th of the whole amount.

Again, any one plate, associated with a copper plate as a simple circle, will lose less the first time of its immersion than during the second, of which the following is one taken out of many such examples:

A plate of amalgamated zinc, arranged with copper as a simple voltaic circle, and immersed (the acid being each time renewed) during periods of 30 minutes each:

Table No. 1.

In the	1st	time lost	8·8	grains
——	2nd	——	9·0	—
——	3rd	——	9·8	—
——	4th	——	11·2	—
——	5th	——	13·0	—
——	6th	——	14·3	—

And when at the end of these the plate was amalgamated afresh and reimmersed, the action was reduced below its first amount, namely, to 6·7 grains in the 30 minutes.

These sources of error, in cases where such elements are used, are independent of many others well known to experimenters, such, for instance, as accidental differences in the *distance* of the plates from one another, and the varying conditions of their surfaces.

Again, the common rolled zinc of the shops is very impure, and before undertaking investigations of phænomena in which comparisons of effects with the quantities of zinc consumed and hydrogen evolved are needed, the proper equivalent of the zinc actually employed must be determined.

In the midst of so many sources of error, against which the utmost care can scarcely provide, it is but to be expected, so long as the same materials are used, that the greatest contrariety of opinion will exist on points apparently so easy to determine. A method of avoiding those errors which have their origin in the irregularity of the mutual action of the acid and zinc will be submitted to you towards the end of my letter.

In order to avoid a too frequent recurrence to description in the course of this paper, I will, at this point, state generally some of the precautions (beyond the common and more obvious ones) that were taken to ensure accuracy in the results of the experiments.

The magnetic needle was never resorted to as a measurer of the amount of action or of the quantity of electricity developed; but this was estimated by what appears to me to be the more certain, though infinitely more laborious method, of finding, by the balance, the equivalent of zinc expended, or by actually measuring the volume of the evolved hydrogen. In no instance was the one calculated merely from the volume or weight of the other. In general both the balance and the meter were employed, whatever the number of the series in any experiment to be examined, and the number amounted in some cases to as many as fifty. I had anticipated that some curious results would appear from this mode of testing the phænomena, and have not been altogether disappointed. Again, when *plates* of amalgamated zinc were used in comparative experiments, in which *equal* ones, or differing by a certain ratio, were wanted, these comparative values were not estimated by the mere *measure* of their surfaces, but, by actually finding the amount of action upon them in a given time, by previous immersion in acid of a kind similar to that intended to be used; for an equality in the extent of surface does not ensure an equality of voltaic action, nor does that action increase in the same ratio as the surface may be increased, as has hitherto been believed. Again, the zinc employed throughout these experiments was always of the same quality, indeed was cut from the same sheet, and its equivalent determined, with hydrogen as unity, and found to be 34·5. And in order, as far as possible, to reduce the number of the conditions involved in these experiments and requiring to be attended to and estimated, one of such was entirely avoided, viz. that of variation in the distance between the two elementary plates. Whether used in independent or comparative experiments the mass of fluid interposed between the zinc and copper plates was invariably equal to one inch. The acid employed was the diluted sulphuric, and in those proportions which have become standard ones, through their having been used as such by yourself and Dr. Faraday. It is, perhaps, almost needless to remark that in any case where *local* *action* was discovered to have taken place it was either properly allowed for, the defective plate dismissed, or the experiment wholly repeated, and that where comparative estimates were made of the evolved gas or gasses, the proper corrections were made for temperature and pressure.

First Investigation. Part I.

To determine the relative proportions of the zinc and copper needed to induce the maximum effect in the action of any simple voltaic circle.

1st. To determine the influence and its extent of increasing the surface of the copper plate beyond that of the zinc.

Experiments.—A plate of amalgamated zinc, measuring 4 square inches on each face, was associated successively with plates of copper, first of an equal size to the zinc and afterwards of a greater size, and gradually increasing in the ratio of the subjoined table. The times of immersion were each 30 minutes; the acid, a mixture of 4½ sulphuric (by measure) and 100 parts water, and at the end of each time the acid was renewed and the plate rinsed in clean water and weighed, when

(Table No. 2.)

1st,	with the copper equal to the zinc, it lost	4·0 grains
2nd,	——————— twice the zinc, it lost...	5·2 —
3rd,	——————— 4 times the zinc, it lost	6·7 —
4th,	——————— 8 times the zinc, it lost	9·0 —
5th,	——————— 12 times the zinc, it lost	15·1 —
6th,	——————— 16 times the zinc, it lost	19·5 —
7th,	——————— 20 times the zinc, it lost	16·6 —
8th,	——————— 24 times the zinc, it lost	17·2 —

But throughout these experiments the *same* zinc plate was subjected to the action of the acid, and that extending through eight periods of thirty minutes each, or through four hours. And it has been shown above in table No. 1 that a plate of zinc so placed loses more in the *second* time of immersion than during the *first.* Accordingly, to protect this examination from such a source of error, another plate with the copper *equal* to it was employed simultaneously and under like conditions, and was weighed at the end of the same times, when it was found to have lost in

(Table No. 3.)

1st time, loss = 4·3 grains = ·0
2nd time, loss = 4·3 grains = ·0
3rd time, loss = 4·4 grains = ·1
4th time, loss = 4·6 grains = ·3
5th time, loss = 4·9 grains = ·6
6th time, loss = 5·3 grains = 1·0
7th time, loss = 5·9 grains = 1·6
8th time, loss = 6·7 grains = 2·4

In progressive increase after the first immersion.

which increase of action (reckoning from the third result) being deducted from the results of the corresponding periods in the former table, will leave that table to show the influence merely of the increased size of the copper plates, which is the point sought for. Therefore the table so corrected stands thus:

Table No. 4. corrected (from No. 2).

1st,	copper equal to zinc, loss	4·0	grains
2nd,	——— twice the zinc, loss	5·2	—
3rd,	——— 4 times the zinc, loss	6·6	—
4th,	——— 8 times the zinc, loss	8·7	—
5th,	——— 12 times the zinc, loss	14·5	—
6th,	——— 16 times the zinc, loss	18·5	—
7th,	——— 20 times the zinc, loss	15·0	—
8th,	——— 24 times the zinc, loss	14·8	—

From which it appears that the greatest amount of action between the zinc and the acid is induced when the copper plate is 16 times larger than the zinc.

But in order further to test the accuracy of these results eight distinct zinc plates were selected, as nearly as possible alike, and the experiments repeated with the aid of a *fresh* zinc plate during each time, when the results were exactly the same as those exhibited in the corrected table No. 4.

This table, then, shows the proportion of the copper by which the greatest effect is produced under the then existing conditions of the arrangement. But it does not show us what is the absolute amount of power gained by using this proportion in preference to plates of an equal or of any other size; neither does it show whether the law it establishes will hold good under varying conditions of the action of the arrangement; whether, for instance, if its energy be increased or diminished, it will still require, to produce the utmost absolute effect, that the copper should be maintained of a size 16 times greater than the zinc.

To determine the former of these points let the result obtained by the plates of *equal* surface in zinc and copper (which we have in the first line of the above table No. 4) be taken as unity, and the other results reduced by it, when the table will stand thus:

Table No. 5. reduced (from No. 4).

1st,	copper equal to the zinc, gave, loss	1·0	nearly.
2nd,	——— twice the zinc, gave, loss	1·3	
3rd,	——— 4 times the zinc, gave, loss	1·6	
4th,	——— 8 times the zinc, gave, loss	2·1	
5th,	——— 12 times the zinc, gave, loss	3·6	
6th,	——— 16 times the zinc, gave, loss	4·6	
7th,	——— 20 times the zinc, gave, loss	3·7	
8th,	——— 24 times the zinc, gave, loss	3·7	

By which it is made apparent that (compared with equal plates) the effectiveness of any simple voltaic circle is increased about 4½ times, by having the surface of copper 16 times greater than the zinc.

But these results may not follow from the operation of an *invariable* law, but of one peculiar to the prevailing conditions of the arrangements herein employed; as the activity of its action may evolve as much hydrogen as needs so large a surface to permit its formation with the greatest facility. But let the activity of the generating agents be lessened, and will it then need a surface equal to 16, comparatively, to induce the greatest effect? Now this rapidity of action may be lessened by either removing the elementary plates further apart, or by more largely diluting the exciting acid. And to determine the influence of such changes, first, the plates were separated from the distance of one inch to that of two inches, and the experiments, with coppers of various surfaces, repeated, when, although less zinc was expended in the aggregate during an equal time, yet the greatest loss in any given period occurred when the copper was 16 times larger than the zinc.

The plates being restored to their usual distances from one another, the experiments were repeated twice, with acid solutions, formed, first, by the mixture of 2¼ parts acid and 100 water, and, secondly, of 9 parts acid and 100 water, when the results were so nearly the same as those registered in the above table, as to indicate, without doubt, the operation in each instance of the same law, and to make it unquestionable that it was general, within at least the range of those circumstances here brought into operation. For though, in the one case, the quantity of action was less and in the other more than in the standard tables, yet the maximum effect, in the respective sets of trials, was always exhibited when the copper was 16 times greater than the zinc.

An important point of inquiry now presents itself to be decided, namely, whether it is by the extension of *surface* merely that this advantage is gained, or whether it be due to the greater mass of conducting copper thus brought into action, or to the united influence of both. Experiments were made to determine this point, from which this one may be selected:

An amalgamated zinc plate, measuring 4 square inches, was employed in the first instance, along with a sheet of thin copper, weighing a few ounces, and having a surface of 16 square inches, when the result in zinc lost was $6\frac{9}{10}$ grains. Then the same zinc plate was connected with a solid prism of copper, of the same external dimensions as the sheet just used, but weighing about two pounds, when the zinc lost, during an equal time, 7·2 grs. And when the solid mass was extended in its surface, or (which is the same thing) when two pounds' weight of sheet copper was joined with the same zinc plate, the amount of action upon it was found to be increased from 7·2 to 16·5 grains; the former loss resulting from the influence of a surface equal to 16 square inches, and the latter from that of about 170 square inches; showing distinctly that it is to extended *surface* only that this effect is to be ascribed, and that it is in no respect due to the influence of the better conducting power of the metal as a *mass.*

It is remarkable that in this law of action to which the foregoing experiments have led me, but which is widely different from any I had anticipated, we have an exact correspondence with the specific gravities of the two gases involved in its operation. The specific gravities of oxygen and hydrogen are as 16 to 1; and it is when this proportion between the two surfaces, upon which they respectively appear, is preserved, that any voltaic combination seems to be placed in the best position for the exercise of its full power. This correspondence may therefore be presumed to be something more than a mere coincidence.

Two other points, bearing more immediately upon the application of these principles to the construction of the battery, remain to be decided. In the preceding experiments the copper plate did not extend over, nor was it placed opposite to, both surfaces of the generating zinc, as in the arrangement adopted by Wollaston; but was placed opposite *one* face only of it. It therefore occurs, as a part of the inquiry, whether the action upon the zinc took place upon or belonged to that surface only which was next to the conducting copper one, or whether it was due to an influence extending over the *whole* surface of the zinc immersed: whether, if another copper plate, of the determined dimensions, were placed over against the other surface of the zinc, we should gain a further and double accession of power?

Experiment. 1st. A zinc plate, with copper equal to 16, placed on *one* side only, lost 12·0 grains. 2nd. The same, with *two* copper plates, one on each side, and presenting, in full, a surface comparatively of 32·, lost 12·3 grains in an equal time.

There is, therefore, nothing to be gained by apportioning *both* surfaces of the generating plate, provided the copper plate on the one side be in the full proportion.

Again: Is the influence of the *copper* plate due to the exercise of *one*, or of *both its* surfaces? Do they both operate or only that one opposite the zinc?

Experiment. 1st. A copper plate, clean on *both* its sides, was first put in operation; when its associated zinc plate lost 12·2 grs. 2nd. One side of the copper plate was now covered with wax and the connected zinc plate then lost 10·5 grains; showing, as you have remarked before*, that the outer surface is, to a slight extent, engaged in the operation; but not so importantly as to make it of much regard in the construction of voltaic batteries.

First Investigation. Part II.

To determine the influence and its extent, of increasing the size of the zinc plate beyond that of the copper one.

In some of the galvanic batteries recently constructed the form is such that each successive couple is greater or smaller by a certain ratio than its adjoining one; and the arrangement of such a combination may be, either that a zinc plate is operating along with a larger copper plate; or that the zinc plate is the superior in size compared with the copper one immersed with it and operating in the same mass of liquid. Besides this, it is a matter of common remark, that the power of such arrangements is augmented by the employment of zinc plates the larger of the two.

Having already determined the kind and amount of influence exercised by copper plates of various comparative sizes, it follows to determine in what way the zinc or generating surfaces will operate when made the greater of the two; the ultimate object of the inquiry being to test the alleged superiority of those forms of voltaic batteries which permit of such an arrangement being brought into action.

The experiments were conducted in a manner similar to the preceding ones, except that, in as much as the zinc plates here used were too large to admit of their being weighed with sufficient nicety, the amount of action was estimated, not by the loss of metal, but by the measure of evolved hydrogen.

First the two plates were of equal size, then the zinc was increased, the original copper plate being used throughout.

Table No. 6.

1st.	Copper and zinc, when equal,	gave	hyd. = 0·8 cubic inches.
2nd.	——— with zinc, twice its size,	—	hyd. = 1·6
3rd.	——— zinc, four times,	—	hyd. = 2·4
4th.	——— zinc, six times	—	hyd. = 2·5
5th.	——— zinc, eight times,	—	hyd. = 2·2
6th.	——— zinc, twelve times,	—	hyd. = 2·2
7th.	——— zinc, sixteen times,	—	hyd. = 2·2
8th.	——— zinc, twenty times,	—	hyd. = 2·2
9th.	——— zinc, twenty-four times,		hyd. = 2·2

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* Philosophical Transactions, 1836, p 113, 116.

Then, taking the results of equal plates (or the first line of the above) as unity, this table reduced stands thus:

Table No. 7. (No. 6. reduced.)

1st.	Equal sizes	=	1·0	nearly.
2nd.	Zinc increased by 2	=	2·0	
3rd.	———— by 4	=	3·0	
4th.	———— by 6	=	3·1	
5th.	———— by 8	=	2·7	
6th.	———— by 12	=	2·7	
7th.	———— by 16	=	2·7	
8th.	———— by 20	=	2·7	
9th.	———— by 24	=	2·7	

Showing by the first (No. 6.) that the greatest effect follows upon the employment of a zinc plate six or seven times larger than the copper; and by the second table (No. 7.) that the absolute amount of action gained (when that is at its maximum) is about three times beyond that produced when the plates are equal.

To sum up the results brought out by the foregoing investigation, it appears that both the zinc and copper plates exercise a definite influence when either is made the larger in any voltaic arrangement; but that that influence is different in degree in each case; that when the *zinc* is the larger, its full effect is obtained when it is about seven times the greater, and the absolute amount of that effect is three compared with equal plates as one: but that when the copper is the greater in the arrangement, then its full effect is not reached till it measure sixteen times greater than the zinc, and the total absolute amount gained, over equal plates, is four and a half.

Having ascertained these facts, I now proceeded to trace their influence in *compound* arrangements, as well as in the simple elementary ones to which the experiments had hitherto been restricted. Accordingly I prepared three batteries, consisting of a series of ten couples each.

The first (A) had the zinc and copper plates of *equal* size, and each plate presented a surface of four square inches to the action of the acid.

The second (B) had zinc plates of the same dimensions as those of A, but were connected to copper ones sixteen times larger, that is, the zinc were each four and the copper sixty-four square inches in surface.

The third battery was furnished with copper plates the same size as those in A; but the zinc ones were seven times larger, that is, the copper ones were four square inches each, and the zinc ones twenty-eight.

These batteries being each excited by an acid mixture of the same strength, and their action continued through equal times, gave the following results, as indicated by an interposed voltameter:

A, in	30 minutes gave in	mixed gases	1·2 cubic inch.
B, in	do.	do.	6·8 cubic inches.
C, in	do.	do.	4·5 ———

Demonstrating that the laws just proved to operate in the case of single arrangements, operate also in that of compound ones. But the advantages gained in B and C are greater even than had been anticipated by those laws, a circumstance which is explained in a subsequent investigation (No. 3.), when it will be proved that the above results are in *exact* accordance with those found to obtain in the case of the single arrangements already treated of.

There is one fact brought out by the above experiments to which I should wish to attract your attention, merely premising, that inasmuch as the coincidence between it and some other facts, which I suspect may be detected, may or may not be purely of an accidental character, I should wish to be understood as attaching no importance to it beyond that which would make me bear it in mind till our further acquaintance with the origin and operations of this still mysterious agent may contribute to its proper explanation. The fact I allude to is this: in Table, No. 6, it is shown that it is when the surface of the zinc plate becomes equal to about seven (the copper being one) that the full voltaic effect is produced: why is it, that the action regularly increases in amount till the respective surfaces reach this relative proportion, and that it then ceases to be augmented under any further addition whatever?

Second Investigation.

An inquiry into the comparative action of surfaces of different extent; that is, whether the action of any voltaic combination increases directly as the surface increases, or in some other ratio.

Depending upon the accuracy of that law (which has been generally admitted), that if the surfaces of the plates be increased, the resulting action will be increased in the same ratio, I had been led into some strangely anomalous results in my experiments, which could be accounted for only on the supposition that this law might possibly be incorrect. I had found little reason to dispute its accuracy when the examinations were restricted to plates varying from one square inch to four; but when carried beyond these sizes, its fallacy became strikingly apparent.

To examine into this, I prepared several simple arrangements composed of zinc and copper in the proportions of one to sixteen. The sizes of the zinc plates are stated in the subjoined table. At the end of each time of immersion the acid mixture was renewed, &c.; when it appeared that,

(Table No. 8.)

1st.	A zinc plate measuring		one	square inch,	lost (in 30 min.)	5·0 grs.
2nd.	A zinc do.	——	two —	inches,	lost (do.)	9·9
3rd.	A zinc do.	——	four —	—	lost (—)	19·0
4th.	A zinc do.	——	eight —	—	lost (—)	29·2
5th.	A zinc do.	——	sixteen	—	lost (—)	40·0

Now, had the formerly admitted law of increase been true, we should have had the last plate in the table exhibiting a result of action equal to sixteen times greater than the first one; since its surface is sixteen times larger. But the first loses five grains, and the last 40; whereas, had this law been correct, it should have lost 80 grains, or 5 × 16; but the result found by actual experiment is only one half or 40 grs.

But was this result the effect of any peculiar conditions attending the experiments? I varied the form of the experiments in every possible way, but with still the same general results. For instance, the zinc and copper plates were connected by wires of thicknesses proportional to their theoretical values; when the results were the same: or the conducting wires were made to extend around the whole outer edge of the generating plates and thence to the copper ones, or one entire side of the zinc was covered by a conducting plate of copper, (itself and the joinings, both in this and the last case, being carefully protected from the acid by varnish,) yet by every modification the same general results were brought out. The increase, therefore, is not as the surface, but in some other ratio yet to be determined.

Third Investigation.

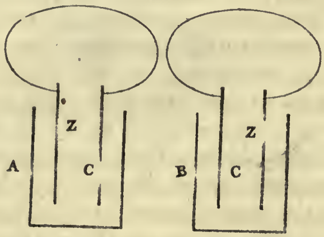
To determine the kind of influence exercised by single voltaic arrangements, one upon another, when used in connection, whether they be of equal or of unequal sizes.

It is but to be expected, *à priori*, that arrangements so situated and connected as in the following experiments, will exhibit the operation of laws, not of an arbitrary but of a definite and unchanging kind. The great discovery of Faraday leads necessarily to this expectation; and every correct experiment will unquestionably confirm his law, and exhibit its extended influence. These experiments, however, have no pretension beyond that of reaching to such a degree of accuracy as shall be subservient to their avowed object: such a degree of minuteness only is aimed at as shall prove of practical value to the ultimate object of the whole inquiry, namely, the construction of the battery.

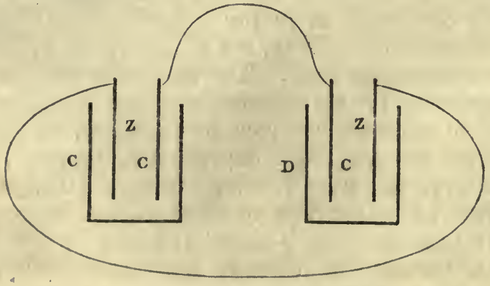
Let a single voltaic arrangement be immersed in acid of a certain strength, and during a certain length of time, and its voltaic action determined by the quantity of zinc lost, or of hydrogen evolved. Let another such arrangement (either of equal or of unequal size) be tested in the same manner; and then (having thus determined the amount of action peculiar to each when acting *separately*) let them be connected as a compound arrangement; that is, let the zinc of each one arrangement be joined by a wire to the copper of the other, and then the amount of action determined when thus operating upon one another.

These experiments were so contrived that the distance between the elementary zinc and copper plates, in any arrangement, was always the same; and also that the wires connecting them (whether when used separately or in connection) were always of the same length, in order to avoid those differences in the results which would have arisen had these precautions not been strictly observed.

I shall select the following as a preliminary example of such a mode of analysis:



A plate of zinc (in A) and a plate of copper of the same size, joined together as a simple circle, were immersed in an acid mixture during 30 minutes, and gave off hydrogen equal to $3\frac{2}{10}$ cubic inches. Another such arrangement (in B), but of larger size, was treated in the same way and yielded 16 cubic inches of hydrogen.



These same arrangements were now connected as represented in C and D; and (every circumstance being preserved the same as before) C yielded 4·4 cubic inches of hydrogen, and D precisely the same, viz. 4·4 cubic inches.

When separated.		When joined.	
A was equal to	3·2 cub. in.	C was equal to	4·4 cub. in.
B ———	16·0 ——	D ———	4·4 cub. in.
Total ...	19·2	Total ...	8·8

From which it appears that when mutually operating the action is distributed equally over both; but that its total amount is reduced by about one half from that which took place when the arrangements were distinct.

This example will serve to indicate the kind and object of the following experiments, with which I will now proceed systematically.

A. To determine the mutual influence exercised by arrangements when they are of the same size.

I prepared forty-eight arrangements, formed each of plates of zinc and copper of the same size, each plate measuring four square inches. I first took a number of these arrangements, and immersed them separately, and found, by an average, the loss that each sustained in a certain length of time. This average I found to be 5·7 grains of zinc expended; and it is accordingly placed first in the annexed table, as a standard of comparison.

I now took *two* of these arrangements and connected them as in the above example, and found the result of their action. Then *four* such were connected, and the results ascertained in the same way, as were also the results of combinations of 8, 16, 32, &c. as shown in the table.

Table No. 9.

1st.	Average loss of single arrangement		=	5·7 grains.
2nd.	Two arrangements, gave	loss	=	3·9 each.
3rd.	Four do.	loss	=	3·8 each.
4th.	Eight do.	loss	=	3·8 each.
5th.	Sixteen do.	loss	=	3·4 each.
6th.	Thirty-two do.	loss	=	3·6 each.
7th.	Forty do.	loss	=	3·8 each.
8th.	Forty-eight do.	loss	=	2·7 each.

Now it will be seen by this table that the amount of action in *combined* arrangements is *less* than the action in *single* ones. For instance, the action of two such joined is about one third less than takes place when they are used separately. This was unexpected; but not more so than some other results that follow. I have had the greatest difficulty in deciding upon the precise amount of the difference between the action of single and *double* arrangements. Sometimes the action in the latter case has been reduced one half, sometimes one fifth, but more frequently one third; and the cause of this variableness I have been wholly unable to detect. But this uncertainty does not attach to those cases in which more than *two* arrangements are operating, as will be made evident as these experiments proceed. It will also be seen by this table, that although the number of the series was extended to 48, yet the action in any one of the combined series never became equal in amount to that which took place when they were used separately.

B. To determine the mutual influence exercised by arrangements when they are of different sizes compared with one another.

As I have already given an example of this kind of examination above, I need not further explain the manner of conducting it, but will exhibit the results at one view in the subjoined

Table No. 10.

1st, Arrangements of equal size.

When separate.			When joined.		
A gave hyd. ...	0·8	cubic inch.	A gave hyd. ...	0·4	cubic inch.
B gave hyd. ...	0·8	——	B gave hyd. ...	0·4	——
Total	1·6		Total	0·8	

2nd, With the arrangements differing in size in the proportion of 1 to 3.

C gave hyd. ...	2·4	cubic inch.	C gave hyd. ...	2·4	cubic inches.
D gave hyd. ...	7·4	——	D gave hyd. ...	2·4	——
Total	9·8		Total	4·8	

3rd, With the arrangements differing in size in the proportion of 1 to 5.

E gave hyd. ...	3·2	cubic inches.	E gave hyd. ...	4·4	cubic inches.
F gave hyd. ...	16·0	——	F gave hyd. ...	4·4	——
Total	19·2		Total	8·8	

4th, With the arrangements differing in size in the proportion of 1 to 9.

G gave hyd. ...	1·4	cubic inches.	G gave hyd. ...	2·6	cubic inches.
H gave hyd. ...	12·0	——	H gave hyd. ...	3·6	——
Total	13·4		Total	6·2	

5th, With the arrangements differing in the proportion of 1 to 16.

I gave hyd. ...	0·8	cubic inches.	I gave hyd. ...	1·6	cubic inches.
K gave hyd. ...	12·8	——	K gave hyd. ...	2·6	——
Total	13·6		Total	4·2	

It will be observed here that in the 4th and 5th instances, in which there was a great disparity between the sizes of the two arrangements, the results of their combined action were not divided equally between the two, but appeared greater in that vessel in which the greater zinc plate was acting. But when we come to those experiments in which the arrangements are fitted to copper plates in the relative proportion of 16 to 1 of zinc, it will be seen that the results are different; that then in every instance the action is equally distributed through any combined series.

I now tried a combination of *three* arrangements, in which

Table No. 11.

When separate.			When joined.		
A gave hyd. ...	2·	cubic inches.	A gave hyd. ...	3·4	cubic inches.
B gave hyd. ...	9·	———	B gave hyd. ...	3 4	———
C gave hyd. ...	18·	———	C gave hyd. ...	3·4	———
Total	29·		Total	10·2	

These experiments appear to be interesting in other respects than as they bear upon the object for which they were expressly made. But confining myself to the immediate purpose I proceed to show:

C. The mutual influence of arrangements when they were equal in size compared with one another, but in which the elementary zinc and copper plates are in the proportion of 1 to 16.

Three arrangements were prepared exactly alike.

Table No. 12.

When separate.		When joined.	
No. 1, lost zinc ...	19·9 grains.	No. 1, lost ...	28·8 grains.
No. 2, lost zinc ...	25·0 grains.	No. 2, lost ...	29·3 grains.
No. 3, lost zinc ...	23·7 grains.	No. 3, lost ...	28·5 grains.
Total	68·6	Total	86·6

Showing by the results in A that when similar arrangements were connected with copper plates of the *same* size as the zinc, their combined action was remarkably *reduced;* but when the copper plates were in the proportion of the last experiment in C, then, instead of a great reduction in the amount of their action when combined, compared with what it was when they were separate, we find that their action is as remarkably *increased:* for by Table, No. 9, (in which the copper and zinc plates are equal) we find a reduction of about one third to take place on combination, and in this last investigation, C, we find as remarkable an increase.

D. To determine the influence exercised in those cases in which the *elementary* plates are in the proportions last observed (in C), but in which the arrangements themselves are *unequal.*

Three arrangements were employed.

Table No. 13.

When separate.		When joined.	
1st, lost zinc ...	5·0 grains.	1st, lost zinc	13·1 grains.
2nd, lost zinc ...	19·0 grains.	2nd, lost zinc	13·2 grains.
3rd, lost zinc ...	40·0 grains.	3rd, lost zinc	13·2 grains.
Total	64·0	Total	39·5

Showing a remarkable decrease in action (as has invariably occurred) to follow the employment of voltaic arrangements of unequal sizes when compared one with another in the same series.

I should wish it to be particularly remarked here, that in this last experiment (D) and in the one preceding it (C) the total surfaces of the metals in operation were exactly of the same dimensions.

The zinc plates in C measured each 7 square inches } which
The copper plates do. do. 112 square inches }

being multiplied by 3 (the number used) give zinc equal to 21 square inches, and copper equal to 336 square inches in the whole. And in D the 1st arrangement was composed of 1 square inch zinc and 16 copper; the 2nd, of 4 square inches zinc and 64 copper; the 3rd, of 16 square inches zinc and 256 copper, making a total, the same exactly as in C, namely, of 21 square inches of zinc, and 336 square inches of copper. And it has been shown that when these two sets of arrangements were put in operation, the one (C) lost 86·6 grs., and the other (D) lost only 39·5 grs.

The conclusion to which these experiments obviously lead is this: that there is no advantage to be gained by using the elementary combinations of any voltaic battery of an unequal size when compared with one another; but on the other hand, that such a form of battery is remarkably defective when compared with another, having an equal extent of metallic surface, but in which the elementary couples are of uniform size throughout the series.

To verify the conclusion here advanced I prepared *thre* small batteries, and tested their comparative efficacy by thei power of decomposing water.

Each battery consisted of a combination of six elementar couples, and the total surface of zinc in each battery was 6 square inches and of copper 1008 square inches.

The first, or A, had its elementary couples of *equal* size; each of its zinc plates measured 10½ square inches, and each of its copper ones 168 square inches.

The second, or B, had its couples of *unequal* sizes, as seen in this table.

1st couple had	zinc =	1 square inch,	and copper =	16 square inches.
2nd ———	zinc =	2 square inches,	and copper =	32 ———
3rd ———	zinc =	4 ———	and copper =	64 ———
4th ———	zinc =	8 ———	and copper =	128 ———
5th ———	zinc =	16 ———	and copper =	256 ———
6th ———	zinc =	32 ———	and copper =	512 ———
	Total	63 square inches.		1008 square inches.

The third battery, or C, had its couples also *unequal*, but increasing from the first one by a different progression. I need only write out the dimensions of the zinc, each corresponding copper being sixteen times larger.

1st couple had	zinc =	8 square inches	copper
2nd ———	zinc =	9 ———	...
3rd ———	zinc =	10 ———	...
4th ———	zinc =	11 ———	...
5th ———	zinc =	12 ———	...
6th ———	zinc =	13 ———	...
	Total	63	1008 ditto.

The electricity developed by each of these batteries was passed in succession through a voltameter holding acidulated water, during times of 30 minutes each; when

A yielded of mixed gases, 1·3 cub. inch,
B yielded do. do. 0·7 cub. inch,
C yielded do. do. 1·1 cub. inch,

results which verify the conclusion drawn from the preceding experiments, and which prove besides that even when the difference in the sizes of the component circles of the battery is as trifling as in C, their inequality is attended too with a marked inferiority of action.

Since the above experiments were made I have contrived an apparatus by which such kinds of experiments, and many others of great importance to the science, may be conducted with the greatest possible precision and certainty. Its principle depends essentially on the substitution, in the place of amalgamated *plates*, of a *fluid* amalgam, formed by the union of 20 parts by weight of mercury and 1 of zinc. But as this paper has already exceeded any reasonable limits, I will not trespass on your patience with any description of its form or operation beyond that of merely stating that it appears to me to be admirably calculated for deciding many of the controverted points in the laws of this science; and one of such to which I shall attempt to apply it will be to determine the *real* relation that subsists between the deflexions of the magnetic needle and the quantity of zinc expended in producing those deflexions, or, which is the same thing, between the deflexions and the quantity of electricity which produces them.

Fourth Investigation.

Up to this step in the inquiry I have restricted myself to the use of diluted sulphuric acid as the exciting agent. The foregoing facts, or, it may be, laws of action, have therefore been determined for this one condition only out of the many under which the electrical action may be developed. The phænomena attendant upon this mode of excitation are, primarily, the decomposition of the water, the combination of its oxygen with the zinc, and the liberation of its hydrogen *as hydrogen*, that is, in its *gaseous state*, upon the surface of the conducting metal. Now, in the results of the first investigation we perceive that there exists a coincidence between the relative bulks (compared with equal weights) of oxygen and hydrogen gases and the relative surfaces of the two metallic plates ot any voltaic circle in which the maximum electrical effect takes place. This fact, which I speak of as a mere coincidence, should not be overlooked. It may indicate *à priori* that the same relative proportions in the two metals which have been determined for this one instance may be needed in every instance in which *water is decomposed and its hydrogen liberated in the form of gas.* But we have no reason to anticipate *à priori* that if any other substance than hydrogen be yielded at the conducting plate, we shall still, to produce the maximum effect, have to preserve the same proportions of the two metals as have been determined for that case. Suppose the operations within the cells of any battery to be so modified that, although water shall still be the substance decomposed, yet intead of the production of hydrogen we have the deposition of metallic copper, or the reformation of water, or the formation of some other body physically different from hydrogen as the results of the operations, what proportions of the two metals will then be needed to ensure the maximum effect? Such modifying agents are readily obtained. A proper mixture of the nitric and sulphuric acids with water will lead, not to the production of gaseous hydrogen, but to the formation of water and ammonia within the cells of the battery; and the solution of sulphate of copper to the deposition of metallic copper and the reformation of water. I will at this time examine but the latter of these two, and endeavour to determine what proportions should be observed between the two metals when the copper is immersed in a solution of its sulphate.

The experiments were conducted as in the first investigation. The zinc was immersed in dilute sulphuric acid, formed of 4½ acid to 100 water, and confined in a membranous bag, the copper solution surrounding it.

The zinc plate measured 2 square inches; it, as well as the acid and solution, was renewed each time of immersion. The first copper plate was equal in size to the zinc, then the copper plates were used of larger sizes in succession, as seen in the table.

Table No. 14.

1st,	zinc with copper, equal,	in 30 minutes	lost...	6·0	grains
2nd,	———————— twice its size	do.	lost...	7·0	—
3rd,	———————— 4 times do.	do.	lost...	9·0	—
4th,	———————— 8 times do.	do.	lost...	13·3	—
5th,	———————— 16 times do.	do.	lost...	11·5	—
6th,	———————— 20 times do.	do.	lost...	10·0	—

I am wholly unable to account for the deficiency in action observed in the fifth and sixth instances, when compared with the fourth, since the conditions of the experiments were in each instance precisely alike, save of course the size of the experimental copper plates.

This table, therefore, indicates that it is when the copper plate is about eight times larger than the zinc one, that the greatest effect ensues in the case in which sulphate of copper surrounds the conducting plate, and the voltaic action is accompanied by the deposition of metallic copper and the reformation of water.

These trials were varied twice; first by using a stronger acid solution to act upon the zinc; and secondly, by using merely water for the same purpose, when the results exhibited the operation of the same law, namely, that the maximum effect followed upon the employment of the copper plate about eight times larger than the zinc.

For reasons already sufficiently adverted to, I have confined my examinations to some of the cases most commonly met with in the ordinary employment of the battery. But were this kind of examination extended into every case of voltaic action in which different bodies, that is, bodies physically different from one another, are the result of the operations within the cells of the battery, I have no doubt that a specific and different proportion between the sizes of the two constituent metals would be found to be needed for each case, in order to obtain the maximum effect; and that the relations which would be thus discovered would be at once curious and of the utmost importance to the general theory of the science.

My own experiments have been extended to the examination of only two of such cases; namely, first to that in which sulphuric acid is the exciting agent, and *gaseous hydrogen* the product upon the surface of the conducting metal; and secondly, to that in which sulphuric acid (derived either from the dissolved sulphate, or from free acid previously added) is still the exciting agent, but in which *metallic copper* is the product appearing at the conducting plate; these two having been selected, from the highly distinctive character of their respective phænomena.

Recurring to the former of these investigations, I should wish, before concluding this paper, to attract your attention to a fact which has been invariably presented in their course, when the arrangement of any voltaic battery has been such that the whole of its evolved hydrogen could be collected, and the total quantity of zinc employed in its formation be also ascertained.

If a *single* voltaic arrangement be employed to generate hydrogen, an exact correspondence will be perceived between the quantity of zinc expended and the volume of hydrogen evolved, and their theoretical proportions as determined by calculation. The same regularity will be maintained whether plain or amalgamated plates of zinc be employed in the arrangement, and also whether single, or double, or treble arrangements, &c., connected, be brought into operation with this view; up to a certain number of such, if the hydrogen and zinc be compared, there will be found to be a perfect agreement between their quantities and those anticipated by theory. But if the number of voltaic pairs be so many that the battery possesses energy enough to decompose water when submitted to it in the usual way, then this exact correspondence between the proportions of the hydrogen and the zinc will be no longer maintained; but it will be found that the total quantity of zinc expended will be greater, by about one third, than is needed to generate the quantity of hydrogen actually evolved.

As I intend to enter upon a more minute examination of this phænomenon, I will, at this moment, offer but a single example of it, which is sufficiently marked and accurate to indicate its real nature.

A small battery of 16 equal pairs, arranged as the *couronne des tasses*, was put in operation; and so combined that the hydrogen from each cell could be collected and each zinc plate weighed at the end of a certain time. Each vessel was found to contain as nearly as possible the same measure of hydrogen; and a like equality of action upon the zinc had taken place throughout the series.

The total quantity of hydrogen was 60·6 cubic inches; the total quantity of zinc expended was 74·0 grains.

Now, I had previously determined that 34·5 grains of the zinc I employed were needed to produce an equivalent of hydrogen; and taking the weight of 100 cubic inches to be 2·1318 grains, we have the measure of an equivalent of hydrogen equal to 46·9 cubic inches. Therefore, the 74·0 grains of zinc here consumed should have yielded 100·5 cubic inches of the gas, but they actually yielded only 60·6 cubic inches.

Whether or not a voltameter were interposed the results were similar: invariably the same want of correspondence appeared between the loss of the zinc and the volume of evolved gas and their relative quantities as found by theory; and the amount of this difference was always somewhat similar to that exhibited in the above experiment.

The operations within the cells of the battery admit of easy analysis, and this phænomenon also admits perhaps of an easy explanation; but the fact it seems to indicate, viz. that a certain portion (and that about one third of the whole) of the hydrogen subserves some purpose closely connected with the power of the battery to decompose water, appears to be of so much importance as to make it deserving of a more careful examination. I remain, dear Sir,

Your obliged friend and pupil,

Newington, Edinburgh, March 28, 1837. C. Binks.

XV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. x. p. 382.]

April 6.—A paper was in part read, entitled, "Further Observations on Voltaic Combinations; in a letter addressed to Michael Faraday, Esq., D.C.L. F.R.S., Fullerian Professor of Chemistry in the Royal Institution, &c. &c." By John Frederick Daniell, Esq., F.R.S., Professor of Chemistry in King's College, London.*

April 13.—The reading of Professor Daniell's paper was resumed and concluded.

In the course of an inquiry into the effects of changes of temperature upon voltaic action, the author was led to observe some curious

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* Abstracts of Prof. Daniell's former papers have been given in Lond. and Edinb. Phil. Mag., vol. viii. p. 421; vol. ix. p. 376.

disturbances and divisions of the electric current produced by the battery, arising from secondary combinations; the results of which observations form the subject of the present paper. He found that the resistance to the passage of the current was diminished by dissolving the sulphate of copper which was in contact with the copper in the standard sulphuric acid, instead of water. The increased effect of the current, as measured by the voltameter, was farther augmented by the heat evolved during the mixture; and wishing to study the influence of temperature in modifying these effects, the author placed the cells of the battery in a tub, filled with hot water. On charging the cells with a solution of muriate of ammonia in the interior, and aqueous solution of sulphate of copper in the exterior compartment, he observed that a portion of the current is discharged by the water in which the apparatus is immersed; its passage being indicated by the disengagement of gas betwixt the adjacent cells; in which case, one of the zinc rods is thrown out of action, and the copper of that cell acts merely as an electrode to the antecedent zinc. A saturated solution of common salt was next placed in contact with the zinc, while the exterior compartments of the cells were filled with a saturated aqueous solution of sulphate of copper; but the effects were much diminished. It thus appeared that the substitution of solutions of the muriates for dilute sulphuric acid was in every way disadvantageous; and it was moreover found that, when the circuit was broken, the copper became seriously injured by their action, and by the formation of a submuriate of that metal.

Finding that the membranous tubes were unable to resist the action of the acid under the influence of high temperatures, the author substituted for them tubes of porous earthenware, of the same texture as that of which wine-coolers are commonly made, closed at their lower ends, and of the same height as the copper cells. The bottoms of the latter were fitted with sockets, for the reception of the tubes, and for confining them in their proper places; the perforated copper plates, or colanders, which held the solid sulphate of copper, passing over their upper ends. The tubes can be easily removed, and instantly replaced; and the facility of emptying and refilling them renders the addition of siphon-tubes unnecessary, except in very particular circumstances. A circular steam-vessel of tin plate was then provided, around which the cells could be placed upon blocks of wood, and closed in with a cover, containing a socket, which could, at pleasure, be connected with the steam pipe of a boiler. Two other sockets were also conveniently placed, provided with cork stoppers, through which the electrodes of the battery could pass, when the proper connexions were made. By using this apparatus the author determined that the increase of effect consequent on an augmentation of temperature is but in a slight degree dependent on an increase of conducting power in the electrolyte, but arises principally from its increased energy of affinity, producing a greater electromotive force.

In heating the battery by the steamer, it frequently happened that, when the thermometer had nearly reached the boiling point, and the action of the battery was at its maximum, a sudden cessation of its action would take place; and this suspension of power would continue for hours, provided the high temperature were maintained. On turning off the steam, and quickly cooling the apparatus, the action would return as suddenly as it had ceased, though, generally, not to the full amount. On closely examining the voltameter, on these occasions, it was found that the current was not wholly stopped; but that there existed a small residual current. This residual current was observed to be often directed in a course opposite to that which had before prevailed; and it was, in that case, the excess of a counter current, arising from a force which was acting in the contrary direction. The author found that variable currents might be produced, under ordinary circumstances, from the separate single cells of the battery when the whole series is connected by short wires. He proved by a series of experiments that the deoxidation of the oxide of copper by the hydrogen is not the exciting cause of the secondary currents; but that when the course of the main current of the battery is obstructed by causing it to pass through the long wire of a galvanometer, or through the electrolyte of a voltameter, the course of the secondary current from each separate cell is always normal, or in the same direction: when, on the other hand, the battery-current is allowed to flow with the least possible resistance, as by completing the main circuit by a short wire, the secondary current of the separate cells is in the opposite direction. Hence the resistance may be so adjusted as that the secondary current shall altogether disappear, or alternate between the two directions.

The remainder of the paper is occupied with the detail of experiments made with a view to ascertain the effects of different degrees of resistance to the voltaic currents under a great variety of circumstances.

April 20.—A paper was read in part, entitled, "Observations taken on the Western Coast of North America." By the late Mr. Douglas; with a report on his paper; by Major Edward Sabine, R.A., F.R.S. Communicated by the Right Honourable Lord Glenelg, one of His Majesty's Principal Secretaries of State, F.R.S., &c.

April 27.—The reading of Mr. Douglas and Major Sabine's paper, was resumed and concluded.

In the report prefixed to this paper, Major Sabine states, that Mr. Douglas was originally a gardener, and was, in the year 1833, recommended by Sir William Jackson Hooker to the late Mr. Joseph Sabine, who was then Secretary to the Horticultural Society of London, as a fit person to be employed by the Society in selecting and bringing to England a collection of plants from the United States of America. Having accomplished this mission to the complete satisfaction of his employers, he was next engaged on an expedition having similar objects with the former, but embracing a much larger field; namely, the tract of country extending from California to the highest latitude he might find it practicable to attain on the western side of the Rocky Mountains. Anxious to render to geographical and physical science all the services in his power, and to avail himself for that purpose of every opportunity which his visiting these hitherto imperfectly explored regions might afford him, he now endeavoured by diligent application to supply the deficiencies of his previous education. During the three months which preceded his departure from England, he studied with unremitting ardour and perseverance for no less than eighteen hours each day; and, conquering every difficulty, acquired a competent knowledge of the principles of science, learned the uses of various instruments, and made himself thoroughly master of the methods of taking observations both at sea and on land.

The narrative proceeds to notice the arrival of Mr. Douglas in America, the progress of his undertaking, the loss of his collections and most of his books and papers, by the upsetting and dashing to pieces of the canoe in which he attempted to pass the rapids, and, lastly, his death in 1833, at Owhyhee, in the Sandwich Islands, whither he had proceeded on his return to Europe.

The books which were preserved, and which have been received by Major Sabine, consist of several volumes of Lunar, Chronometrical, Magnetical, Meteorological and Geographical observations, together with a volume of field sketches. The geographical observations of latitude and longitude refer to two distinct tracts of country; first, the Columbia river, and its tributaries; and the district to the westward of them: and, secondly, California. Mr. Douglas very judiciously selected the junctions of rivers, and other well characterized natural points, as stations for geographical determination. The papers containing the details of his magnetical inquiries comprise records of observation of the dip, and of the intensity, at various stations both in North America and in the Sandwich Islands.

Analysis of the Roots of Equations. By the Rev. R. Murphy, M.A. Communicated by John William Lubbock, Esq., F.R.S.

The object of this memoir is to show how the constituent parts of the roots of algebraic equations may be determined by considering the conditions under which they vanish; and, conversely, to show the signification of each such constituent part.

The following are the propositions on which the author's investigations are founded.

1. In equations of degrees higher than the second, the same constituent part of the root is found in several places, governed by the same radical sign, but affected with the different corresponding roots of unity as multipliers.

2. The root of every equation, of which the coefficients are rational, contains a rational part; for the sum of the roots could not otherwise be rational. This rational part, as such, is insusceptible of change in the different roots of the same equation; consequently its value is the coefficient of the second term, with a changed sign, divided by the number of roots, or index of the first term.

3. The supposed evanescence of any of the other constituent parts, implies that a relation exists between the roots; and if such

a relation be expressed by equating a function of the roots to zero, that constituent part will be the product of all such functions, and a numerical factor.

4. The joint evanescence of various constituent parts, implies the coexistence of various relations between the roots, and that an interpretation may be given to each of the constituent parts, riveting the expression of the root in the memory, and converting the solution of a problem into a condensed enunciation of various theorems. The author exhibits the application of these principles to equations of various degrees, beginning with quadratic and cubic, and proceeding to those involving higher powers.

"On the first changes in the Ova of the Mammifera, in consequence of Impregnation; and of the mode of origin of the Chorion." By Thomas Wharton Jones, Esq. Communicated by Richard Owen, Esq., F.R.S.

The author having, in a former paper*, described the structure of the unimpregnated ovum of mammiferous animals, now proceeds to investigate the changes which the ovum undergoes in consequence of impregnation. In the rabbit, the first perceptible difference is the addition of a thick gelatinous matter surrounding the parts of which the ovum was composed in its original state, and apparently derived from the ovaries. In the progress of development the vitellary membrane gives way, as happens in the ova of the newt, and of many of the oviparous animals. The gelatinous envelope acquired in the ovary, and which is more especially circumscribed and defined after impregnation, constitutes the only covering of the vascular blastoderma, after the giving way of the vitellary membrane, and afterwards forms the chorion, which in rodent animals, at a further stage of development, presents itself under the form of a thin and transparent membrane, very similar to the vitellary membrane of a bird's egg, and situated immediately outside the non-vascular and reflected layer of the umbilical or erythroid vesicle. The author draws similar conclusions with regard to the development of the human ovum.

The second part of the paper relates to the changes taking place in the vitellus, the inferences concerning which are deduced chiefly from observations of the development of the ova of batrachian reptiles. The author concludes that the disappearance of the germinal vesicle is prior to impregnation. In the newt, the vesicle, at first imbedded in the substance of the yelk, gradually approaches the surface, until its situation is immediately underneath the vitellary membrane: its coat, having now become very soft, gives way, allowing the contained fluid to be effused on the surrounding surface of the yelk; and the small depression in which the vesicle was lodged now forms the cicatricula. The effused fluid gives a degree of consistence to the matter composing the surface of the yelk, and thus promotes the formation of the blastoderma. In the frog, the surface of the yelk becomes every day more and more broken up, and the resulting crystalline forms described by Prevost and Dumas be-

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* See Lond. and Edinb. Phil. Mag., vol. vii. p. 209.

come smaller and smaller, until the surface of the black blastoderma appears under a magnifying glass like shagreen. The blastoderma, consisting of an aggregation of clear globules, different from those of the rest of the yelk, is now fully formed, and has extended itself so as to close in the white spot. The change which takes place in the yelk of the bird's egg appears to be limited to the neighbourhood of the cicatricula.

May 4th.—On the adaptation of different modes of illuminating Light-houses, as depending on their situations and the object contemplated in their erection. By William Henry Barlow, Esq., in a Letter addressed to Peter Barlow, Esq., F.R.S., and communicated by him.

The letter of Mr. W. H. Barlow, addressed to his father, in which the paper is contained, is dated Constantinople, March 14th, 1837, and states that the experiments which he made with the Drummond light, and other means of illuminating Light-houses, and of which he now communicates the results, were undertaken at the request of the Turkish Government, with the view of placing lights at the entrance of the Bosphorus from the Black Sea *. The object of his inquiry is to investigate the principles on which the illuminating power, resulting from the employment of reflectors, and of lenses, depends; and the most advantageous application of that power to the purposes of Light-houses.

In discussing the relation which exists between the illuminating power and the intensity of an artificial light, he observes that the former is proportional to the quantity of light projected on a given surface at a given distance; and that the latter is dependent on the quantity of light projected by a given area of the luminous body on a given surface at a given distance. Hence the intensity of a light multiplied into its surface is the measure of the illuminating power, whether the light proceed from one or from several luminous bodies: and the illuminating power is equal to that of a sphere of light, whose intensity and apparent surface are equal to that of the light itself at any given mean distance.

Within a certain limit of distance, the property of light which produces the strongest impression on the eye, is its intensity; but when the light is so remote that the angle subtended by it at the eye is very minute, as is generally the case in Light-houses, the intensity of the impression made on the retina is proportional only to the illuminating power. The mathematical investigations of the author lead him to the conclusion that all reflectors and lenses of the same diameter have the same illuminating power when illuminated by the same lamp; and that by diminishing the focal distance, and intercepting more rays, the illuminating power is not increased, but simply the divergence, and consequently the surface or space over which it acts. The author then proceeds to inquire into the comparative utility of lenses and reflectors, and arrives at

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* In Lond. and Edinb. Phil. Mag., vol. viii. p. 238. will be found a letter from Mr. W. H. Barlow, describing some of his preliminary trials at Constantinople.

the inference that the advantage gained by the employment of the former does not arise from their superior perfection as optical instruments, but from their using the light more economically, in consequence of their producing less divergence of the rays, both horizontally and vertically, and illuminating a much smaller space in the horizon. Rules are then deduced for the application of lenses and reflectors in Light-houses, according to the particular situations in which they are placed, and the purposes they are intended to serve. With this view, the author divides Light-houses into three classes: the first comprising Beacon or Warning Lights, placed in order to prevent the approach of vessels, and which consequently can never be nearer than three or four miles; the second being Guiding or Leading Lights, placed to guide a vessel, and therefore admitting of a very near approach; and the third including those which, according to the respective directions in which they are seen, have both these duties to fulfil. In the first we require great illuminating power, and a long duration of the brightest period, with a small angle of vertical divergence; in the second, less illuminating power, but a larger angle of vertical divergence are requisite, while the duration of the extreme brightness is of minor importance; and in the third, all these properties, namely, great illuminating power, a long duration of the brightest period, and a large angle of vertical divergence, are necessary.

May 11.—A paper was in part read, entitled, "On the connexion between the Phænomena of the absorption of Light and the Colours of thin Plates." By Sir David Brewster, K.H., F.R.S.

The Society then adjourned over the Whitsun week, to meet again on the 25th instant.

May 25.—Sir David Brewster's paper was resumed and concluded.

The phænomena of the absorption of light by coloured media have been regarded by modern philosophers as inexplicable on the theory of the colours of thin plates, and therefore irreconcileable with the Newtonian hypothesis, that the colours of natural bodies are dependent on the same causes as the colours of thin plates. The discovery by Mr. Horner of a peculiar nacreous substance possessing remarkable optical properties, of which the author has already given an account*, furnished him with the means of instituting a more accurate comparison between these two classes of phænomena. By a careful and minute analysis of the reflected tints of its three first orders of colours exhibited by a single film of the above mentioned substance, they were found to consist of that part of the spectrum which gives the predominating colour of the tint mixed with the rays on each side of it. In analysing the transmitted beam, bands of the colours complementary to the former are seen, with intervening dark bands; and when the analysis is made with a high magnifying power, the spectrum is observed to be crossed throughout its whole extent with alternate dark and coloured bands, increasing in number and diminishing in magnitude with the thickness of its plate. In the phæno-

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* See Lond. and Edinb. Phil. Mag., vol. x. p. 201.

mena of periodical colours there are three peculiarities demanding notice; first, that the dark lines change their places by varying the inclination of the plate; secondly, that two or more lines never coalesce into one; and thirdly, that the colour of the luminous bands in the complementary spectrum are the same as those of the original spectrum when the thin plate is perfectly colourless. The author institutes a comparison of these phænomena with those of absorption as exhibited by a solid, a fluid, and a gaseous body; employing as an example of the first, smalt blue glass; of the second, the green sap of vegetables; and of the third, nitrous acid gas. No connecting link between these phænomena appeared to exist, excepting that both exhibited a divided or mutilated spectrum; but even this common fact has not the same character in both. The nacreous substance described by Mr. Horner, however, in some cases, when the plates were small, was found to produce bands perfectly identical with those of thin plates; while in other cases the bands were exactly similar to those of coloured media. By employing the iridescent films of decomposed glass, the author obtained combinations of films which gave, by transmitted light, the most rich and splendid colours, surpassing every thing he had previously seen among the colours either of nature or of art. These facts have proved that the transmitted colours, though wholly unlike those of thin plates, are yet produced by the same causes, and are residuary, and generally complementary to the sum of the reflected tints. Thus the author has succeeded in completely identifying in their primary features the two classes of facts; the one resulting from absorption, the other from periodic action. The minor points of difference, namely, the uniformity of the bands and tints of absorbing media at all incidences, and the non-appearance of the reflected tints in such media, are endeavoured to be explained by the introduction of several considerations, the complete discussion of which the author reserves for the subject of a future paper. From the phænomena of thin plates, of polarized tints, and of absorption, the existence of a new property of light is deduced, in virtue of which the reflecting force selects out of differently coloured rays of the same refrangibility rays of a particular colour, allowing the others to pass into the transmitted ray; a principle not provided for in either of the theories of light to which the phænomena of absorption are ultimately referable, and furnishing an explanation of certain remarkable phænomena of dichroism in doubly refracting bodies, in which rays of the same refrangibility, but of different colours, pass into the ordinary and extraordinary pencils.

A paper was read "On the hereditary instinctive propensities of Animals." By Thomas Andrew Knight, Esq., F.R.S.

The author adduces, in support of the principle he had advanced in his paper on the economy of bees*, namely, that instinctive propensities to the performance of certain actions are transmitted, independently of education, from the parent to its offspring, several facts which have fallen under his observation in the course of various experiments commenced by him nearly sixty years ago and continued

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* See Phil. Mag. and Annals, vol. iv. pp. 59, 60.

to the present time. He relates that a young terrier, whose parents had been trained to destroy pole-cats, and a young springing spaniel, whose ancestors through many generations had been employed in finding woodcocks, were reared together as companions; and that each of them, immediately on seeing, and for the first time in its life, the particular prey to which it was guided by hereditary instinct, pursued it with intense eagerness, while it did not appear to notice that which attracted its companion. In several instances he found that young springing spaniels, wholly inexperienced, were very nearly as expert in finding woodcocks as their well-trained parents. The habits of the woodcock have in the course of the last sixty years undergone considerable change, the fear of man having during that period become much stronger by transmission through many successive generations. The author believes that by continued education these hereditary propensities might be suppressed and others substituted: thus the habits of the springing spaniel would never have been acquired, if shooting on the wing had not been practised by man. A young dog, of the variety usually called *retrievers*, on account of their being trained to find and recover wounded game, performed this office, although wholly untaught, quite as well as the best instructed dog. The male and the female parents appear to possess similar powers of transmitting to their offspring these hereditary feelings and propensities; excepting in the case of hybrid progeny, in which the author thinks he has witnessed a decided prevalence of the character of the male parent. With regard to dogs, the influence of one or other of the parents, and sometimes of both, may occasionally be traced, but without any constancy as to the particular predominance of either sex.

A paper was read "On Meteorological deductions from Observations made at the Observatory at Port Louis in the Mauritius, during the years 1833, 1834, and 1835." By John Augustus Lloyd, Esq., Surveyor-General of that Island, F.R.S. Communicated by Captain Beaufort, R.N., Hydrographer to the Admiralty, F.R.S.

The observations, from which the results recorded in the present paper were made, are nearly 50,000 in number, and were taken four times each day, at the hours of 8 A.M. noon, 4 and 8 P.M. The details of the observations themselves are about to be forwarded to the Royal Society; they relate to the states of the barometer, thermometer, hygrometer, rain guage, and the appearance of the atmosphere with regard to clearness or cloudiness.

June 1.—A paper was read, entitled, "On the development and extinction of regular doubly refracting structures in the crystalline lenses of animals after death." By Sir David Brewster, K.H., L.L.D., F.R.S.

Since the year 1816, when the author communicated to the Royal Society an account of the doubly refracting structures which exist in the crystalline lenses of fishes and other animals, he has examined a great variety of recent lenses with the view of ascertaining the origin of these structures, the order of their succession in different lenses, and the purpose which they answer in the animal economy. He had discovered in the lenses of many fishes the alternation of portions, exerting, the one a positive, and the other a negative refractive action; but in his subsequent investigations he met with the greatest discrepancy as to the regularity of their arrangement. He found that in quadrupeds the central structure is positive; while in fishes, where there are three structures, it is always negative; but their positive structure in the former case sometimes exists alone, with faint traces of a negative structure, and sometimes it is followed by another positive structure separated from the first by a black neutral circle, in which the double refraction disappears; at other times various other combinations of these structures are presented. Occasionally, in the dark neutral line which separated two positive structures, he perceived a trace of an intervening structure, which seemed to be either about to disappear or about to be developed. This conjecture was satisfactorily verified by a series of observations which he made on the lenses of the sheep, the ox, and the horse, at different ages, and also on the same lens, during the spontaneous changes it undergoes when kept in distilled water. The negative structure was in these experiments gradually developed at the space intervening between the portions of the lens which had possessed the positive structure; and thus the same parts assumed in succession doubly refractive actions of opposite kinds. The author intimates his intention of pointing out, in a separate paper, the conclusions deducible from these facts respecting the cause and cure of cataract.

GEOLOGICAL SOCIETY.

January 4.—A paper entitled, "Some Observations on the Elevation of the Strata on the Coast of Chili," by Alexander Caldcleugh, Esq., F.G.S., &c., was first read.

The author commences by stating, that previously to his return to South America several circumstances induced him to suspend his opinion as to the correctness of the details which had been published of the effects of the earthquake of 1822. He thought that the wreck in the Bay of Valparaiso, which had become accessible after the earthquake, might have been thrown higher up by the heavy rollers, and that rocks covered with testacea, which after the event were laid dry, might likewise have been drifted. He also thought that the accuracy of the observations might be doubted, because certain genera of shells were stated to have been found adhering to the rocks in Valparaiso Bay, which it was well known did not exist there; and because there was a vagueness in some of the statements,—as the raising of the whole country from the foot of the Andes to far out at sea. Since his return to South America, however, especially since the earthquake of 1835, Mr. Caldcleugh has investigated the evidences of change of level on the Chilian coast, and he states, in this communication, his full conviction, that there have been many distinct alterations in the relative level of land and sea.

In detailing the results of his researches, he gives extracts from all the historical and documentary evidence which he has been able to consult, separating those proofs of accession of land which may be ascribed to the deposition of sedimentary matter, from those which demonstrate a vertical movement.

1. *Accumulations of sedimentary matter.* The Abate Molina states, that the sea had long been gradually retiring, and even hazards the observation, that in some places it had receded two, in others, especially near the mouth of rivers, six inches annually. Near the village Talcahuano, Frezier says, that in 1712 the depth of water near the shore was from five to six fathoms; while Ulloa in 1744 gives five fathoms, and Capt. Fitzroy has found the soundings near the same spot to be only three and four fathoms. Frezier also states, that in Valparaiso Bay vessels anchored close to the shore in eight or ten fathoms; and Ulloa, in fourteen fathoms at the distance of a cable and a half; though at present the soundings at the same distance are five to seven fathoms. In 1778, a Spanish seventy-four moored within a cable's length of the arsenal in fifty fathoms water, where there are now but six.

In the port of Coquimbo, according to Frezier, at half a cable's length from the Tortoise Rock, the soundings were from six to ten fathoms: they are now from three to four: it is also said that a whale once passed between the rock and the main land. In the port of Huasco, the same author gives the depth very near the shore to be eighteen and twenty fathoms; but a vessel (the Byron) in 1831, touched upon the ballast she had thrown out.

At Mr. Caldcleugh's first visit to Valparaiso in 1821, the town consisted of one narrow causeway with houses on one side, and part of the road which wound round an old fort, was washed by the sea at every high tide. On a late visit to the port, he found that two lines of houses had been built to seaward of the original street, and that vessels of three or four hundred tons were obliged to anchor much further out.

With respect to the dependence which ought to be placed on the preceding observations, he says, that the facilities for making them were great, as vessels, to avoid the gusts from the gulleys, almost always anchor on the same spot; and he remarks in explanation of this great accumulation of detritus, that the rivers of Chili partake, with few exceptions, of the nature of mountain torrents, swollen in winter by the rains, and in summer by the melting of the snow in the great Cordillera.

2. *Evidences of vertical movements.*—The author then proceeds to point out the evidences of change between land and sea, which cannot be accounted for by the accumulation of loose materials.

In Concepcion Bay there are two or three rocks which are not noticed by Frezier or Ulloa, but of which seamen are now warned; and the Belem rock has at present two fathoms upon it at low water, while in the chart of Ulloa it is not laid down.

In the Bay of Valparaiso, outside of the inner point near the Quebrada de los Angeles, at a cable and a half or two cables from the shore, is a rock which Ulloa directs to be looked out for with care as it was not visible; at present it is not above 100 fathoms from the shore; and there is a ripple upon it at all times of the tide. Other rocks near the Cruz de Reyes, which in 1821 were always covered by the sea, are now four feet above the level of high water mark.

In the port of Coquimbo are three rocks called the Pelicans, which rise twelve feet above low tide; in 1710, according to Feuillée, they were *à fleur d'eau.* Another rock, about twelve feet long, called the Tortoise, in the time of Frezier and Feuillée, rose five or six feet out of the water; it is now nine feet above high water mark.

Mr. Caldcleugh then alludes to the beds of recent shells which rest upon the edges of the sea cliffs from Concepcion to Copiapo, and in consequence of their being found at levels varying from 14 to 300 feet, he conceives that the coast has been raised *per gradus.*

He next states, in addition to the evidences afforded by Mrs. Calcot of the change of level produced by the earthquake of 1822, that persons who escaped on board vessels, remarked that the sentries before an old fort on the summit of the hill over the ruins of the town, and previously visible from the feet upwards, had, subsequently to the event, half the body concealed by the fore part of the cliffs*. He also says that the street or causeway, which wound round an old fort, and in 1821 was washed by the sea at every high tide, is now seven feet above the wash of the sea at the high water line of ordinary tides.

In conclusion, Mr. Caldcleugh gave an account of the effects produced by the earthquake of 1835, chiefly from the observations of Capt. Fitzroy, full details of which have been published in the Transactions of the Royal Society † and the Royal Geographical Society ‡.

The President then announced that he had received from the Foreign Office the translation of an article published in the South American Journal, El Araucano, by Don Mariano Rivero; but as none except original communications were read before the Society, he could only state that Don Mariano dissents entirely from the belief, that earthquakes have produced vertical changes of level on the coast of Chili.

This communication was accompanied by a letter from Col. Walpole addressed to Lord Palmerston, an extract from which, read by the Secretary, strongly supported Don Mariano Rivero's views.

A paper entitled "Observations of proofs of recent elevation on the coast of Chili, made during the survey of His Majesty's ship

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* In page 102 a part of the fort previously invisible is stated to have become visible; but this apparent discrepancy arises from the observations alluded to by Mr. Caldcleugh having been made from the shipping, and those by Mr. Darwin from a point on the land.

† Mr. Caldcleugh on the Great Earthquake in Chili, 1835. Phil. Trans. 1836, p. 21, noticed in Lond. and Edinb. Phil. Mag., vol. viii. p. 148.

‡ Sketch of the surveying Voyage of His Majesty's Ship Beagle. Journal of the Royal Geographical Society, vol. vi. p. 319.

Beagle, commanded by Capt. Fitzroy, R.N.," by Charles Darwin, Esq., F.G.S., was afterwards read.

The subject of recent elevations on the coast of Chili being, in the opinion of many, still open to discussion, Mr. Darwin gives, in this memoir, the results of his own observations. The portion of the coast, more particularly examined by the author, extends from the river Rapel, about sixty miles south of Valparaiso, to Conchali, about eighty miles north of it.

Close to the mouth of the Rapel, dead barnacles occur adhering to rocks three or four feet above the highest tidal level; and in the neighbouring country recent marine shells are scattered abundantly to the height of about 100 feet. Ten miles to the north, and at an equal distance from the sea, is the village of Bucalemu, in the neighbourhood of which are very extensive beds of recent shells. At the bottom of the great valley of Maypo, and some miles from the coast, marine shells of existing species are also numerous; and at St. Antonio, near the northern point of that river, are large quarries of shells. Between this point and Valparaiso in the ravine Quebrada Onda, the remains of a species of shell common on the coast, were noticed by the author. Along the bold granitic coast south of the promontory which forms the bay of Valparaiso, are numerous level and horizontal beds of shells, constituting an almost continuous band, elevated from 60 to 230 feet above the level of the sea. The shells are brittle, but of various kinds, and are all similar and in similar proportional numbers to those on the beach. They are mingled with some earth, though packed closely together, and overlie a partially consolidated breccia of granitic fragments which rests on the solid rock. After a careful examination of these deposits, first by himself, and afterwards with Mr. Alison, guarded by a recent inspection of the heaps of shells accumulated by the natives in Tierra del Fuego, Mr. Darwin was convinced that the shelly beds near Valparaiso were formed when the sea occupied a different level. The following are the principal circumstances which lead to this conviction. The great number of the shells forming extensive horizontal beds, whereas the heaps in Tierra del Fuego collected by the inhabitants, always retain a conical figure: their position, at the extremities of headlands inaccessible from the sea, and unfit for strongholds, being without fresh water: the large proportional number of extremely small shells: and lastly, their brittle and decayed condition, the state of decomposition having an evident relation to the comparative heights at which the shells were lying. Comminuted shells were noticed by Mr. Darwin at the heights of 560 and 1300 feet, but the evidence of their having been part of a beach was not convincing.

At San Lorenzo in the bay of Callao, Mr. Darwin traced a similar process of decay from perfect shells in the lowest beds to a mere layer of calcareous powder in the highest. This phænomenon, he adds, can be observed only in countries where rain never falls.

On the north side of the bay of Valparaiso, near the Viña del Mar, is an abundance of elevated shells. Mr. Alison, by climbing a point of rock about fourteen feet above high water, and removing the dung of sea fowls, discovered Balani adhering to the stone.

With respect to the historical evidence of the earthquake of 1822, Mr. Darwin says that he met with no intelligent person who doubted the rise of the land, or with any of the lower order who doubted that the sea had fallen. He mentions also the altered position of the wreck and of the rock in the bay; and from a part of the fort being invisible from a point on the land before the earthquake, but visible afterwards, he infers that the movement of the land was unequal*. A further proof of change, obtained for the author by Mr. Alison, is shown by the remains of a sea-wall built in 1680, and over which, up to 1817, the sea broke during the northerly gales. Mr. John Martin, a ship carpenter of Valparaiso, remembers walking in 1819 on the beach at the foot of this wall, and he has been frequently obliged to climb up to the street to avoid the sea. This wall is now separated from the bay by two rows of houses, but a portion of what appeared to be its base, carefully levelled by a resident engineer, was found to be 11 feet 6 inches above high water mark. Mr. Darwin does not ascribe the whole of this change to the earthquake of 1822, and is of opinion that the alteration then produced was under three feet. The church of San Augustin is believed to have been built in 1634, and the base of its walls is 19 feet 6 inches above high tide level; but there is a tradition that the sea formerly approached very close to its foundations. Allowing, therefore, 4 feet 6 inches for its protection when built, the amount of change in 220 years is only 15 feet. The granite rocks which form the coast are also water-worn and hollowed at about the same height, namely, 14 feet above the present sea level. These data, Mr. Darwin is of opinion, prove, that though the changes in 220 years have been small, yet that they were preceded by a period of comparative rest, during which there was time for any former marks on the rocks to become obliterated.

The author then described the beds of recent shells between Concon and Quintero, about 100 feet above the sea level; the deposits near Plazilla and Catapilco; and in the valley of Longotomo. On the hills to the north of the latter, about 200 feet above the sea, immense quantities of recent shells coat the surface or the sides of the ravines; and hence Mr. Darwin infers that the action of the sea determined the minor inequalities of the land. Similar deposits, more or less abounding in shells, were noticed by him near Guachen, and in the valley of Quilimap. Close to Conchali, on the south side of the bay, are two very distinct terrace-like plains, the lower being about sixty feet high.

Mr. Darwin then gave a very brief notice respecting the marine origin of the terraces at Coquimbo, described by Capt. Basil Hall and discussed by Mr. Lyell. The proofs of the origin assigned to them rest on the occurrence of recent shells in a friable calcareous rock elevated 250 feet above the sea. This calcareous stratum passes downwards into a shelly mass chiefly composed of fragments of Bala-

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* See note * in page 100.

nidæ, and this again overlies a sandstone abounding with silicified bones of gigantic sharks mingled with extinct species of oysters and Pernæ of a great size. The intermediate bed contains some shells in common with the upper, in which all are recent, and with the lowest, in which the greater number are extinct. The phænomena of the parallel terraces and the elevated shells occur in a strongly marked manner in the villages of Guasco and Copiapo, the latter being 350 miles to the north of Valparaiso: recent shells also occur at different elevations at an equal distance to the south of it at Concepcion and Imperial. Mr. Darwin believes that the land on the coast of Chili has risen, though insensibly, since 1822. In the island of Chiloe he is fully convinced, from oral testimony and the state of the coast, that a change effected imperceptibly is now in progress. In support of this gradual rise, independent of earthquakes, he states, that the eastern coast of South America, bordering the Atlantic from the Rio Plata to the Strait of Magellan, presents terraces containing recent shells; yet in the provinces near the mouth of the Plata, earthquakes are never experienced; and it is impossible to suppose that the most violent of the Chilian earthquakes could produce these effects, as the shocks are scarcely transmitted to the plains at the western foot of the Cordilleras. Hence, he concludes that the earthquakes, volcanic eruptions, and sudden elevations on the coast line of the Pacific, ought to be considered as irregularities of action in some more widely extended phænomenon.

January 18.—A paper entitled, "An Account of a deposit containing land shells at Gore Cliff, Isle of Wight," by J. S. Bowerbank, Esq., F.G.S., was first read.

During a recent examination of the greensand of Gore Cliff, Mr. Bowerbank discovered on the top of the cliff and overlying the chalk marl by which it is capped, a bed consisting of detritus of chalk and chalk marl, and inclosing, in every part examined by him, numerous specimens of existing species of land shells. The deposit extends from nearly the edge of the cliff to the foot of St. Catherine's Down, a distance of about 660 yards. The range of the deposit he could not ascertain, as at a short distance from the spot examined the cliff assumed its usual perpendicular form; but he is of opinion that it is considerable, or else that there are many such deposits, as he found fallen masses of a similar bed near St. Lawrence and between Ventnor and Bonchurch.

A letter addressed to Dr. Buckland by J. Wyatt, Esq., respecting a trap dyke in the Penrhyn Slate Quarries near Bangor, Carmarthenshire, was then read.

These quarries were opened fifty years since, and the excavation is now about 700 yards in length, 300 in breadth, and 90 below the natural surface. In carrying on the highest opening of the quarry, the men, a few months since, came suddenly in contact with a trap dyke, which has since been cut through and proved to be 11 feet in width. Its direction appears to be between W.N.W. and N.W., and it intersects the bedding of the slate nearly at right angles. The dip at present is almost 90°, the slight inclination being to the N.E. The "cheeks" of the dyke on the N.W. side, are broken conformably with the natural joints of the schist. The slate immediately in contact with the trap is, in some parts, quite flinty, having lost its fissile properties, and the colour is changed from purple to black; but at the distance of two or three feet the slate recovers its natural colour and cleavage.

A notice of a successful attempt at boring for water at Mortlake in Surrey, by William Richardson, Esq., F.G.S., was next read, an abstract of which appears in No. 48 of the Society's Proceedings.

A paper "On the Strata usually termed Plastic Clay," by John Morris, Esq., and communicated by the President, was then read.

The author commences by objecting to an arrangement of tertiary formations in different countries according to the classification of the Paris basin by MM. Cuvier and Brongniart. He then refers to the memoirs of Mr. Webster, Dr. Buckland, and Mr. Richardson, on the strata immediately above the chalk in England, and proceeds to show that they ought to be considered as belonging to the London clay.

For the sake of convenience in arranging their organic remains, Mr. Morris makes three divisions of these beds: 1. those containing the Reading oyster; 2. the Woolwich and Upnor strata; and 3. the Bognor or lower arenaceous beds of the London clay.

The first division has been long well known in consequence of the sections given by Mr. Webster* and Dr. Buckland† of the Catsgrove pits near Reading; and has been subsequently described in a paper by Mr. Rofe on the same district‡. Mr. Morris gives the following sections of this deposit at Northaw in Hertfordshire, and Headley in Surrey.

		Feet.
Northaw, Top,	grey sand	20
	green sand with oysters	1
	grey sand	2
	iron flint bed	8 inches.
	chalk	

		Feet.
Headley, Top,	red and green variegated marls	4
	clay and sand	3
	grey sand with oysters	1
	ash-coloured sand	
	chalk	

2. The Woolwich strata, also described by Mr. Webster§ and Dr. Buckland¶, extend along the south side of the Thames, and patches of them are said to occur near Stifford** and Plaistow in Essex. They are chiefly composed of sand, clay, pebbles, and calcareous rock, the strata varying greatly in their thickness in different parts of even the same pit; and in their order of succession in different localities. At Woolwich, Sundridge park, Upnor, and some other places, there is also a marked distinction in the fossils of the upper beds from those of the lower; the former being characterized by the prevalence of freshwater or estuary shells of the genera Cyrena, Neritina, Melanopsis

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* Geol. Trans., First Series, vol. ii. p. 198.

† Ibid. vol. iv. p. 278.

‡ Geol. Proceedings, vol. ii. p. 72, or Lond. and Edinb. Phil. Mag. vol. v. p. 212.

§ Geol. Trans., First Series, vol. ii. pp. 196—221.

¶ Ibid. vol. iv. p. 284.

** Ibid. vol. ii. p. 196.

and Planorbis, associated with marine shells. How far this distinction may be owing to the action of a river, Mr. Morris says it may be difficult to determine, as the recent species of the above genera have variable habits. The principal localities at which the Woolwich beds have been noticed are, Sundridge park near Bromley, Chiselhurst, Orpington, Beckenham, Sydenham, Counter Hill, Loam-pit hill near Lewisham, the Thames Tunnel, Vauxhall, the road leading from Oldfield to Plumstead; the ninth milestone beyond Shooter's Hill, Erith ballast-pit, Bexley Heath, Swanscombe Wood, Green Street near Stoke, and Upnor near Rochester.

The following sections presented at the last locality, illustrate the variations in different parts of the same pit.

	Feet.
South End, Top, brown clay with fragments of chalk and chalk flints	10
fine calcareous clay	3
sand with bluish flint pebbles, and numerous remains of Cyrena, Cerithium, Planorbis, Cytherea, Pectunculus, Cardium, and Natica	1
white sand, with occasionally layers of shells	4
blue and brown clay with compressed Cyrena, Cerithium and Ostrea	4
lignite in gray sand	1
white sand, lower part containing pebbles	20
North End, Top, brown clay, &c.	14
sand with pebbles and shells	1½
sand chiefly white, in the upper part thin seams of shells	15
brown and blue clay, with compressed shells intermingled with sand	6
purplish sandy clay with ochreous concretions	2
lignite in sulphur-coloured clay and sand	8 inches.
white sand	

With respect to the geological position of the Woolwich beds, Mr. Morris is of opinion that they ought to be assigned to the lower part of the London clay, as they occupy the same position, with reference to the chalk, as the Bognor strata.

3. The Bognor or lower arenaceous beds of the London clay are then described, and the following list given of the localities at which they have been noticed: Pegwell Bay (Isle of Thanet): Herne Bay[*], Faversham, Hampstead well[†], Watford, Egham, Bray, Binfield[‡], Catsgrove quarry near Reading[§], Alum Bay, Bognor[¶] and Stubbington. At all these localities the strata are considered by Mr. Morris to be contemporaneous and to belong to the lower part of the London

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[*] See Abstract of Mr. Richardson's Memoir, Geol. Proceedings, vol. ii. p. 78. or L. and E. Phil. Mag. vol. v. p. 219.

[†] Abstract of Mr. Wetherell's Memoir, Geol. Proceedings, vol. ii. p. 93, or L. and E. Phil. Mag. vol. v. p. 295.

[‡] Mr. Warburton's Memoir, Geol. Trans., Second Series, vol. i. p. 52.

[§] Abstract of Mr. Rofe's paper, Geol. Proceedings, vol. ii. p. 72, or L. and E. Phil. Mag. vol. v. p. 212.

[¶] Mr. Webster's paper, Geol. Trans., First Series, vol. ii. p. 190.

clay, reposing in some instances immediately on the chalk. Their mineral character is remarkably persistent, consisting of grayish green sands with layers of pebbles, but sometimes passing into sandstone and limestone. The fossils, which in some localities are numerous, agree specifically with the well-known Bognor shells; those imbedded in the sands being in good preservation but those in the rock chalky and friable. From the position occupied by these beds Mr. Morris is induced to consider them as the equivalents of the lower beds of Woolwich, Upnor, &c., and in support of this opinion alludes to the strata of calcaire grossier which rest immediately upon chalk at Meudon.

A Memoir on the Geology of Suffolk, by the Rev. W. B. Clarke, F.G.S., was then commenced.

Feb. 1.—A paper, "On the occurrence of Keuper-Sandstone in the upper region of the New Red Sandstone formation or *Poikilitic system* in England and Wales," by Professor Buckland, D.D., V.P.G.S., was first read.

The term Keuper is applied in Germany to the entire series of red and variegated marls and sandstones, which lie between the lias and muschel-kalk. Several of these sandstone beds afford a valuable building stone, specimens of which from the quarries of Stuttgard and of Sinzheim, near Heidelberg, were presented by the author to the Society. Dr. Buckland has identified several varieties of this German Keuper-sandstone with sandstones which occupy an analogous position in England in the lower region of the red rock marl.

The total absence of the Muschel-kalk in this country, has left us without that obvious division which it affords in Germany, between sandstones of the era of the red marl or Keuper, and those more ancient new red sandstones which are distinguished on the Continent by the name of Grès bigarré, or Grès de Vosges, and which in England, occupy a large space between the red rock marl and magnesian limestone.

In the vicinity of Warwick, which forms the principal example cited in the present memoir, an excellent section is seen in Guy's Cliff, and a considerable extent of surface is occupied by Keuper-sandstone, which emerges from beneath the red rock marl, near that town and Leamington, and occupies a breadth of three or four miles between Warwick and Kenilworth; at the latter place the Vosges sandstone rises from beneath it, affording the materials for the construction of the castle of Kenilworth, as the Keuper-sandstone has afforded those of the castle and other ancient buildings at Warwick.

In the absence of the Muschel-kalk, there is here no obvious proof of any interval between the deposition of the new red sandstone of Kenilworth, and of the olive-coloured Keuper-sandstone which rests immediately upon it, and although the mineral condition of this latter sandstone agrees with that of the Keuper-sandstone of Germany, some doubt might have remained as to the identity of strata so distant from each other, without the aid afforded by organic remains. In 1823, part of the jaw and other bones of a Saurian, found in the sandstone of Guy's Cliff near Warwick, were presented to the Oxford Museum, by the late Butic Greathead, Esq.; Dr. Buckland has identified these with the remains of the Phytosaurus, which in 1835 he saw in the Museum at Wirtemberg; and, as this genus has hitherto been found in no other formation than the Keuper, it leaves little doubt as to the identity of the Warwick sandstone with the Keuper-sandstone of Germany. Fragments of vegetables also are dispersed through the Warwick sandstone, in the same state of imperfect preservation as the greater part of those in the Keuper of Stuttgard.

In October 1836, further remains, apparently of Phytosaurus, were found at Warwick by Dr. Lloyd of Leamington, who is engaged in tracing the extent of the Keuper throughout this district.

Dr. Buckland has also recognised the Keuper-sandstone in the quarries of Sutton Mallet near Bridgewater; and of Rumwell Heale and Oake near Taunton; the latter have supplied the freestone used in the towers and bridges at the town of Taunton.

In the cliffs at Oreham, two miles E. of Exmouth, there are beds of sandstone, probably referrible to the Keuper formation, which have supplied the olive-coloured sandstone of which the cathedral of Exeter is built; and at Pyle, in Glamorganshire, a few miles E. of Neath, a valuable building-stone lately employed in constructing the castle of Margum, is obtained from strata, which the author also refers to the Keuper-sandstone of Germany.

Mr. Murchison has noticed Keuper-sandstone in several localities in Gloucestershire and Worcestershire, near Tewkesbury and Newent.

A paper "On the Geological Structure and Phænomena of the northern part of the Cotentin, and particularly of the immediate vicinity of Cherbourg," by the Rev. W. B. Clarke, F.G.S., was then read.

In an account of the Cotentin published in the 35th volume of the Journal des Mines, M. Alexander Brongniart gives a full account of the limestone, slates, quartz rock and syenite or granite composing the country; but the chief object of that memoir is to prove the comparatively recent origin of the granitic rocks.

Mr. Clarke, in detailing the characters of the formations follows closely the account of M. Brongniart, and adopts fully the views of that geologist respecting the age of the granite. He also quotes Prof. Sedgwick's observations on the comparatively recent origin of the granite of Cornwall, and the adjacent portion of Devonshire; and points out the general agreement in structure of that district with the Cotentin. The characters of the formations are further noticed in the abstract of the paper given in No. 48 of the Proceedings.

Feb. 22.—A paper on the Geology of Cutch, by Captain Grant, of the Bombay Engineers, and communicated by Charles Lyell, Esq., F.G.S., was read.

This district, so highly interesting on account of the phænomena which accompanied the earthquake that devastated it in 1819*, is situated near the eastern branch of the Indus, between 22 and 24 degrees of north latitude, and 68 and 72 degrees of east longitude. On the north, it is bounded by the Grand Runn, and the Thur or

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* A collection of papers relative to this earthquake appeared in Phil. Mag. First Series, vol. lxiii. p. 105 *et seq.*

Little Desert, on the south by the Gulf of Cutch and the Indian Ocean, on the east by the province of Guzerat, and on the west by the eastern branch of the Indus and the territory of Sinde. Its superficial contents are about 6500 square miles. The surface is traversed by three ranges of hills, having in general an east and west direction. The hills constituting the northern chain, which borders the Runn, present a perpendicular capping of sandstone, surmounting towards the north a sloping talus, and towards the south an inclined plane, both composed of laminated clay and slaty limestone, with occasionally layers of sandstone. The second or central range, is constituted partly of the formation last mentioned, and partly of another consisting of sandstone and shale. The third or southern, is formed wholly of volcanic rocks, but has nearly the same linear direction as the others.

To the south of the last range is an extensive flat, composed of a deposit, considered by Capt. Grant to be tertiary, and of an alluvial band, bordering the sea coast.

The first of these formations, which constitutes the northern range of hills, abounds with Ammonites, Nautili, Belemnites, Trigoniæ, and other fossils characteristic of the oolitic system of England. The formation of sandstone and shale, which occupies a much greater surface, contains, in various localities, thin beds of coal, sometimes very impure, but at others tolerably good; also layers of iron ore; and in the shale as well as in the sandstone, casts of reeds and impressions of ferns are stated to occur. With respect to the relative age of these two formations, Capt. Grant was unable to procure any decisive information; but he thinks that the sandstone and shale system passes beneath that of laminated clay and limestone.

The iron ore is smelted by the natives to some extent, particularly near the town of Doodye. The variety generally selected, on account of the imperfect apparatus employed, has a spongiform texture, small specific gravity, and is easily frangible. The ore is broken into small pieces and disposed in layers, alternately with others of charcoal, in a rude open furnace, acted upon by two small bellows made of sheep skin. The metal on being fused, falls into a small hole at the bottom of the furnace, whence it is removed into an inclosed furnace, and subjected to the same blasts until it acquires a white heat, when it is taken out and beaten into a bar. A considerable quantity of iron was formerly made from another variety of ore, found in the superficial soil at the north-western extremity of Cutch.

In one part of the province, the author noticed a deposit of variegated sandstone and marl, but was unable to determine its position with respect to the other formations. It is covered, in part, by an aluminous earth, on which rests a bed of red clay. The former, when visited by Capt. Grant, had been burning spontaneously for a long time, sending forth a suffocating sulphureous smoke. Considerable quantities of alum are made from the earth and exported to Bombay.

Another formation, described by the author, occurs south of Luckput, near the eastern branch of the Indus. It consists of soft and hard, whitish limestone, containing innumerable Nummulites and Fasciolites, also Echini, Spatangi, Ostrea and Corals.

The tertiary deposit consists of a hard, argillaceous grit covered by a conglomerate. The organic remains, which are very numerous, are often disposed in beds confined to one species; the prevaling genera being Arca, Pecten, Ostrea, Cardium, Conus, Cypræa, Ovula, Fusus, Trochus, Solarium, Strombus, and Cassis. Patches of Corals, two or three acres in extent, sometimes also occur.

Under the head of alluvial tracts, Capt. Grant gave an account of changes, produced along the southern coast by the deposition of sediment. At Mandavee is a ruin, at a spot called the old Bunder or quay, now about three miles inland; and in the centre of the town is a small temple, built upon a rocky foundation, but said to have stood in the sea when the old Bunder was the landing place. At other localities in the Gulf of Cutch, similar processes are going on, rendering it necessary to remove the landing-places frequently further seaward. The rapid progress of these accumulations is ascribed to the sea, during nine months washing back the sandy detritus, brought down by the periodical floods. The same operation is also in progress at places separated from the main waters of the gulf by small creeks or inlets, some of which penetrate six or seven miles from the coast, through a tract covered with shrubs. At low-water the whole of these plants are exposed down to their roots, but at high tide merely their tops are visible, so that boats sail through a completely marine forest. The growth of these shrubs is rapid, and the sailors have constantly to force the boats through the upper branches, particularly at the angles of the creeks, when they wish to save a tack. The stems and lower branches are covered with testacea, whilst the upper are occupied by numerous water-fowl. The land gains in this dictrict, by the deposition of the muddy contents of the small streams during the monsoon, when the water passing very slowly between the stems of the shrubs, a great portion of the matter held in suspension, is precipitated. This alluvial district occurs only on the southern coast of the Province. In August, 1834, the rains were very violent and continuous, and the river which flows past Nurra, on the borders of the Grand Runn, covered with a fine soil a surface of nearly 1000 acres. On the opposite or southern side of Cutch, not far from Mandavee, 300 acres were washed away; and not far from the same spot, half a small village was removed bodily into the sea.

Volcanic Rocks.—Besides the southern range of hills already stated to be entirely composed of trap or volcanic rocks, other extensive districts of the same nature occur between the northern and central ranges, and to the south of Luckput; besides innumerable minor outbursts, some of which forming small conical hills, are arranged around a central area. The author noticed no recent crater, unless the hill, called Denudar, be considered as such, and down the flanks of which he traced a lava stream. The volcanic rocks consist of several varieties of basalt, often columnar, amygdaloid, greenstone, and trachyte. Capt. Grant described these rocks in great detail, as well as the effects evidently produced by them, enumerating a great variety of instances, in which the disturbance of the strata can be traced, in the clearest manner, to the protrusion of trap. In some cases the volcanic mounds are themselves cracked or fissured from top to bottom.

That the igneous eruptions occurred at many distinct periods, Capt. Grant showed by sections, in which beds of trap alternate with others of crystalline limestone, calcareous tuff, and a calcareous grit, which sometimes contains angular fragments of basalt; and by beds of very different characters reposing on each other.

Among the phænomena connected apparently with volcanic action, the author described a number of mounds, varying in diameter from 3 to 20 yards, and covered with small tabular plates of sandstone, the lines of fracture radiating, though irregularly, from a centre. In some instances the summits of these little mounds having been removed, a regular circle of stones appeared, inclosing an area of sandstone, the fracture of the stones decidedly radiating as the stones of an arch. In other instances they resembled small hillocks, from the upper part of which the outer coating or tabular plates had generally fallen away, and the whole consisted of a heap of broken masses of rock.

The author then described what he considers to be a very recent volcanic outburst. It is situated in the nummulitic limestone, near the village of Wagé-ké-pudda, and forms a rather high flat basin, or table land of about two square miles, composed of calcareous marl, and flanked by low irregular hills of ironstone and gravel. The sides are broken by fissures, ravines, and hollows, and the bed of the basin is covered with hillocks of loose volcanic scoriæ of various colours. Within the basin are also several small craters or circular spaces, surrounded imperfectly by walls of columnar, globular, or friable basalt. These basaltic walls, however, he conceives, are of anterior date to the mounds of scoriæ, which he is of opinion cannot be of great antiquity, on account of the facility with which their loose materials are removed by atmospheric agents. Other similar outbursts were also described.

The paper concluded with an account of the Great Runn, a district (exclusive of the elevated tracts called "the Bunnee and Islands") of 7000 square miles. This singular region, as already described by Capt. Burns, consists of a sandy flat, dry for the greater part of the year, but during the prevalence of the south-west winds, converted into an inland sea, passable however on camels. Capt. Grant believes that its present oscillating position between land and water, is due to an elevation of the Runn, and not to a change in the level of the sea; and in support of his opinion adduced the alterations both of elevation and depression of land, by the earthquake of 1819. He described also several extraordinary walls of rock, thrown up apparently by volcanic action, sometimes assuming a dome shape, at others segments of circles or straight lines.

March 8.—The reading of a paper "On the Geological structure and phænomena of Suffolk, and its physical relations with Norfolk and Essex;" by the Rev. W. B. Clarke, F.G.S., began on the 18th of January, was concluded.

The observations detailed in this memoir were made during 1827, 1828, and 1829, and are arranged under the heads of the physical features of the county, the geological structure, and the effects produced by causes now in action.

Physical Features.—The general form of Suffolk is an oblong of about 47 miles by 27, bounded on the east by the German Ocean, the south by the river Stour, the west by the Ouse and Lark, and the north by the Little Ouse and Waveney. It is impossible, says Mr. Clarke, not to be struck with the fact, that whilst some of these river-courses have an east and west direction, others flow from N.N.W. to S.S.E., and that the coast section from Harwich to Orford is nearly parallel with the latter; and he adds, if these observations be extended to Norfolk and Essex, counties similar in geological structure, an accordance will be found in the direction of their rivers and estuaries.

How far these river channels may be due to dynamical action, operating from below, the author thinks it is difficult to determine; but he is of opinion that when they are studied with reference to the proofs of violent derangement in the north, east, and west corners of Norfolk, and the almost unequivocal evidence of disturbance on the coast of Suffolk, there is sufficient reason for assuming, that the drainage of Norfolk, Suffolk, and Essex has been induced by a violent strain acting from below, and throwing the whole mass of the country into a position, by which 1200 square miles of Norfolk and 220 of Suffolk are drained by the Yare, and about 2000 of the latter county at its south-east corner.

In addition to these facts, Mr. Clarke says, that the estuaries of the Alde, the Deben, the Orwell, and the Stour, having an average length of 11 or 12 miles, meet the fresh water at nearly the same distance from the sea, and at the boundary line of the great continuous bed of diluvium, which covers so extensive an area in that part of England; only detached patches of greater or less extent being found to the east of the line.

The Geological Structure.—The formations of which Suffolk consists, are chalk, the plastic and London clays, crag, diluvium or ancient superficial detritus, and recent lacustrine deposits; the first occupying the N.W. portion of the county, the second, third, and fourth the south-eastern, and the fifth or diluvium, the intermediate part, resting on all the preceding deposits; while the sixth or lacustrine accumulations are of very local occurrence.

Chalk.—This formation in the S.E. of Suffolk, is principally exposed in the banks of the estuaries and rivers, and contains the usual plates and nodules of flints, as well as the fossils characteristic of the upper chalk. In making a well at Harwich, at the depth of 93 feet, and between the white and gray chalk, a stratum 10 feet thick of sandy, gritty chalk was penetrated. The beds are in general nearly horizontal, declining gently to the south-east, but they sometimes dip at considerable angles, and the surface appears to have been violently dislocated and worn into deep gulleys, locally called sand-galls. At Harwich in making two wells only 70 yards apart, the chalk was reached in the northern at the depth of 88 feet, but in the southern at 64 feet. Where the formation has been denuded by the rivers, the tertiary beds frequently rest upon it; but where it rises into an elevation above the river levels, it is covered altogether by diluvium.

Plastic clay.—There is some difficulty in separating this deposit from the diluvium of Suffolk. At certain points, however, it displays the same beds of mottled clay and sands with pebbles, which characterize it in other parts of the kingdom; and it is occasionally exposed between the London clay and chalk. From its local occurrence Mr. Clarke is of opinion, that it has been subjected to denudating agents, and that it has partaken, in part at least, of the dislocations which have affected the chalk.

London clay.—This formation presents, in Suffolk, the usual characters. At Walton on the Naze and Bawdsey, considerable quantities of pyritous vegetable remains are washed up from a bed below the level of the sea; and are said to rival in variety and abundance the Sheppy fossils. The furthest western point at which the formation is visible, is Layham near Hadleigh, the country beyond being covered by diluvial clay. It constitutes, however, the substratum of all the estuaries which intersect the S.E. of Suffolk, and the coast section from Orford Ness to Walton Naze, being everywhere capped by crag. This section is constantly varying in its features, but presented when the author examined it, in 1827, the appearance of beds once horizontal, having been upheaved in some places and depressed in others; but to what extent these disturbances may have been produced by elevatory movements, the action of the sea, or the undermining of land springs, he could not satisfactorily determine. The supply of water obtained from the formation appears to be governed by local phænomena. At East Bergholt, a well was dug to the depth of 40 feet in London clay, without success; but in excavating a cellar a few feet from the well, a copious spring was tapped, and conveyed into it by a channel. In the river Ore is an island of London clay, in which two wells were sunk through 80 feet of clay, 3 inches of rock and 20 feet of sand. The water rose to the surface, had a strong smell of sulphur, but no saline taste, though it overflowed only during high tide in the river. The fossils mentioned by the author, are confined to fishes' teeth, and the occasional occurrence of shells. He mentions that land animals have undoubtedly been found in the London clay, as the tusk of an elephant at Harwich, but he is of opinion that the greater part of such remains, said to have been obtained from the formation, have been washed out either of the crag or the diluvium.

Crag.—Mr. Clarke says the term crag is applied in Suffolk only to the shelly beds, and the word gravel to the associated beds of pebbles, as well as to the accumulation of superficial pebbles. The portion of the county occupied by the deposit, is bounded on the west by a line connecting the water-head of the estuaries; and the most southern point at which it is now visible is Blackbrooke Hill, near Dedham, the patch at Walton Naze having been entirely removed by the partial destruction of the cliffs. Sand, however, containing shells occurs at Ardleigh Wood near Colchester, and it has been said that Danberry Hill, near Chelmsford, is capped by crag; but Mr. Clarke doubts the accuracy of this observation. The general surface of the area assigned to the deposit in Suffolk, is a platform of nearly regular elevation, which appears to have been worn into ridges and valleys by currents acting in parallel lines from N.E. to S.W., and the cliffs both in the interior and on the coast, are sections of these ridges. The dip of the London clay corresponds with that of the crag, and therefore Mr. Clarke infers, that both were acted upon by the same agents, and while they were beneath the level of the sea.

According to the author's observation, the deposit nowhere extends more than twelve or thirteen miles from the coast, and at Pakefield, where the diluvial clay comes to the very edge of the sea, it disappears as a surface deposit, but is visible at intervals further north, between that point and Cromer. Mr. Clarke also states, that though undoubtedly crag is discernible here and there, *in situ*, as a regular formation north of the Waveny, yet he by no means allows that it is regularly stratified, as an undisturbed deposit, between Leiston and Pakefield. He is fully convinced from observation, that the diluvial clay and crag are distinct deposits; and he is almost equally convinced, that if the crag has any share in the formation of the cliffs between the Blithe and Lake Lothing, it has been introduced by disturbances of a similar nature to those which are presented in the cliffs of East Norfolk. That the localities in dispute may have been once occupied by crag, there is no reason to deny, but they now present no traces of an undisturbed deposit.

To previous descriptions of the structure of the crag, the author states that he has nothing to add, except that where the shells are not visible, the sands contain a slight mixture of calcareous matter.

He objects to the separation of the beds with shells from those without, and shows that the shifting of a sand-bank, would correctly account for the occasional occurrence of beds of sand 30 feet thick, resting upon strata inclosing testacea.

Believing that the true rationale of the crag is to be found in the hypothesis of sandbanks, inhabited by testacea, and situated in a tidal way exposed to violent fluctuations of the sea, as well as subject to drifts of extraneous matter from land waters, the author sees nothing extraordinary in the idea that accumulations of sand and shingle may have formed a part of that deposit in which the crag is regularly stratified; but he cannot consent to such accumulations, though contemporaneous with the crag, being classed under that name, much less can he consent to diluvial clay being also included in it.

If then, says Mr. Clarke, we assume that in this tertiary sea, sandbanks were formed, around the shelves and under the lea of which testacea collected, lived and died, as at present, many of the phænomena of the crag may be readily solved, and we shall not need to wonder why the bivalve shells are found lying with the flat sides to the strata of sand; why the young are congregated in one group, and the old in another; why pebbles are found covered with balani; or why the remains of terrestrial mammalia are associated with those of whales and fishes.

With respect to Mr. Charlesworth's* subdivision of the crag into two ages, the author fully agrees with a qualification of the word age. If that gentleman had said different periods of the same age, Mr. Clarke would find no difficulty in admitting the justness of the classification; as not only species but genera of shells are differently grouped according to localities. The Norwich crag, he also admits, differs from the Suffolk, and on the authority of Mr. Woodward alludes to bouldered Suffolk shells occurring at Thorpe, in Norfolk.

The corals of the lower bed of the Suffolk crag, the author is of opinion, betoken a warmer climate, and he says, if during the crag era the earth gradually cooled, the change from a coralline deposit to one more nearly related to the inhabitants of the present era, would be the natural and inevitable result.

Mr. Clarke fully assents to the observation brought forward by Mr. Charlesworth in a paper, read before the British Association at Bristol†, on the mixed nature of the deposits now forming on the coasts of England, in which the remains of the adjacent cliffs are intermingled with the shells of existing species; and adduces examples with which he was acquainted, long before the reading of that paper.

The author then offers some further remarks on the necessity of separating the diluvium from the crag. He is of opinion that the gravel found in the latter, if carefully observed, will be acknowledged to differ from regular diluvial gravel, and that its occurrence only betokens a diluvial action during the period of the crag. That such actions have been often repeated, he says there can be no doubt, for the beds of superficial gravel of Suffolk and Dorsetshire, are evidently not of the same period; and no one who has studied gravel deposits accurately, can refuse to admit, that there have been more than one diluvial action, since the deposition of the tertiary formations. In Norfolk, he adds, it is true, the crag is involved in the clay, but if this clay which in that county is 400 feet and in Suffolk 300 feet thick, be one with the crag, it is most curious that a line of demarcation should actually exist, between the districts in Suffolk occupied by these deposits; and that the clay is never found below or intermixed with the crag. Moreover this diluvial clay has been traced not only into Norfolk but into Cambridgeshire and Essex, close up to the metropolis. In Suffolk the same line which bounds the London clay bounds the diluvial. By an extension of Mr. Lyell's argument all diluvial deposits, the author observes, might be included in the crag, and all other formations considered as diluvial. The only rational conclusion in Mr. Clarke's opinion, is, that during the crag era an extraordinary convulsion took place which shook the whole country. He gives also one or two instances in which diluvial clay and gravel have been introduced into cavities in the crag from overlying beds of superficial detritus.

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* Mr. Charlesworth's paper in which this division is established will be found in Lond. and Edinb. Phil. Mag. vol. vii. p. 81. See also p. 464 of the same volume, vol. viii. p. 529, and the paper referred to in the next note.

† This paper appeared in L. and E. Phil. Mag. vol. x. p. 1.

On the conchological history of the crag, Mr. Clarke offers no remarks, partly because it did not fall within his object in writing the paper, and partly because the data which he formerly collected have been lost.

Diluvium.—The diluvium of Suffolk may be divided into three classes: 1, clay; 2, gravel; and 3, erratic blocks.

1. *Clay.*—This division covers a considerable portion of the county, and extends into Norfolk, Cambridgeshire, and Essex, rising to a considerable elevation in High Suffolk, and attaining near Cromer, a thickness of 400 feet. A considerable portion of the clay is of a yellowish hue, but the greater part is blue; and both varieties contain chalk pebbles, sometimes disposed in layers but more commonly dispersed, a character by which the diluvial may be distinguished from the London or plastic clay. It is difficult to determine the origin of this argillaceous deposit; but the author is inclined to think, that the yellowish portion may have been derived from the plastic clays, and the blue from the clays below the chalk. Fragments of coal have been found in the diluvium at Lavenham, also fragments of mica slate containing garnets and tourmaline. Specimens of a similar rock were obtained by Mr. R. C. Taylor, at Cromer, with masses of granite, porphyry, trap, oolites, &c.* At Ballingdon Hill near Sudbury, Mr. Brown has procured thirty varieties of primary, secondary, and tertiary rocks. Comparatively few flints occur in the clay. At Ickworth a beautiful specimen of the Dudley trilobite was obtained in making a drain: and at various other localities, numerous species of tertiary and secondary fossils abound.

It is inferred that the clay contains cavities, as streams of noxious air occasionally issue from fissures.

2. *Gravel.*—The gravel is less generally diffused than the clay, and is considered by Mr. Clarke to have been partly deposited at a distinct period. In some cases, it consists merely of unrolled flints, left *in situ* by the dissolution of the chalk; in others, large masses of flint slightly mixed with chalk pebbles are imbedded in sand; and occasionally flints are intermingled with boulders of various dimensions, and sometimes unknown origin. Many of these extraneous fragments, he thinks, may have been washed out of the clay; and he shows that the river valleys have been excavated through both the clay and the gravel. With respect to the relative proportional quantity of each ingredient, chalk flints are said to be the most numerous, primary and transition rocks the next in abundance, and secondary and tertiary the fewest in number; the absence of the two latter being explained by their inferior hardness.

3. *Erratic blocks.*—These occur in great abundance, and occasionally of vast size. They are sometimes found in the river valleys, and sometimes on the level platforms and hills. They agree in lithological characters with the smaller fragments of the gravel, and are considered to be of the same diluvial origin; but are so conspicuous as to deserve a distinct notice.

Lacustrine deposits.—Under this head the author alludes to the bed containing freshwater shells, discovered by Mr. Charlesworth and Mr.

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* See Phil. Mag. and Annals, N.S. vol. i. p. 346.

Wood on the banks of the Stour at Sutton; and that described by Mr. Brown at Copford near Colchester, which contains similar shells; also bones of the ox and deer. Accumulations containing the same testacea, occur at Grays near Purfleet, and at Southend in Essex, but associated with remains of the elephant, rhinoceros, deer, ox, bear, &c. In the bed at Grays rounded pebbles of chalk occur, agreeing with those found in the diluvium, and from which they were probably derived.

Causes in Action.—In this division of the memoir are described, 1st, the alluvial accumulations; 2ndly, the changes in the river courses; and 3rdly, the action of the sea on the coast. In illustration of the two first, Mr. Clarke enters into a discussion on the supposed position of the Roman station, Ad Ansam, and the alterations which must have taken place, if the site assigned to it be correct. He then proceeds to the action of the tides on the cliffs.

His first acquaintance with the coast about Bawdsey was in 1814, and between that period and 1829, a battery which once stood 100 yards beyond the present low-water mark, has been dismantled; and the life-boat house has been three times removed, to a distance at least a quarter of a mile in rear of its original position.

The destruction between the Alde and Bawdsey cliff during the last 20 years, is calculated to have been upwards of 100 acres; and the coast between that cliff and Bawdsey Haven, is stated to have diminished about two yards annually. Similar remarks apply to the cliff between the Deben and Harwich harbour, batteries and martello towers having been successively undermined.

In 941 the church at Walton Naze was at a considerable distance inland; about 50 years since the church and burial-ground remained, but not a vestige of either is left. In 885 a sea fight took place between Alfred and the Danes at the mouth of the Stour, where the shingle bank now is. Harwich is also stated to have arisen in consequence of the destruction of Orwell, which stood on the spot called the West Rocks, and was overwhelmed by an inroad of the sea, since the Conquest. During the period, however, that these destructive changes have been proceeding on one side of Harwich harbour, sandbanks have accumulated at another, and compelled the Stour and the Orwell to open a new line of communication with the sea.

The author then gives the following conclusions as deducible from the statements in the body of the memoir.

1. The substratum of the whole of Suffolk, Norfolk, and Essex is chalk, which appears to have been dislocated and worn into deep hollows by the action of water, previously to the commencement of the tertiary era.

2. On this abraded surface the plastic clays and sands were formed, but not over the whole area.

3. Partly on these beds and partly on the chalk, the London clay was then deposited, but to no very great thickness.

4. Upon the London and plastic clays as well as the chalk, the crag was next accumulated in sandbanks, produced by the tidal waters, and around projecting masses of chalk.

5. While the crag still lay beneath the sea, a violent catastrophe

broke up many of the secondary strata, from the chalk to the lias inclusive, and the debris thus produced, together with numerous masses of ancient rocks, was spread by a rush of water over the surface of the tertiary formations and the chalk, in some places to a depth of 400 feet, constituting the beds of drift clay, &c., which occupy so great an area in Suffolk.

6. Previously to this diluvial action, and after it, the rivers of the then dry land bore to the sea, animal and vegetable remains, vestiges of which occur on the Norfolk coast and elsewhere.

7. The climate of this part of the globe was at that era different from the present.

8. After this period, and probably in prolongation of the first great catastrophe, a series of shocks acting from below, shattered the surface and gradually elevated the whole district, till the crag attained the height of nearly 100 feet above the level of the sea; and by this movement were produced the valleys or lines of fissure, through which the drainage of the county is effected.

9. No great convulsions have since taken place.

10. By the action of springs, and the constant battering of the sea, the superficial contents of the London clay and crag have been reduced several miles, vestiges of their former extent being traceable in rocks and sandbanks nearly always submerged.

11. By the set of the tides vast accumulations of shingle and sand have been formed at projecting points, protecting in some places the cliffs from further destruction; but at Harwich harbour they have blocked up the ancient estuary, and compelled the Stour and Orwell to form a new outlet.

12. The average amount of annual degradation of the coast is about two yards in breadth; and in consequence of the conformation of the ridges of crag and London clay, the cliffs will gradually diminish into a low sandy shore. The period estimated by Mr. Clarke for effecting this destruction is another century.

A paper "On the raised beaches of Saunton Downend and Baggy Point," by the Rev. David Williams, F.G.S., was then read.

The first of these breaches extends from Braunton Burrows to Downend Point; the other on the N. coast of Croyd Bay, from near the limekilns to half way to Baggy Point. These beaches were described in the paper read by Professor Sedgwick and Mr. Murchison on the 14th December, 1836*; and Mr. Williams in this paper fully agrees with the conclusions drawn by those authors, relative to the beaches having been raised.

In addition, however, to the proofs afforded by the abundance of remains of existing British marine shells in these accumulations, cemented together by calcareous infiltration into a tough sandstone, he stated that he had discovered in many places, from 6 to 10 feet, above the tidal level, and at the line of contact of the beaches with the old slate rocks of the district, countless Balani attached to the surface of the latter, but so firmly entangled in the substance of the former as to be separated with its fragments. A large granite boulder also rests on the

* See our last volume, p. 477.

slate, and is involved in the sandstone above any high-water mark. In support also of the land having been raised and not the sea depressed, he referred to the submarine forests of Somersetshire, in the prolongation of the same coast from Blue Anchor to the Parret; and argued that their position could not be accounted for by a subsidence in the sea level, but by an unequal movement of the land.

A communication by Mr. James de Carle Sowerby on his new genus of fossil shells, *Tropæum*, was then read.

This fossil is described by Mr. Sowerby as an involute chambered shell with sinuated septa; the whorls free, sometimes very distant; siphon in the external margin. The natural place of the genus is between Hamites and Scaphites, and the shells which may be grouped with Tropæum, have been hitherto ranked as Hamites, but have no sudden bend which may be compared to a hook. The species hitherto found have been obtained from the gault and green sand. The species (*Tropæum Bowerbankii*) described in the paper was obtained by Mr. Bowerbank in the Isle of Wight, and was found in the lower green sand on the south side of the Island.

ZOOLOGICAL SOCIETY.

Nov. 22, 1836 (*continued*).

The following observations on a species of *Glaucus*, referred to the *Glaucus hexapterygius*, Cuvier, by George Bennett, Esq., F.L.S., Corresponding Member of the Zoological Society, Surgeon and Superintendent of the Australian Museum at Sydney, New South Wales, were read.

"On the 20th of April, 1835, during a voyage from England to Sydney, New South Wales, in latitude 4° 26′ N., and longitude 19° 30′ W., with light airs and calms prevailing at the time, about 3 P.M., a number of damaged and perfect specimens of the *Glaucus hexapterygius*, Cuv., were caught in the towing net. On being immediately removed from the net and placed in a glass of sea water, they resumed their vital actions and floated about in the liquid element, exhibiting a brilliancy of colour and peculiarity of form, which did not fail to excite the admiration of the beholders.

"The back of the animal, as well as the upper surface of the fins and digitated processes, and the upper portion of the head and tail, was of a vivid purple colour, varying occasionally in its intensity; appearing brighter in colour when the animal was active or excited, and deeper when remaining floating tranquilly upon the surface of the water. The abdomen, and under surface of the fins, are of a beautiful pearly white colour, appearing as if it had been enamelled. The usual length of my specimens, measured from the extremity of the head to the tail, when extended floating upon the surface of the water, was 1¾ inches; sometimes one or two lines more or less. The body of the animal is subcylindrical, terminating in a tail, which gradually becomes more slender towards the extremity, until it finally terminates in a delicate point. The head is short, with very small conical *tentacula* in pairs; two superior, and two inferior; three (and in *G. octopterygius*, Cuv., four) branchial fins on each side, opposite, palmated, and digitated at their extremities; the number of digitations, however, varying; and the centre digitations are the longest; the first branchial fins, those nearest the head, are larger and denser than the others. The mouth is armed with bony jaws; the body is gelatinous and covered by a thin and extremely sensible membrane.

"These little animals were very delicate and fragile in their structure, and although many, indeed, I may say numbers, were caught, yet very few in comparison were found to be in a perfect condition, some being deficient in one, two, or more fins, and others being completely crushed. Not one of the specimens caught on this occasion, or during the voyage, had the silvery line or streak running down the back, from the head to the extremity of the tail; branching off also to the fins and along the centre of each of the digitations. Several *Porpitæ* were also captured in the net at the same time with these animals, and serve as food for them.

"It caused much regret to see the change death produced in the beauty of these interesting little animals, and all means of preserving them were found to be useless. When placed in spirits, the digits of the branchial fins speedily became retracted, the beautiful purple gradually faded and at last disappeared, and the delicate pearly white of the under surface of the body and fins peeled off and disappeared; thus did this beautiful mollusk become decomposed in less than the space of an hour. Some mollusks quickly lose their colour after death, but retain their form for a long time; but these speedily change after death, both in form and colour, and the beauty before so much admired perishes never to be regained.

"When taken in the hand, the under surface of the animal soon becomes denuded of the beautiful pearly white it previously had, and at that time appears like a small transparent bladder, in which a number of air-bubbles are observed, together with the *viscera*. On the *abdomen* being laid open, a large quantity of air-bubbles escaped, and perhaps a query may arise how far they assist the animal in floating upon the surface of the water?

"The figure of *Glaucus hexapterygius* in Cuvier's work '*Sur les Mollusques*,' is tolerably well executed, but no engraving can convey to the beholder the inconceivable delicacy and beauty of this mollusk; in the engraving alluded to, there is an inaccuracy at least as compared with the specimens before me,—in the digitated processes of the fins not being sufficiently united at the base; in the living specimens before me, they were united together at the base, and then branching off became gradually smaller until they terminated in a fine point. Again, in the engraving in Cuvier's work, the anal orifice is placed on the right side, whereas in my specimens it was situated on the left; for in all the specimens I examined, I found the *anus* was disposed laterally and could be plainly distinguished situated on the left side of the animal, a little below the first fin. This I consider also the orifice of generation, as in some of the specimens examined, a rather long string of dots resembling *ova* were seen to protrude from it. One of the animals discharged from this orifice a large quantity of very light brownish fluid; this no doubt was the *fæces*.

"But few of these animals were caught after the 20th until the 24th of the same month, in latitude 2° 26′ N., longitude 19° 51′ W., when having light airs from S. by E., nearly calm; in the morning a great number were seen floating by the ship, and it was not difficult, by aid of my towing-net, to capture as many as I required, for they swam very superficially upon the water. The whole of those taken proved to be of the same species (*G. hexapterygius*) as those before caught. I again placed several of the specimens in a glass of sea water; they were full of life, sometimes moving about, not very briskly, however,—and at other times remaining floating upon the surface of the water, merely gently moving the fins. As they floated upon the surface of the water in the glass, the sides of the head, back, tail, fins, &c., exhibited at the time a light silvery blue colour, which was admirably contrasted with the deeper blue of the upper surface, and falling into the elegant pearly or silvery white of the under surface of the animal, displaying an exceedingly rich and elegant appearance. Often, when at rest, the animal would drop one or more of the fins, but on touching them, they would be immediately raised to the former position, and that organ was turned back as if to throw off the offending object, followed at the same time by a general movement of the whole body. On touching the animal upon the back, it seemed to display more sensitiveness in that than in any other part of the body, judging from the effects produced, in comparison with similar experiments on other portions of the body; for instance, the centre of the back was touched lightly and rapidly with a feather; which caused the little creature to sink as if under the pressure of the touch, throwing at the same time the head, tail, and all the fins upwards, followed by a general distortion of the whole body of the animal, as if the gentle touch had been productive of severe pain. I invariably found every part of the upper surface of the body very sensitive when touched, and displayed a general movement of uneasiness throughout the whole of the body of the creature.

"These creatures have a peculiar manner of throwing the head towards the tail, and flouncing the tail towards the head, when they are desirous of removing any object of annoyance. It is at that time these animals seem to recover from their torpidity, and evince the greatest activity in their movements. When much annoyed, they throw the body about with great activity, coiling up the head, tail, fins, &c., in a somewhat rotundiform position; and if the tormenting object is not removed, dash out again in full activity of body, then return to the rotundiform position, and there remain for a short period apparently exhausted by their efforts. But on the cessation of the irritating cause, the animal quietly resumed its original position, perhaps dropping one or two of its wearied fins according as its own sensations of ease or comfort might dictate.

"When nothing irritated this tender mollusk, it would remain tranquilly floating upon the surface of the water with scarcely any movement but that which proceeded from the undulating movements of the digitated extremities of the fins, as well as an occasional slight twisting motion of the same organs.

"I felt much interest in the beautiful display of a circulating fluid on the dorsal surface of these animals, which was afforded me by the assistance of a microscope. Through the semi-transparent membrane of the back, a fluid could be readily perceived close to the surface, evidently flowing in two directions, one taking a course downwards, and the other returning upwards; but I was unable to distinguish two distinct vessels for these separate actions.

"These animals seemed to be very torpid in their movements, although sometimes, when floating upon the water, they would be seen busily engaged in moving their fins about, but those actions were soon suspended and their fins were suffered to hang lazily down, as if fatigued with the short exertion, which did not move them one inch about the glass of water; and even when the little indolent creatures did take the trouble to move themselves from one side of the glass to the other, it was effected by a tardy motion, stirring themselves first with one fin and then with the other, according as circumstances might require.

"I placed some small specimens of *Porpita* in the glass of water containing the *Glauci*, to observe if they would attack them; for some time one of the *Glauci* was close to a *Porpita* and was even annoyed by the *tentaculæ* of the latter touching its back, yet the *Glaucus* bore this, although with the usual characters of impatience, yet without attempting to attack it. At last it seized the *Porpita* between its jaws, and by aid of a powerful lens, an excellent opportunity was afforded me of closely watching the devouring process, which was effected by an apparently sucking motion; and at this time all the digitated processes of the fins were floating about, as at other times when the animal was at rest; but I did not observe, in one single instance, that they were of any use to the animal, either to aid in the capture or to securely hold their prey when in the act of being devoured; for the animal seems to depend merely upon the mouth in capturing its prey; as in this and other instances, which I had opportunities of observing, they seized their prey instantly with the mouth, and held it by that power alone, whilst by a kind of sucking motion the prey was devoured. The digitations may therefore only be regarded as appendages to the fins to aid the animal perhaps in the direction of its movements, as it was observed that they turned and twisted them about during the progressive motion, (that is, when this tardy animal is pleased to progress, which appeared to me very rarely to meet with its inclination,) as if in some way or other to direct the movements of the animal.

"The *Glaucus*, after eating the tentacles and nearly the whole of the soft under surface of its prey, left the horny portion, and remained tranquilly reposing upon the surface of the water after its meal, the only motion visible in the animal being the playing of the digits of its fins. The mutilated remains of the *Porpita* sank to the bottom of the glass.

"Soon after, another *Glaucus* began a devouring attack upon another *Porpita* which had been placed in the glass, eating a little of it and then ceasing after a short meal, occasionally renewing the attack at short intervals. On examining the *Porpita*, which had been partially devoured by the ravenous *Glaucus*, I found the disc had been cleared of the tentacles and other soft parts; a small part of the fleshy portion only remaining upon the disc. Only one part of the horny disc exhibited any injury, and that appeared to be the place where the animal was first grasped by the *Glaucus*.

"When any of these animals came in contact with another in the glass, they did not display any annoyance, or coil themselves up, nor did they evince any savage propensities one towards the other; and they would often float about, having their digitated processes in contact one with the other, without exhibiting any signs of annoyance; even when placed or pushed one against the other, they did not manifest any irritation, but remained undisturbed as in their usual moments of quiet repose.

"On the back of the animal being seen in a strong light, a black line could be discerned on each margin, and passing down the centre of each fin, and sometimes varied in having two black lines on the upper part of one fin, although the opposite fin may display but one.

"The margin between the falling of the purple colour of the back into the silvery white of the *abdomen* often exhibited beautiful tints of a golden green; but these variations were probably produced by the effect of different rays of light.

"These animals soon perished; I could not preserve them for any length of time in the glass of sea water, although the water was changed as often as it was thought necessary; the digitated processes of the fins were observed to shrink up on the death of the animal, and the process of decomposition rapidly took place, the whole body becoming a shapeless mass, having a bluish colour of deadly hue for a short period, and then became of a blackish or brownish black colour. I have seldom seen a gelatinous animal which appeared so firm whilst in the water, that proved so speedily to decompose when removed from it; even the beautiful purple of the back, the silvery or enamel of the *abdomen*, and the silvery blue of the sides, all speedily vanish, indeed instantly disappear, upon the death of the animal, as if it had been washed off; the expansive, delicate, and beautiful fins and digitated processes are no longer seen; they shrank up to nothing.

"Even on taking the animal alive out of the water and placing it upon the hand, that instant almost, from its extreme delicacy, it was destroyed: the digitations of the fins fell off, the least movement destroyed the beauty of the animal; it speedily lost all the deep purple and silvery enamelled tints, and became a loathsome mass. Thus do we too often find animals beautiful in external adornments, curious in their habits and organization, and calculated in every respect to supply us with inexhaustible sources of intellectual gratification, doomed speedily to perish; brief is the period allotted to them in the busy theatre of animated existence; but doubtless, with the gift of existence, they have received from the bounteous hand of their Creator, the means of enjoying their fleeting lives.

"To place these little animals in the glass of water from the towing net without injury to their delicate structure required care; so that as soon as they were captured in the net attached to the meshes, they were not handled, but carefully washed off, which was effected by dipping the meshes in the glass of water, when the animal soon detached itself without sustaining any injury, and floated in the water.

"Although these animals are so fragile, so easily destroyed on being taken out of their natural element, yet they fling themselves about in the water without sustaining any injury, without even the loss of any of the digitated processes of the fins; yet when there is much movement of the water in carrying the glass from one place to another, they are evidently disturbed and restless, and the fins are dropped; if therefore, a slight motion of the water disturbs them, what can become of these delicate mollusks during tempestuous weather? can they be similar to the delicate *Ephemeris*, doomed to live merely for the space of a day and perish in myriads? From the immense number seen only from the ship—and how many myriads more extended beyond our range of vision!—it conveyed to the mind some idea of the profusion of living beings inhabiting the wide expanse of ocean, and a feeling of astonishment at the inconceivable variety of forms and constructions to which animation has been imparted by creative power.

"The tail of this animal has been described as resembling that of a *Lizard:* the comparison is good, not only with regard to form, but also, with perhaps a little more flexibility of motion, when in action. Sometimes the animal throws its tail up to the body, as if intended to brush off any annoying object, and at other times, it has been observed to turn the head towards the side as if for a similar purpose. It seems, in the action of eating, to resemble a *Caterpillar*.

"No more of these animals were seen until the 15th of May at 10 P.M., when in lat. 24° 18′·5, long. 31° ′·01 W., moderate breezes and fine weather; a number of *Glauci* were captured as well as *Porpitæ;* some of the latter had been partially devoured, and in some only the horny disc remained; this, there was no doubt, from the previous knowledge of the carnivorous propensities of the *Glaucus*, was their work, more especially as we had positive proof that tribes of them were wandering or prowling about the ocean tonight. This was the last time during the voyage the *Glauci* were captured.

"From these animals devouring the *Porpitæ*, we had positive evidence of their carnivorous habits, independent of the structure of the jaws; and the *tentacula* of the *Porpitæ* were no protection against their enemies; indeed, these appendages were first devoured and the horny disc was alone left, in many instances being quite picked clean; from this circumstance we may infer, that the horny discs of the *Porpitæ* and *Velellæ*, which previously, and for the last four days were found in the net, were the remains of those which had been devoured by the *Glauci* or similar carnivorous mollusks, among which we may with safety include (from the structure of its jaws, and from often capturing it attached to *Velella*,) the inhabitant of the *Janthina fragilis* or violet shell.

"The more we pursue the investigation of the actions of living objects, the more we see of the unbounded resources of creative power; and, after all our reasoning, must conclude that some wise purpose, though dimly perceptible to our imperfect understandings, is no doubt answered by this great law of organic formation,—the law of variety."

Mr. Ogilby called the attention of the Meeting to the various preserved specimens of *Antelopes* then exhibited, and made the following observations on some *hollow-horned Ruminants*.

"In arranging the Society's collection subsequent to the late removal from Bruton Street, the following rare or undescribed species of *Ruminants* were observed, which it is thought proper to bring under the public notice of the Society.

"1. *Ixalus Probaton*. A single skin of the very anomalous animal to which I propose assigning this name, was presented to the Society by Dr. Richardson, and has been considered as the female of *A. Furcifer*, from which, however, it differs in some of the most important characters. Of its origin there can be no reasonable doubt; it was contained in the same box with the skins of *A. Furcifer*, and other animals obtained by the celebrated zoologist just mentioned, during Capt. Franklin's memorable expedition, and the hay with which it was stuffed contained numerous small locks of the very peculiar hair of *A. Furcifer*. The specimen is a male about the size of a *fallow Deer*, the length from the nose to the end of the tail being 4 feet 10 inches. The head is 9¼ inches long, the tail, 5½ inches; and the ear, 3¾ inches. Though the skin is that of an adult individual, as is proved by the incisors, which are all of the permanent class and considerably worn down, the head is without horns, having only two small, naked, flat scales, in the positions usually occupied by these organs; yet the bones of the skull remain beneath, and the specimen is unquestionably the spoil of a male animal. In form, as well as size, the animal resembles the *fallow Deer* (*Cervus Dama*). The colour is a uniform pale reddish brown above and on the outsides of the members; the breast, belly, and inner face of the *anus* and thighs are grayish white; the lower part of the cheeks, the lips and beneath the chin are of the same colour, but the whole throat or under surface of the neck is pale reddish brown, like the back and sides. The tail is covered above with short reddish hair like that of the body, but it is perfectly naked beneath, and in form and length resembles the tail of some species of *Deer* (*Cervus*). The nose is hairy like that of a *Goat:* the animal is furnished with lachrymal sinuses of considerable size, opening by very obvious apertures of a circular form; it has inguinal pores and two teats, as in the *common Antelope* (*A. Cervicapra*); large spurious hoofs, and no appearance of *scopæ* or knee-brushes either on the anterior or posterior extremities. These characters will not permit it to be associated with any known group of *Ruminants*. That it is not merely a *Deer* which has cast its horns, is proved by the absence of the pedestals which support these organs in the solid-horned *Ruminants*, as well as by the hairy lips, two teats and inguinal pores: neither can it be a *Sheep* or a *Goat*, as is evinced by the lachrymal sinuses, inguinal pores, and the length and form of the tail, which, in the wild species of these genera, is nearly tuberculous. The supposition of its being the female of *A. Furcifer* is disproved by the sex of the specimen; in other respects, the existence of large spurious hoofs shows plainly enough that it has no affinity to that animal. There is but one other supposition: may it not be a species of *Antelope* allied to the typical group of that genus? and may not the abortive horns of the present specimen be the result of some accident? This may certainly be the case; the other characters of the specimen agree with those of the *common Indian Antelope*, and if the animal should eventually prove to belong to that genus, it may bear the specific name of *A. Ixalus*, which the classical scholar will recognise as the name of an undetermined species of *Ruminant* mentioned in the Iliad.

"2. *Antilope Eurycerus.* Of this magnificent and hitherto undescribed species, two pairs of horns, one attached to the skull, the other to the integuments of the head, have long existed in the Society's collection. Their origin is unknown, but I have reason to believe that they come from Western Africa. Their length in a straight line is 2 feet 1¾ inch; on the curve, 2 feet 7½ inches; their circumference at the base is 10 inches; their distance at base 1 inch, and at the points 11 inches. In form they bear some resemblance to those of *A. Strepsiceros*, being wrinkled as in that species, and having a prominent ridge on their posterior face; but they form only one spiral twist instead of two, and their direction throughout lies in the plane of the forehead, whilst in the *Koodoo* these two planes form an angle of about 100°. The characters of the skull are likewise similar to those of the *Koodoo*, but it is broader and larger than in that animal. The points of the horns are of an ivory colour. The animal has a large muzzle, but is without lachrymal sinuses; it has a white band across the face, immediately under the eyes, and two white spots on each cheek. All these characters are distinctive of the natural group which includes the *Koodoo*, the present species, the *Boshbok*, the *Guib*, and the beautiful species mentioned by Mr. Bennett (Proc. Zool. Soc., 1833, p. 1.) which is a real *Antelope*, and which I hope shortly to have an opportunity of describing in detail under the name of *A. Doria*, as a friend, who has connexions with the West Coast of Africa, has kindly undertaken to procure me skins.

"3. *Antilope Philantomba.* Two females of this minute species lived for some time in the Society's Gardens: they were brought from Sierra Leone and presented by Mr. M^c^Cormick. Mr. Rendall, who saw them with me at the Gardens, assured me that they were the *Philantomba* of the Sierra Leone negroes. The larger and older specimen has small horns about 1½ inch long, bent slightly forwards and surrounded at the base with 5 or 6 small rings: the species is distinguished from the *pygmy Antelope* of the Cape by its longer tail and ears, the latter clothed with white hair on the inside, by the darker mouse-colour of the body and the uniform hue of the legs, which instead of being sandy red as in the Cape species, are of the same colour as the body, only rather paler. But for the circumstance of the female possessing horns, I should have been inclined to identify this animal with the *A. Maxwellii* of Col. Smith.

"4. *Antilope Sumatrensis.* This species and *A. Thar* were exhibited together for the purpose of pointing out the similarity of their zoological characters, and correcting a mistake into which Messrs. F. Cuvier, Desmarest, and Col. Smith have fallen with regard to the former species. According to these zoologists the *Cambing Outan* (*A. Sumatrensis*) possesses both the lachrymal sinus and the longitudinal gland on the maxillary bone, which distinguishes the *Duykerbok* (*A. Mergens*) and some other *Antelopes:* in reality the lachrymal sinus is sufficiently distinct, but there is not the slightest trace of any maxillary gland. The same zoologists represent the female *Cambing* as being without horns and having only two teats: the specimen exhibited, a young female, had tolerably large horns and distinctly showed four teats, thus agreeing in all respects with the adult female *Thar* with which it was compared.

"5. *Antilope palmata.* Colonel Smith has described the horns of this species from an imperfect pair preserved in the Museum of the College of Surgeons, but was undecided whether it should be considered as a distinct species or only a variety of the *Prongbaick* (*A. Furcifer*). The present perfect pair, with the skin of the head attached, goes far to prove the specific distinction, but the habitat is widely different from that assigned by Colonel Smith. The specimen came from Mexico, where Dr. Coulter informs me it is sufficiently common. The horns are twice or thrice as large again as those of *A. Furcifer,* and instead of preserving a tolerable degree of parallelism, as in that species, spread widely, and are much hooked at the points. The face also is of a very dark brown colour, whilst in *A. Furcifer* it is of the same light fawn as the upper parts of the body."

Mr. Gray exhibited a specimen of *Argonaut* with an *Ocythoë* from the Cape of Good Hope, and stated that as the subject had been brought forward at the last meeting, he was induced to remark that every time he considered it, and compared it under its various bearings with the relations of other *Molluscans* and their shells, he was more and more inclined to believe that the animal found in the shell of *Argonauta* was a parasite. He gave the following reasons for this belief.

"1. The animal has none of those peculiarities of organization for the deposition, formation, and growth of the shell, nor even the muscles for attaching it to the shell, which are found in all other shell-bearing *Molluscans*; instead of which it agrees in form, colour, and structure with the naked *Mollusca,* especially the naked *Cephalopods.*

"2. The shell, although it agrees in every respect with the shells of other *Molluscans* in structure, formation, and growth, is evidently not moulded on the body of the animal usually found in it, as other shells are; but exactly agrees in every point (except in the form of the spire), with the shell of *Carinaria*, which coincided with the other *Molluscans* in all these respects.

"3. The body of the animal does not appear to have the power of secreting calcareous matter, for it does not, like all the *Mollusca* which have that power, secrete either a solid deposit or distinct *septa* to adapt the cavity of the shell to the increase of the body, nor does it cover over with calcareous matter any sand or other extraneous bodies which may have accidentally intruded themselves between the mantle and the shell, but leaves the sand, which is often found mixed with the eggs, free, without taking any means to prevent it from irritating the skin.

"4. The young shell of the just hatched animal which forms the *apex* of the shell at all periods of its growth, is much larger (ten times) than the eggs contained in the upper part of the cavity of the *Argonaut*."

Mr. Gray further stated, that he does not think that any inference can be drawn in favour of the opinion that the *Ocythoë* forms the shell, from either of the three arguments which have been produced in favour of that hypothesis, which he then examined in detail.

"5. He believes that Poli must have been misled when he thought that he had discovered the animal in the egg of an *Ocythoë* covered with the 'rudiment of a shell,' because all the *Molluscans* which he has seen in the egg (*Cephalopods* as well as others) were covered with a well-developed shell, even before all the organs were developed, and the figure which Poli gives of the rudiment does not agree with the nucleus found on the *apex* of the shell of the *Argonauts*. Unfortunately, none of the eggs of the *Ocythoës* that have been examined by other observers have been enough developed to show the fœtal animal.

"6. The different species of *Argonauta* are said to be inhabited by different species of *Ocythoë;* but allowing this to be the case, it only proves that each of these genera have local species: the same may be observed with respect to the *Hermit Crabs*, without proving anything in favour of their being the framers of the shell they live in.

"7. That though some specimens of *Ocythoë* preserved in their shells are marked with cross grooves resembling the grooves on the shell, yet these grooves are only formed by the pressure of the dead animal against the shell; for the specimens of the animal which are found out of the shell, or which are taken out of the shell while recent, are always destitute of these grooves, or of the compressed form of the cavity of the shell. That some specimens which he had received from the Cape (of which that now on the table was one), which had been packed on their sides, had the upper side of the animal smooth and rounded, and the lower flat, and curved like the shell on which it was pressed by its own weight; while a specimen which he had received from the Mediterranean packed erect, with the mouth upwards, so that the animal was equally pressed against each side of the shell, was flattened and curved on each side, like the specimen examined by M. Ferussac."

Mr. Gray also stated that, so far from the animal using the finned arms as sails, they were the means by which it retained itself in the shell; and he further observed, that it was very difficult to distinguish the species of *Argonauta*, as they varied greatly in shape, and that on a comparison of many specimens, he had found that the presence or absence of the spines or ears at the back of the mouth were of no importance as a specific character, specimens of each of the recorded species having this process developed only on one or the other side.

The Chairman, (Mr. Owen,) after premising some observations on the diseases to which the mortality of the larger feline animals in the Society's Menagerie was attributable, proceeded to read the following description of two *Entozoa* infesting the stomach of the *Tiger*, (*Felis Tigris*, Linn.,) one of which forms the type of a new genus of *Nematoidea*.

"I received a few days ago, from the Medical Superintendent of the Society's Menagerie, a portion of the stomach of a young *Tiger* (which died of rupture of the *aorta*), exhibiting on the internal or mucous surface what were considered to be scrofulous tumours. They were five or six in number, of a round and oblong form, varying in size from half an inch to two inches in the largest diameter, and the largest of them projecting about half an inch from the plane of the inner surface: they made no projection externally. The mucous membrane covering the smaller tumours was puckered up into minute reticulate *rugæ:* the surface of the largest tumour was smooth. On wiping away the tough thick mucous secretion from the tumours, and examining more closely their surface, two or three orifices presented themselves in the larger, and a single orifice in each of the smaller tumours. These orifices conducted to irregular sinuses which were the *nidi* of two kinds of *Nematoid Entozoa*, some measuring nearly an inch in length and a line in thickness; the others being more minute, not exceeding 5 lines in length, and about $\frac{1}{30}$ of an inch in diameter. Only a pair of the larger *Entozoa* were found in each of the three largest tumours; the smaller species existed in countless numbers.

"Before proceeding with the description of the worms, I may briefly conclude the history of the tumours by observing that they were composed of condensed accumulated layers of the sub-mucous cellular tissue, presenting a flat surface next the muscular coat, to which the larger tumours firmly adhered, and projecting with a rounded convexity towards the cavity of the stomach, where the sinuses opened and terminated. They did not contain any of the caseous secretion characteristic of *struma*, but were most probably caused by the irritation of the *Entozoa*.

"The dimensions of the larger *Entozoa* above given are those of the female: the male is about one fourth smaller. In both sexes the body is slightly attenuated at the two extremities; the caudal extremity is more inflected and more obtuse in the male; the oral extremity in both is obtuse and truncate.

"The surface of the body appears to the naked eye to be minutely striated transversely: it is variegated by the white genital, and amber-coloured digestive tubes appearing through the transparent integument. When examined with a lens of half-inch focus, the anterior two-thirds of the body are seen to be covered with circular series of minute reflected spines, which, viewed with a still higher power, present three distinct points, one large one in the middle and two small lateral ones.

"The mouth is surrounded by a tumid circular lip armed with six or seven circular rows of well-developed spinous processes of a similar complex structure to those on the body. The oral orifice itself presents the form of a vertical elliptical fissure, bounded on each side by a jaw-like membranous fold or process, the anterior margin of which is produced in the form of three straight horny points or processes, directed forwards. These lateral processes can be protruded beyond the circular lip by compressing the smooth spineless skin behind the latter; and the elasticity of the structure causes them to be again retracted on remitting the pressure.

"The *vulva* is situated at the junction of the middle and posterior thirds of the body; the *anus* in the female is in the form of a transverse semilunar fissure immediately behind the obtuse posterior apex, and on the concave side of the inflection.

"The *anus* of the male, from the anterior part of which a single slightly-curved intromittent *spiculum* is protruded, is surrounded by eight distinct pointed *papillæ*, three of which are placed in a vertical row on each side, and two smaller ones at the lower boundary of the common opening to the *rectum* and male gland.

"On comparing this *Nematoid* worm with those already described, it approaches most nearly to some species which are referred by Rudolphi to the genus *Strongylus*, as the *Strongylus trigonocephalus*, R., (*Hist. Entoz.* ii. pl. I. p. 231.,) in which species the '*Bursa maris subglobosa, biloba, multiradiata,*' presents an approximation to the structure of the external male organs above described, in which the eight tubercles surround the opening somewhat after the manner of rays. But on pursuing the comparison we find that here the resemblance ceases: there is no subglobose bilobed sheath to the intromittent organ in the species here described; the head is surrounded by a circular instead of a trigonal lip; the *Strong. trigonocephalus* is placed by Rudolphi in the section *c, ore nudo*, while the armature of the mouth, in the present species, is so remarkable, as to induce me to regard it as the type of a new genus, which I propose to denominate *Gnathostoma**.

"Gen. Char. *Corpus* teres, elasticum, utrinque attenuatum. *Caput* unilabiatum, labio circulari tumido integro; os emissile, processibus corneis maxilliformibus duobus lateralibus denticulatis. *Genitale masculum* spiculum simplex, ad basin papillis circumdatum.

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* γναθος *maxilla*, στομα *os*.

"Sp. *Gnath. spinigerum.* Gnath., capite truncato, corpore seriebus plurimis spinulorum armato.

"The generic difference indicated by the external peculiarities of the *Entozoa* above described, is confirmed by the internal anatomy, which presents some peculiarities which appear not to have been hitherto detected in the class *Entozoa:* I refer more particularly to a distinct salivary apparatus, conformable to that which exists in the *Holothuria* and other *Echinodermata*. This apparatus consists of four elongated straight blind tubes, each about two lines in length, which are placed at equal distances around the commencement of the alimentary canal, having their smaller extremities directed forward, and opening into the mouth, at the base of the lateral tridentate processes, and their closed obtuse ends passing backwards into the abdominal cavity. When examined with a lens of $\frac{1}{4}$ inch focus, the *parietes* of these salivary tubes present very distinct oblique or spiral decussating fibres; their contents are semi-pellucid in the recent worm, but become opake in spirit of wine.

"The coexistence of these salivary glands with an oral apparatus which is better adapted for trituration than any that has hitherto been detected in the *Entozoa*, is conformable to the laws which regulate the existence and condition of the salivary apparatus in higher animals; and is highly interesting on that account. The only allusion which I can find to salivary organs in other *Entozoa* is in Cloquet's '*Anatomie de l'Ascaride Lombricoïde*,' in which he considers the thickened glandular *parietes* of the *œsophagus* to serve for an analogous secretion.

"The first portion of the alimentary canal or stomach is about 3 lines in length; it contains a milk-white substance, and is separated by a well-marked constriction from the remaining portion, which we may regard as intestine: this is filled with a pulpy substance of an amber colour, which grows deeper in tint as it approaches the *anus*. The intestine enlarges slightly as it passes backward; it is wide and straight: is not tied down to the *parietes* of the body by mesenteric filaments as in the *Strongylus gigas*, &c.; its surface is irregular, and it seems to contain a spiral tube or valve, but this appearance arises from the nature of the internal surface of the intestinal tunics, which is beset with large regular obtuse lozenge-shaped processes arranged in alternate longitudinal rows.

"The lateral lines of the body consist distinctly of two vessels, which project into the interior of the body, being attached by a small part of their circumference; and becoming very wide and free near the head. The dorsal and ventral nervous cords are plainly visible in the midspace of the lateral vessels. The muscular tunics of the body are well developed, consisting of external transverse and internal longitudinal fibres. The latter are lined with a layer of pulpy flocculent substance.

"The male organs consist of a slightly-curved slender single *spiculum*, projecting from the caudal extremity of the body, as above described. The base of this *spiculum* communicates with a dilated receptacle, 2 lines long, of an opake white colour, which is separated by a slight constriction from the rest of the seminal tube; this is, as usual, single: it is semi-transparent, and gradually grows smaller to its blind extremity, which is attached by cellular tissue to the middle line of the ventral surface of the body, half-way between the two extremities. The whole length of the seminal tube is ten times that of the entire worm.

"The female organs consist of the *vulva, vagina, uterus bicornis,* and *oviducts* or *ovarian tubes.*

"From the *vulva,* the situation of which has been already mentioned, the *vagina* is continued, at first wide, then narrower, and lastly widening again to pass into the *uterus:* it exceeds an inch in length. The two *cornua* of the *uterus* are each about ½ a line in diameter, and 5 lines in length; they diminish and are continued without any constriction into the *ovarian* tubes; these are of immense proportional length, each exceeding, by 30 times, the length of the body; their attenuated extremities or beginnings are not attached to the *parietes* of the body; although the coils of the oviducts appear at first sight to be inextricably interwoven around the intestine, they in reality cover it in aggregate folds, which are easily separated from the intestine, and unravelled."

Mr. Owen stated in conclusion, that preparations exhibiting the male and female organs thus unfolded, with the digestive canal and salivary apparatus, had been deposited in the Museum of the Royal College of Surgeons.

ROYAL IRISH ACADEMY.

January 9.—Sir William Betham read a letter from the Baron de Donop, of Saxe Meiningen, on the subject of the alleged discovery of the MS. Translation of Sanconiathon's History of the Phœnicians, by Philo Biblius.

Sir William Betham read a letter from Sir John Tobin, of Liverpool, respecting the cast-iron ring money found on board the wreck of a vessel, and exhibited at the meeting of the Academy in November. The following is an extract:

"On the subject of the schooner Magnificent, which was lost somewhere near Cork some time since; she was bound to the river Bonney, or New Calabar, which is not far from the kingdom of Benin. The trade to these rivers for palm oil and ivory, is cotton goods, gunpowder, muskets, and a great variety of other articles; and among them *manillas,* both of iron and a mixed metal of copper and brass, which is the money that the people of Eboe and Brass Country, and all the nations in that neighbourhood, go to market with. On Wednesday next I will send you a manilla of each kind."

Sir John Tobin states the price of the copper manillas to be 105*l.* per ton, and that of the cast iron 22*l.*; the former passes, therefore, for about five of the latter. They so perfectly resemble the Irish antique as to be scarcely distinguishable except by the difference of the material.

Sir William Betham also read a letter from Captain Edward Jones to Samuel Hibbert, M.D., which the latter gentleman transmitted to him, with the sketches there alluded to.

"The annexed two sketches are taken from a cast of the species of money now at the present day passing current among the Africans. It so strongly resembles what we saw in Ireland, that I thought you might be interested in a copy of it. Mr. Dyson, who was for some years a surgeon on board an African merchantman, brought it with him; and the first opportunity I shall make inquiries respecting this and other coin used among the natives. I am told that in the country they are made of solid gold, as in Ireland."

Sir William Betham also read an extract from a letter from Mr. Bonomi to T. C. Croker, Esq.

"You asked me for a note on the ring money of Africa; here it is. So little has the interior of the country changed in that particular since the days of the Pharaohs, that to this day, among the inhabitants of Sennaar, pieces of gold in the form of a ring pass current as money. The rings have a cut in them for the convenience of keeping them together; the gold being so pure you easily bend them and unite them in the manner of a chain. This money is weighed as in the days of Joseph."

These gold rings are so similar in shape to the ancient rings found in Ireland, that the sketch of one accurately represents the other.

It is a remarkable fact that the name *manilla*, which these brass and iron articles still bear in Africa, signifies *money* in the Celto-Phœnician Irish. *Main* is 'value,' 'worth,' and *aillech* is 'cattle,' 'household stuff,' or 'any kind of property.' So that in this respect the derivation is similar to that of *pecunia* from *pecus*. The manillas were, no doubt, introduced into Africa by the same people that brought them to Ireland; and as the negro nations have changed but little, if at all, they still pass as money by their old Phœnician name.

The Rev. James H. Todd, A.M., M.R.I.A., Fellow of Trinity College, gave an account of a discovery made by Mr. John O'Donovan, of a valuable though imperfect copy, in MS., of the Annals of Kilronan, or Book of the O'Duigenans, a work that had hitherto been supposed to be lost. It is particularly described in the 'Proceedings' of the Academy, No. 2.

Mr. Petrie exhibited a MS. of the four Gospels, in Latin, of which he had given an account in a paper read some time since before the Academy. This manuscript is said to have been that given by St. Patrick to the first Bishop of Clogher. It is inclosed in a brazen case, of very curious workmanship, on which the circumstances connected with the gift are represented in highly raised figures.

Professor Lloyd communicated to the Academy the continuation of his investigations "On the Propagation of Light in uncrystallized Media."

In the first part of this paper, read on a former evening, the author had expressed his conviction that the problem of wave-propagation in bodies was incompletely solved, unless the action of the material molecules be taken into account. This he has attempted to do in the present continuation, confining himself to the comparatively simple case in which the molecules of the æther and of the body are uniformly diffused.

The differential equations of motion inferred from these considerations contain, each, the displacements of the molecules of the æther and of the body, with coefficients depending on the masses and distances of the molecules, the law of force to which they are subjected, and the length of the wave. By a particular method of elimination, these pairs of *simultaneous* equations may be reduced each to a single one, of the simple form which occurs in the case of a single vibrating medium, the new coefficient being connected with those of the original equations by an equation of the second degree. The expression for the displacement, then, is of the same form as in the case of a single vibrating medium; but the relation between the coefficients of the time and of the distance, and consequently the velocity of propagation, will be very different.

The quadratic equation above alluded to expresses the relation of these coefficients, or, in other words, the relation between the period of vibration and the length of the wave. When the action of the molecules of the æther and of the body, *inter se*, and on one another, is governed by the same law, this equation is resolvable into simple factors, one of which only seems to belong to the problem, the other giving an expression for the velocity of propagation independent of the length of the wave. The author accordingly proceeds to develop the former of these formulæ, converting the triple sums which it contains into triple integrals, according to the method of M. Cauchy.

Among the consequences deducible from this development is the following: In the expanded expression for the velocity of propagation, each term consists of two parts, one of which is due to the action of the æther, and the other to that of the body. It is not improbable that there may be bodies for which the first or principal term is nearly nothing, the two parts of which it is composed being of opposite signs, and nearly equal. In this case the principal part of the expression for the velocity will be that derived from the second term; and, if that term be taken as an approximate value, it will follow that the refractive index of the substance must be in the sub-duplicate ratio of the length of the wave nearly. Now, it is remarkable that this law of dispersion, so unlike anything observed in transparent media, agrees pretty closely with the results obtained by Sir David Brewster in some of the metals. In all these bodies the refractive index (inferred from the angle of maximum polarization) *increases* with the length of the wave. Its values for the red, mean, and blue ray, in silver, are 3·866, 3·271, 2·824; the ratios of the second and third to the first being ·85 and ·73. According to the law above given, these ratios should be ·88 and ·79.

Professor MacCullagh made a verbal communication on the probable nature of the light transmitted by the diamond and by gold leaf. He conceives that as there is a change of phase caused by reflexion from these bodies, so there is also a change of phase produced by refraction; the change being different according as the incident light is polarized in the plane of incidence, or in the perpendicular plane. Consequently, if the incident ray be polarized in any intermediate plane, the refracted ray should be elliptically polarized; and on examining the light transmitted by gold leaf, this was found to be the case. Of course the same thing is true of the light which enters the other metals, and which is subsequently absorbed. The same remark explains the appearance of double refraction in specimens of the diamond which give only a single image; and it is likely that other precious stones will be found to possess similar properties. Mr. MacCullagh has obtained a general formula for the difference of phase between the two component portions of the refracted light, one polarized in the plane of incidence, and the other perpendicular to it. He finds from this formula, that the difference of phase, which is nothing at a perpendicular incidence, increases until it becomes equal to the *characteristic* at an incidence of 90°; and when the light emerges into air, the difference of phase is doubled. The formula has not yet been submitted to the test of experiment.

Mr. MacCullagh then read a paper "On the Laws of Crystalline Reflexion and Refraction.*"

In this paper the solution of the following problem is given for the first time:—Supposing a ray of light, polarized in a given plane, to fall on a doubly refracting crystal, it is required to find the plane of polarization of the reflected ray, and the proportion between the amplitudes of vibration in the incident, the reflected, and the two refracted rays.

The constructions to which the author has been led by his theory are extremely simple, and may be explained most easily by referring to a paper which he has already published in the Transactions of the Academy, vol. xvii. pp. 251, 252. To avoid circumlocution, he uses the term *transversal,* to denote a right line parallel to the plane of polarization of a ray, and perpendicular to the direction of the ray itself. When the transversal is spoken of as a finite magnitude, its length is understood to be proportional to the amplitude of the vibrations in the polarized ray. Let O (as in the place just referred to) be the point of incidence on the crystal, and OT, OT′ the directions of the two refracted rays, the points T, T′ being on the wave-surface. Corresponding to the points T and T′ on the wave-surface, there are two other points, P and M, on a second surface, which is reciprocal to the wave-surface. The points P and M are derived from the points T and T′ by an easy rule, which is given in the place before cited. Now if we wish to find in what direction the incident ray must be polarized in order that the ray O T′ may disappear, let us draw, through the point O, a plane A perpendicular to the plane OTP, and parallel to the right line TP, which joins the corresponding points T, P. This plane A will intersect the planes of the incident and reflected waves in two right lines, which will be the transversals of those waves; so that if the incident ray or wave be polarized parallel to the first in-

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* Papers on this subject by Prof. MacCullagh will be found in Lond. and Edinb. Phil. Mag., vol. viii. p. 103, vol. x. p. 42.—Edit.

tersection, the reflected ray will be polarized parallel to the second intersection, and there will be only a single refracted ray OT. A right line drawn through the point O, perpendicular to the plane OTP, will lie in the plane A, and will be the transversal of the refracted ray OT; and if, measuring from the point O, the lengths of the three transversals represent the amplitudes of the respective vibrations, the transversal of the refracted ray OT will be the diagonal of the parallelogram, whose sides are the transversals of the incident and reflected rays. The problem is, therefore, completely solved in this case, and it is obvious, that a construction precisely similar will apply to the other case, in which OT′ is the only refracted ray. The plane B, which, in this second case, answers to the plane A in the first case, is perpendicular to the plane OT′M, and parallel to the right line T′M.

If the incident ray be polarized in a direction intermediate between the two transversal directions which give only a single refracted ray, the incident vibration may be resolved into two vibrations parallel to those two transversals. The reflected vibrations arising from each of the component incident vibrations are to be found by the foregoing rules, and then to be compounded.

When the intersection of the planes A and B is perpendicular to the direction of the reflected ray, this ray is polarized parallel to that intersection, whatever be the plane of polarization of the incident ray. The angle of incidence at which this takes place is the polarizing angle.

When the refracted ray OT or OT′ is a normal to the wave-surface, the plane A or B is the plane of polarization of the ray. For example, if OT be the ordinary ray in a uniaxal crystal, the plane A contains the ray OT and the axis of the crystal.

The hypotheses from which Mr. MacCullagh has obtained the foregoing laws, are these:

1. The density of the æther is the same in all media.
2. The vibrations are parallel to the plane of polarization.
3. The *vis viva* is preserved.
4. The vibrations are preserved: that is, the resultant of the incident and reflected vibrations is the same as the resultant of the refracted vibrations.

The author finds that his theory represents very accurately the experiments of Sir David Brewster and M. Seebeck, on the light reflected in air from a surface of Iceland spar.

January 23.—Captain Portlock read a notice of the occurrence of *Anatifa vitrea**, of Lamarck, in several localities on the Irish coast. He commenced by enforcing the great importance of recording as quickly as possible the first discovery in a new locality of any species of the animal or vegetable kingdom, as tending to perfect the Fauna or Flora of the district in which it is found; and pointed out the value of such local Faunæ and Floræ in estimating the relations and mutual dependencies of coexisting animals and plants, and affording a basis of comparison by which future observers may be enabled to test the probability of new organic beings occa-

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* *Lepas fascicularis* of Ellis, Montagu, and other authors; *Lepas dilata* of Donovan.

sionally appearing on the surface of the present earth, in the same manner as they appear to have occurred at very distinct epochs in the more ancient world*.

Captain Portlock then cited the various authors who have mentioned this species of the pedunculated division of Lamarck's class Cirrhipeda, beginning with its first discoverer, Ellis, who figured and briefly described it in his Natural History of Zoophytes, published in 1786. It is there stated to have been obtained in St. George's Channel. It was afterwards found on the western coast of England by Mr. Brier and Mr. Montagu, but is still considered there (as stated by Turton in his Conchological Dictionary) very rare. The Rev. Dr. Fleming communicated to the Wernerian Society, between 1811 and 1814, his discovery of the species in considerable abundance on the coast of the Zetland Islands. Lamarck formed his species *vitrea* from a specimen obtained on the shore of Noirmantier, an island off the coast of Poitou, apparently the first noticed in France. He had, however, seen a specimen of the *Lepas fascicularis*, sent him by Mr. [afterwards Dr.] Leach, and states his opinion that it is only a variety of *vitrea*. A cluster of this species of Cirrhipedæ having been sent to Captain Portlock by one of the Ordnance Survey Collectors, from the north coast of Antrim in the autumn of the last year, he was induced to make further inquiry as to its previously known existence in Ireland, and having mentioned the circumstance to Mr. R. Ball, was informed by him of four cases of its occurrence which he had recorded, viz. on the coast of Youghal in 1819; coast of Clare, 1823; coast of Clare, 1828; coast of Antrim, 1834. These localities, therefore, taken with his own, constitute a very wide range, and show that this species, still considered as very rare on the coast of England, and apparently equally so in France, has been traced round the western shore from the north to the south of Ireland. Specimens of *Anatifa lævis*, Lamarck, (*Lepas Anatifera*, Linn.,) accompanied those of *vitrea*. This is a common species all round the Irish coast. Captain Portlock mentioned that Mr. Ball had either in possession, or a record of, the following species of Cirrhipedæ, as Irish:

Anatifa sulcata, (*Lepas sulcata*, Mont.), Youghal; found also by Mr. O'Kelly, near Kenmare.

Anatifa striata, Lamarck, (*Lepas Anserifera*, Linn.,) Dublin Bay.

Pollicipes scalpellum, Lamarck, (*Lepas scalpellum*, Mont.,) found by Mr. W. H. Harvey in Dublin Bay.

Cineras vittata, Leach, Lamarck, (*Lepas membranacea*, Turton,) attached to a plank cast on shore near Malahide.

Otion Cuvieri, Leach, Lamarck, (*Lepas aurita*, Linn.,) attached with a Cineras to a Balanus. The whole constituting a very large proportion of the pedunculated Cirrhipedes at present known in Great Britain.

Professor Lloyd exhibited to the Academy some modifications which have been recently made in the construction of the Magneto-electric machine.

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* We are glad to see the consideration of this almost neglected subject thus brought forward in a definite manner. Its relations to the philosophy of both zoology and geology appear to us to be very important.—Edit.

XVI. *Intelligence and Miscellaneous Articles.*

REMARKS ON THE COMMENCEMENT OF SIR E. FF. BROMHEAD'S PAPER ON BOTANICAL CLASSIFICATION.

The subjoined remarks were intended to be appended as a note to this paper, but were omitted for want of room, as stated in p. 51.

THE introductory passages of Sir E. Ffrench Bromhead's interesting paper have vividly reminded us of the principles on which the natural arrangement of organized beings should be investigated, as laid down in the *Horæ Entomologicæ.* The principle of the "first attempt to arrange natural families," that of assemblage, is one of those on which Mr. Macleay has most strongly insisted. The remark on the representation of the progress of development by a *spiral,* perfectly agrees with the result of investigating the affinities of animals, already compared by the Rev. Mr. Kirby (Introduction to Entomology, vol. iv. chap. xlvii. p. 407.) to "*a convolving series.*" We may add, however, that the relation between the opposite points of contiguous whorls of the spiral, "which appear to be the nearest, though distant by a whole round," is of a different nature from that which unites into a series the groups forming each whorl. The former is the relation of *analogy*: the latter that of *affinity*. Were Sir E. Ff. Bromhead to subject the principles hitherto followed in investigating the classification of plants, and those advocated by Mr. Macleay, to the reciprocally severe test of applying the latter, directly, to botanical arrangement, and comparing the results, we are convinced that new light would be thrown on the subject of classification in general. We understand that Mr. Macleay himself, while surrounded with the varied forms of tropical vegetation, has pursued this line of research, and found that the principles which he has already demonstrated to pervade the animal world are equally apparent as regulating the vegetable kingdom. Nothing could aid the progress of the science of natural arrangement, generally, more than the publication of the results which Mr. Macleay has obtained. May we hope that he will not long delay to present them to the world?

June 21, 1837. E. W. B.

ON THE COLOURING MATTER OF THE ANCIENT RUBY GLASS.

BY J. T. COOPER, ESQ.

To Richard Taylor, Esq.

DEAR SIR,

Since the publication of my paper in the Annals of Philosophy, to which Mr. Essex has referred in your No. for June, as relating to the composition of the ancient ruby glass, I have obtained a variety of specimens from various places, which I have also submitted to analysis for the purpose of determining whether they all agree in containing silver as a necessary constituent, which I find not to be the case; but in most instances, and in those which possess the brightest tints, I have invariably found traces of that metal, but in variable quantities.

Another circumstance which had escaped my observation at the time I prepared the communication alluded to, and which accounts for the fact that Mr. Essex states, namely, that the old ruby glass may be painted on and passed through the fire any number of times without altering its colour, arises from the colouring material, viz. the oxide of copper, being inclosed between two layers of the glass, constituting a film of about the two hundredth part of an inch in thickness; and in one instance, that of a piece I brought from Strasburg cathedral which is of a very deep tint, there are two such films inclosed between three layers of the ordinary glass. I may also remark that oxide of iron is invariably to be found; but this, as far as I have been able to determine, does not form any portion of the colouring ingredient, but exists in the common glass, from the impure materials of which it has been made.

I remain, dear Sir, yours very truly,
82, Blackfriars Road, June 15, 1837. J. T. COOPER.

SIR ISAAC NEWTON'S MANUSCRIPTS.

We extract the following statements from the *Morning Chronicle*, to the Editor of which they are addressed.

Sir—A paragraph appeared in yesterday's *Chronicle* stating that " the Council of the Royal Society have lately purchased from the descendants of Sir Isaac Newton all the letters, manuscripts, and various unpublished documents left behind him at his death by that illustrious philosopher," and that " these valuable papers are placed in the hands of a very eminent person, and such ~~of~~ them as are important to science will shortly be given to the public." As the writer of this paragraph appears to have been completely misinformed, and the subject is one in which the scientific world feels a strong interest, you will probably allow room for the following brief statement of the facts: —

On the death of Sir Isaac Newton all his manuscripts and papers fell into the possession of Mr. Conduit, whose only child, a daughter, married into the Lymington family; from her they have descended by inheritance to the present Earl of Portsmouth, and are now in the custody of His Lordship's family. It may therefore be presumed that no intention of *selling* them has ever been entertained; at all events, no purchase of them has been made by the Council of the Royal Society, nor has that learned body acquired possession of them, or control over them, in any way whatever.

The circumstance of the Newton MSS. having been locked up from the public inspection has, however, long been matter of regret to scientific men; and the recent publication of Flamsteed's correspondence, in which the conduct of Newton, as President of the Royal Society, is so bitterly censured, has given a new and extraordinary interest to all matters connected with his personal history. It is reasonable to expect that a complete vindication of Newton's character from the aspersions cast on it by the irascible and prejudiced astronomer will be found among those papers. Mr. Baily, indeed, in his Life of Flamsteed, says he can state most decidedly that they contain many documents and much information connected with Newton's life and pursuits that are now highly interesting, and not generally known. The announcement, therefore, that the MSS. are now "placed in the hands of an eminent person," with a view to their publication, will be hailed with satisfaction by every friend of science and admirer of Newton, provided they are to be published in an authentic and unmutilated form. But as considerable feeling has already been displayed in connection with this subject, it would be far more satisfactory if the selection were made by a committee of competent persons, acting under the authority of a scientific body, and not left to the discretion of any individual, however eminent, especially as the original documents are not accessible to those who desire to investigate for themselves. Government, it has been stated, some time ago, expressed a willingness to print them at the public expense. It is greatly to be regretted that this proposal was not agreed to. Controverted points in the history of science cannot be set at rest, nor will the reputation of Newton be consulted, by the publication of extracts, however copious and impartially made. The MSS. form a voluminous mass; it is not, therefore, to be expected that they will be printed entire; but, on the other hand, it will be a just subject of national reproach if the correspondence of the most illustrious individual who ever adorned our country is garbled for the purposes of a bookmaking speculation. A. B. —*Morn. Chron.* June 21, 1837.

Sir Isaac Newton's Manuscripts.—[From a Correspondent.] An erroneous statement having found its way into the newspapers, respecting the purchase of Sir Isaac Newton's MSS. by the Royal Society, we are authorized to state that it has no foundation whatever. In consequence of Sir David Brewster being at present engaged in a large work on the life, writings, and discoveries of Sir Isaac Newton, he was kindly permitted by the trustees of the Earl of Portsmouth to examine the valuable collection of MSS. at Hursbourne Park. With the assistance of H. A. W. Fellowes, Esq., the accomplished nephew of Lord Portsmouth, many interesting and important letters and papers were discovered, which not only throw much new light on the early life and studies of our immortal countryman, but tend to refute the groundless rumours respecting a temporary derangement of his mind in 1692, and to exalt, in the highest degree, his moral and intellectual character.—*Ib.* June 27.

ANALYSIS OF CITRIC ÆTHER. BY M. MALAGUTI.

The process recommended to obtain this æther is the following: take 90 parts of crystallized citric acid, 110 of alcohol of sp. gr. 0·814, and 50 of concentrated sulphuric acid. Put the citric acid, powdered, and alcohol into a tubulated retort, then add the sulphuric acid in small portions. Heat the mixture gradually to ebullition, and stop the process, when a very sensible disengagement of sulphuric æther occurs, which happens when about one third of the volume of the alcohol employed is distilled; the residue is to be removed from the retort, and twice its volume of distilled water is to be added to it; an oily matter almost instantaneously collects at the bottom of the vessel, which is citric æther; it must be repeatedly washed with cold water, and afterwards with a dilute alkaline solution. When the liquid which floats upon the æther leaves no residue on drying, the washing is to be discontinued, and the æther is to be dissolved in alcohol; this solution, which has considerable colour, is to be digested with pure animal charcoal: it is then to be filtered, and the desiccation is to be finished in vacuo. If $8\frac{6}{10}$ oz. avoirdupoise be employed, the experiment requires only about an hour for its completion, and the product amounts to above $5\frac{1}{4}$ oz. Pure citric æther is liquid, transparent, of an oily consistence, and a yellowish colour. Its smell somewhat resembles that of olive oil, its taste is bitter and disagreeable, and its density 1·142; it is volatile, but the temperature at which it volatilizes is so near that at which it decomposes, that it cannot be distilled without the decomposition of a large portion of it. If it be heated in an open vessel, it emits a very dense vapour, which inflames on the approach of flame, and a coaly residue is left. In close vessels it begins to lose its limpidity at about 248° Fahrenheit, becomes reddish at 518°, and begins to boil and to decompose at about 542°; an oily matter being disengaged, afterwards dilute alcohol, lastly carburetted gases and citric æther [acid?]; the residue is charcoal.

Citric æther is perfectly neutral, leaving no residue after combustion; it is soluble in æther, in weak alcohol, and even slightly so in water. An aqueous solution of citric æther becomes acid after some time, and much more quickly so if heated. If citric æther be boiled with a solution of potash or soda, alcohol is obtained, with citrate of the alkali. Solution of ammonia has no immediate action, nor has the dry gas. Neither barytes nor strontian water render either citric æther, or a fresh solution in water, turbid. Nitric acid dissolves it cold, and if the solution be poured into water, the æther does not separate. When the nitric solution is gently heated, a brisk action occurs, which goes on spontaneously; there is a disengagement of red vapours, and the residue has a smell resembling that of hyponitrous acid. If considerable quantities be acted upon, and the ebullition be long continued, oxalic acid is found in the residue; this residue, which is slightly yellow, becomes of a deep red when saturated with ammonia.

Concentrated sulphuric acid immediately imparts colour to citric æther; it dissolves it cold, but quits it on the addition of water, and the æther is unchanged. The sulphuric solution when heated to about 158°, begins to exhibit appearances of a reaction, which becomes extremely strong as the temperature increases; there is a disengagement of alcohol and sulphuric æther, and the residue is red, transparent, very thick, and soluble in water.

Hydrochloric acid dissolves citric æther cold, as the other two acids do, and quits it on the addition of water. The hydrochloric solution does not exhibit any appearance of reaction; the liquid boils, hydrochloric æther is disengaged; a little alcohol, and no citric æther, remain in the residue.

Potassium put in contact with citric æther occasions disengagement of a gas; but the action stops as soon as the surface of the metal is oxidized, which very soon occurs; whether this action is occasioned by the decomposition of the æther, or of a little water which it may contain, was not determined.

By analysis this æther yielded, nearly,

Hydrogen	7·29
Carbon	51·00
Oxygen	41·71
	100·

If constituted of an equivalent of citric acid 58 and an equivalent of æther 37, it would consist of

Seven equivalents of Hydrogen	7	or 7·37
Eight equivalents of Carbon	48	,, 50·52
Five equivalents of Oxygen	40	,, 42·11
	95	100·

This agrees very nearly with Malaguti's analysis, and perfectly with his symbolic representation of its constitution, the weights representing the equivalents varying slightly.

Ann. de Ch. et de Phys., October 1836.

ON THE COMBINATIONS OF AMMONIA WITH ANHYDROUS SALTS.

M. Rose has examined a considerable number of the compounds which dry ammonia forms with anhydrous salts, volatile metallic chlorides, and oxacids, with the view of ascertaining the general laws to which these combinations are subject.

Their preparation is very simple, but requires time; the ammonia should be passed into a vessel containing caustic potash or lime, and then through a tube filled with caustic potash in small lumps, before it comes in contact with the salt, which is weighed before the operation, the current of ammoniacal gas being continued as long as the salt increases in weight. The combination is usually effected in the cold, and if the substance becomes heated the current of gas must be decreased. The absorption is at first very rapid, but becomes slower by degrees, so much so, that even with those substances which absorb ammonia with avidity, the operation often requires two days. We shall describe the general properties without going into the details of all the compounds examined by M. Rose.

The sulphates of manganese, zinc, copper, nickel, cobalt, cadmium and silver, and the nitrate of the last metal, absorb ammonia; on the contrary, the sulphates of magnesia, nitrates of soda and barytes, phosphate of copper and bichromate of potash do not unite with this alkali. Among the combinations of chlorine with the metals whose oxides are basic, there are many which act with ammonia in a precisely similar manner to the oxacid salts; such are the chlorides of calcium, strontium, copper, nickel, cobalt, lead, silver, the proto- and perchloride of mercury, protochloride of antimony, the perbromide, periodide, and cyanide of mercury. The cyanuret of iron and potassium does not absorb ammonia, nor is it when cold at all acted upon by it. From his numerous researches Mr. Rose draws the following conclusions: many oxides, and many chlorides whose oxides form bases, can combine with ammonia, but there are some which do not possess this property although they resemble the former class in many points. When an anhydrous salt unites with ammonia it always forms a determinate compound. Salts which agree in many of their properties absorb ammonia often in the same, very often also in different proportions, so that the combinations of anhydrous salts and ammonia do not come under any law so constant as to admit of an *à priori* calculation of their relative proportions in a compound. Ammonia acts with anhydrous salts and the metallic chlorides analogous to them, as an extremely feeble base, abandoning almost the whole of them, mostly or altogether, when they are exposed to the air, or a very gentle heat: the only exceptions are combinations with perchloride and perbromide of mercury which do not give off ammonia when heated, which property necessarily places them amongst a particular class of ammoniacal compounds.

The combinations of ammonia with the oxacid anhydrous salts and the analogous metallic chlorides present a striking resemblance to the compounds which the same salts form with water. Thus water does not combine with all salts, and even amongst those whose properties are extremely analogous some will be found containing and others not containing water. Thus chloride of calcium absorbs a large quantity of ammonia whilst chloride of barium combines with none; sulphate of lime also unites with water of crystallization, whilst sulphate of barytes does not contain any. Besides, the water of crystallization exists in a determinate proportion in all its combinations with salts; yet salts possessing very similar properties often combine with very different proportions of water. Finally, water in its combination with salts may be considered as a base, although a very weak one, and which can be usually expelled by a moderate heat.* — *Jour. de Phar.*, Jan. 1837.

ON THE OXALHYDRIC ACID OF M. GUÉRIN.

M. Erdmann of Leipsick considers that the composition of the acid obtained by the treatment of sugar (acide oxalhydrique of M. Guérin, artificial malic acid,) is the same as tartaric acid; for if a solution of this acid is allowed to stand for some time it is converted into common tartaric acid, the oxalhydrates are converted into common tartrates, and the crystallized oxalhydrate of ammonia described by M. Guérin is a tartrate of that base. A further examination has proved that this acid is identical with the isomeric tartaric acid of M. Braconnot obtained by the fusion of common tartaric acid. M. Liebig has procured well-formed crystals of tartaric acid from an acid syrup remaining after the preparation of oxalic acid from sugar and nitric acid which had been left to itself for a length of time. These researches will remove all the anomalies of the oxalhydrates of M. Guérin.—*L'Institut,* April, 1837.

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* See Professor Graham on this subject, Lond. and Edinb. Phil. Mag., vol. vi. p. 327, *et seq.*, vol. vii. p. 400.

NATIVE IODIDE OF MERCURY.

M. Del Rio having noticed on some specimens of seleniuret of mercury in the collection of *L'École des Mines* of Mexico, some particles of a reddish brown colour, submitted them to the blowpipe, when they afforded evident indications of iodide of mercury, agreeing in its characters with the artificial iodide.—*Journ. de Chim. Méd.* Feb. 1837.

CARBOMETHYLATE OF BARYTES.

This salt occurs in white shining plates, is soluble in water, and is not decomposed by exposure to the atmosphere nor *in vacuo*: its composition according to MM. Dumas and Peligot the discoverers is indicated by the formula

$$\mathrm{Ba\,O + C^2\,O^2 + C^2\,H^3 + C^2\,O^2 + O\,H.}$$

Dissolved in water it spontaneously decomposes even at common temperatures, into carbonate of barytes, carbonic acid, and pyroxylic spirit (bihydrate of methylene or carbo-hydrogen). This decomposition is accelerated by heat, and is complete before the aqueous solution is raised to ebullition.—*L'Institut,* 20th March.

ANALYSIS OF GADOLINITE. BY MR. A. CONNELL.

Yttria	36·54
Glucina	5·90
Protoxide of cerium	14·31
Protoxide of iron	14·14
Silica	27·10
Lime	·45
	98·44

In the statement of this analysis in the New Edition of Phillips's Mineralogy, the silica has been, by a typographical error, omitted.

METEOROLOGICAL OBSERVATIONS FOR MAY 1837.

Chiswick. — May 1, 2. Fine. 3. Hazy. 4—7. Very fine. 8. Rain. 9. Cloudy; fine, clear and cold. 10. Cloudy and cold. 11. Fine. 12. Drizzly. 13. Fine: rain. 14. Hazy: cloudy and fine. 15. Cloudy, and cold: fine. 16—18. Fine. 19. Cold rain. 20, 21. Slight rain. 22. Cloudy and cold: hail showers: stormy with rain. 23. Cloudy. 24. Slight haze: frosty at night. 25—29. Very fine. 30. Fine: rain at night. 31. Slight rain.

Erratum.—In the last Number, the minimum average of the barometrical pressure should be 29·777 instead of 29·868, which it will be observed is the average of the max. observations.

Boston.—May 1. Cloudy. 2—5. Fine. 6. Cloudy. 7. Fine. 8. Cloudy: rain early A.M.: rain P.M. 9. Cloudy. 10. Fine: rain and hail A.M. and P.M. 11. Fine. 12. Cloudy. 13, 14. Fine. 15. Fine. rain P.M. 16. Cloudy. 17, 18. Fine. 19, 20. Cloudy: rain early A.M.: 21. Cloudy. 22. Fine: rain early A.M. 23. Fine. 24. Cloudy. 25—28. Fine. 29. Cloudy. 30. Cloudy: rain and hail with thunder and lightning P.M. 31. Fine.

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

Days of Month. 1837. May.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Max.	Max.	Min.								
M. 1.	29·656	29·848	29·720	29·06	55·3	48·0	59·3	64	44	55·0	SW.	SW.	W.	·063	...	·18	48
T. 2.	29·902	29·973	29·928	29·34	54·5	47·2	60·8	64	43	54	SW.	W.	calm	...	...	...	49
W. 3.	29·739	29·839	29·687	29·27	55·3	50·8	61·4	61	42	58	E.	E.	calm	...	...	...	48
● Th. 4.	29·779	29·955	29·830	29·30	46·2	44·2	61·2	58	39	52	NNE.	N.	calm	...	...	...	43
F. 5.	29·983	30·094	30·059	29·42	51·2	43·2	57·2	61	32	52	W.	N.	NW.	...	...	...	42
S. 6.	30·088	30·164	30·144	29·65	49·0	43·2	58·2	59	32	48·5	W.	NE.	calm	...	...	...	42
⊙ 7.	30·019	30·108	29·973	29·57	47·7	41·4	55·2	59	41	52	NNE.	NW.	calm	...	...	...	43
M. 8.	29·776	29·859	29·647	29·27	50·3	45·2	53·5	56	37	47	S.	S.	W.	..	·28	·12	44
T. 9.	29·581	29·659	29·636	29·22	43·6	38·4	52·2	50	30	41	W.	NE.	calm	·291	·06	·17	37
W. 10.	29·595	29·821	29·640	29·24	43·2	35·5	48·2	52	33	45	SW.	N.	calm	...	·10	...	36
Th. 11.	29·942	30·041	30·006	29·55	44·6	37·8	51·0	55	40	47	SW.	W.	calm	·033	·01	·06	38
☽ F. 12.	29·808	29·883	29·814	29·40	46·4	42·8	51·2	61	33	49	SSE.	S.	NW.	...	·06	...	42
S. 13.	29·776	29·883	29·795	29·32	54·3	44·0	60·4	58	35	54	S.	S.	calm	·147	·10	...	45
⊙ 14.	29·745	29·990	29·786	29·36	45·5	42·3	55·2	59	42	56	SSW.	SE.	calm	·183	·20	...	43
M. 15.	30·061	30·228	30·128	29·52	47·9	43·4	56·3	58	42	54·5	SSE.	N.	NE.	·683	...	...	45
T. 16.	30·265	30·361	30·329	29·83	46·7	43·8	56·0	68	36	46	SSE.	N.	calm	...	...	·12	43
W. 17.	30·239	30·355	30·167	29·70	58·2	45·0	59·3	71	46	63	SW.	NW.	NW.	...	...	...	49
Th. 18.	30·124	30·211	30·079	29·63	51·6	48·2	68·6	56	40	50	NE.	N.	NE.	...	...	...	44
F. 19.	30·034	30·119	29·813	29·60	45·3	44·4	54·2	54	34	47	N.	NE.	E.	...	·01	·09	42
○ S. 20.	29·939	30·018	29·941	29·47	42·5	40·2	52·4	53	36	46·5	N.	NE.	NE.	·058	·01	·06	42
⊙ 21.	29·692	29·772	29·695	29·30	46·5	44·0	50·3	50	35	48	SW.	NE.	calm	...	·12	...	40
M. 22.	29·731	29·893	29·801	29·27	45·6	40·6	51·2	52	36	46	NW.	N.	N.	·166	·03	·08	39
T. 23.	29·918	29·999	29·987	29·46	45·8	40·2	51·3	58	30	49·5	NE.	NE.	calm	...	...	...	40
W. 24.	29·907	29·995	29·893	29·44	51·8	42·8	60·3	61	30	50	SW.	SW.	calm	...	...	...	43
Th. 25.	29·788	29·868	29·856	29·25	52·4	43·7	58·3	64	44	56	SSW.	S.	calm	...	...	...	44
F. 26.	29·837	29·922	29·897	29·33	58·7	48·8	63·6	66	33	59	SSW.	S.	calm	...	...	...	49
☾ S. 27.	29·925	30·033	30·005	29·40	56·4	45·2	60·5	71	38	61	SSW.	SW.	calm	...	...	...	48
⊙ 28.	29·988	30·010	29·979	29·41	57·7	50·6	65·4	70	49	63·5	E.	E.	calm	...	...	...	49
M. 29.	29·930	30·026	29·994	29·27	59·7	53·0	61·6	74	45	62	SW.	S.	calm	...	...	...	52
T. 30.	30·005	30·101	30·008	29·35	58·2	49·3	64·6	65	41	57	SW.	W.	calm	...	·05	...	51
W. 31.	29·978	30·111	29·982	29·34	55·2	48·4	66·3	66	54	60	SW.	SW.	calm	·050	·04	·20	52
	29·895	30·004	29·910	29·40	50·6	44·4	57·6	60·45	38·45	52·5				Sum 1·674	1·07	1·08	44·3

THE
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AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

AUGUST 1837.

XVII. *On the Use of Sulphate of Copper for exciting Voltaic Electricity, and on the Employment of Iron in the Construction of Batteries. By* ANDREW FYFE, *M.D., F.R.S.E., Lecturer on Chemistry, Edinburgh.*[*]

THE action of metals with metallic salts has been long known, but it is only lately that the latter have been used in electric arrangements, and chiefly for electro-magnetic purposes. Aware that sulphate of copper was easily decomposed by zinc, I was induced some time ago to try it, when used as the exciting fluid in the galvanic battery, and found that in its action it is far superior to the acids in common use. It is however unnecessary for me to relate the experiments that were performed, as I find that it has been made the subject of investigation by others, particularly by Mr. Faraday, the results of whose labours are already, so far, before the public. Since first using the sulphate of copper, I have been led to inquire not only as to its powers compared with other fluids, but also whether other combinations of that metal would not act equally well, or perhaps more powerfully, and it is to the use of these, in comparison with acids of different strengths, not only in batteries as usually constructed, but likewise in other modifications of them, that I wish to advert.

In the tenth series of his experimental researches, Mr. Faraday has shown, that in voltaic arrangements the electrolyzation depends, along with other causes, on the kind of trough

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[*] Communicated by the Author.

employed. In a voltaic circuit the forces which make the instrument active are of two kinds, the one local, the other that which is transmitted along the object by which the communication is established. Of course it is of the utmost consequence in all voltaic arrangements, to have as much as possible of the latter made to circulate, because it is by means of it that electrolytic action is effected. By using the trough recommended in the paper alluded to, Mr. Faraday found, that for the same destruction of zinc, the electrolytic action, in other words the quantity of electricity transmitted through the electrolyte, was greater than when a trough of ordinary construction was used. In the trough mentioned, which by Faraday is considered superior to the best previously in use, viz. that with double coppers, in which the cells are insulated, there was, even when acting under favourable circumstances, a considerable waste of electricity. In a perfect trough this would not be the case, but as in all arrangements now in use it has been found to be so, owing to the interfering powers, which prevent the transference of electricity from one end to the other, it is desirable to discover means by which it may be prevented, so that if possible there should be no waste of the electrolytic agent, provided at the same time we can do so by means equally œconomical with those commonly resorted to; and I trust that the experiments now to be stated will show that this is, to a certain extent, within our reach.

The trough generally employed in the following experiments consisted of thirty copper and zinc plates, on the principle of the *couronne des tasses;* the copper three inches square, the zinc three inches long and one and a half wide. It was capable of holding sixty ounces of fluid. The measure of the quantity of electricity conveyed through the electrolyte was of course that pointed out by Faraday; using in all the experiments solution of sulphate of soda, always of the same strength, in the volta-electrometer, and electrodes of the same size. The experiments were occasionally varied by sometimes collecting both gases, and sometimes only one: 25 degrees of the volta-electrometer were equal to one cubic inch.

Oil of Vitriol.—When diluted oil of vitriol is used, the loss of electricity is considerable, and depends much on its state of dilution. 163·3 grains by weight, when mixed with sixty ounces of water, discharged 14 measures of hydrogen in the volta-electrometer. 490 grs., that is, three times as much, diluted with the same quantity of water as above, discharged very nearly three times as much gas; but when 1470 grs. were used, I did not find that the amount of decomposition was increased; in fact, they yielded only 40 measures of gas, or 1·6 cubic inch, which was the quantity discharged by 490 grains.

When 490 grs. of the acid are used in the trough with thirty plates, these by their action should in each cell excite a decomposition, which should evolve 15·75 cubic inches of hydrogen, and of course the same quantity in the fluid electrolyzed; whereas only 1·6 cubic inch was evolved. When three times as much acid was used, the quantity of gas disengaged was the same; so that the loss of electricity was in that case very great, the gas evolved being to that which should have been evolved, provided the whole of the electricity were transmitted, as 1·6 to 47·25 or as 1 to 29·5: of course, these and the following experiments are not given with the view of showing the actual amount of decomposition that may be effected by sulphuric acid, because much depends on the kind of trough, and other circumstances. But as the circumstances under which they were conducted were generally the same, they may be considered as pointing out under similar circumstances the comparative amount of decomposition.

Sulphate of Copper.—In the experiments with oil of vitriol 490 grs. were used as being the equivalent compared to hydrogen as 10. As blue vitriol is a *bisulphate* and its equivalent number 2500, it was necessary to employ 1250, or only half of that number of it in trying its electrolytic power compared to that of oil of vitriol under similar circumstances; but other quantities were also used, so as to ascertain the power of solutions of different strengths. 1250 of the salt dissolved in sixty ounces of water yielded 180 measures of hydrogen. Now the gas given off by using 490 of oil of vitriol, and of course 400 of sulphuric acid, was only 40; so that a quantity of sulphate of copper, which contains the same proportion of acid, and of course causing the same destruction of the plates of the trough, yielded 4·5 times as much. When a weaker solution was used, a proportionally greater quantity of gas was set free. 416·6 grs. or ⅓ yielded 82, which multiplied by 3 = 246, that is, rather more than six times the quantity given off by a similar proportion of oil of vitriol. 246 measures of the volta-electrometer are equal to 9·6 cubic inches, which deducted from 15·75 leaves 6·15, so that the loss amounted to 39 per cent. When blue vitriol is employed the action is at first by no means energetic, but it gradually becomes more and more so for about an hour or so, after which it becomes weaker; but it continues for a much longer time than when oil of vitriol is used, the action of which is in general over in less than an hour, whereas that of the other continues for eight or ten hours, indeed sometimes much longer, though of course very feeble. I have occasionally observed the trough in action at the expiration of thirty-six hours.

Nitrate of Copper.—The electrolytic action of nitrous acid, as is well known, being more powerful than that of sulphuric acid, naturally led me to try the effect of nitrate or rather *binitrate* of copper in the cells of the trough. Using half of its equivalent, I found that the quantity of gas in the volta-electrometer, was much greater than when sulphate was employed, showing that by means of it there was a greater transmission of electricity. But as the use of the nitrate is precluded, owing to its expense, I was induced to try what effect the addition of other salts, such as a nitrate, would have, either by causing the decomposition of the sulphate, and its conversion into nitrate, or perhaps by increasing the conducting power, and consequently admitting of the more ready transmission of the electrolytic agent.

Sulphate of Copper and Nitrate of Potass.—Nitre was the salt to which I first had recourse, two equivalents of which are necessary for the decomposition of one of blue vitriol. In using this mixture I found that the power was increased, but the results were not always the same; in some they were as 40 to 57, in others as 40 to 53. Using 1250 of blue vitriol and 1040 of nitre, dissolved in sixty ounces of water, the gas given off amounted on an average to 330, or 13·2 cubic inches. The quantity that ought to be given off, supposing the whole of the electricity transmitted, is 15·75, so that there was in this case a loss of about 16 per cent., whereas when blue vitriol alone was used, under similar circumstances, the loss amounted to 39 per cent. With oil of vitriol the loss was 90 per cent.

Sulphate of Copper and Sea Salt.—Having found the increase in electrolytic action of the sulphate by admixture of nitre I was next induced to try it with sea salt, using them in the proportion of 1250 to 600. When these quantities were employed dissolved in sixty ounces of water, the gas collected in the volta-electrometer very nearly amounted to that given off by the mixture of sulphate and nitre. It was generally about 310, compared to the other as 330; so that it yielded 12·4 cub. inch. The deficiency therefore was 3·35 or 21 per cent.

Though by the use of sulphate of copper, particularly when mixed with other salts, there is little loss of electricity, it is of importance to ascertain their expense, compared with that of oil of vitriol, as indicated by the above experiments. The commercial price of blue vitriol is to that of oil of vitriol as

4 to 1, and their equivalents nearly as 3 to 1, taking only half of the atomic number of the former, because blue vitriol is a bisulphate; so that for equivalents of sulphuric acid the price is nearly as 12 to 1, but the former gives off six times as much gas as the latter, so that for equal electrolyzing power the price would be nearly as 2 to 1; but then, to get the same electrolyzing power requires six times the equivalent proportion of acid, by which of course six times as much zinc must be destroyed, which will add materially to the expense; in other words, comparatively diminish that incurred by the use of the sulphate of copper. When nitre is mixed with the sulphate the electrolytic action is as 8 to 1, so that the slight additional expense of the nitre, without adding to the expenditure of zinc, is more than counterbalanced by the increase of power. Though sea salt does not increase the electrolytic action of the sulphate so much as nitre does, yet considering its cheapness compared with it, it is the most œconomical of the substances tried. Thus 490 of oil of vitriol yielded 40 of gas, and 1250 of sulphate of copper + 600 sea salt gave 310, consequently 490 with the requisite proportion of salt would yield 120, or three times as much as the oil of vitriol would do; so that the expense of the fluids would be about 4 to 3. But as eight equivalents of acid are required to yield the 310 of gas, and consequently eight equivalents of zinc consumed, in other words, as only $\frac{1}{8}$th of the quantity of zinc is destroyed, for getting the same electrolytic action when the saline solution is used, though the price of the fluids is as 4 to 3, the expense would in all, even with the addition of the salt, be only about half of that incurred by using the oil of vitriol.

The above remarks regarding the comparative expense of the materials mentioned, it must be borne in mind, apply to the electrolytic power when the troughs which are in common use are employed. It is different when the trough recommended by Faraday (Phil. Magazine, February 1836, Third Series, vol. viii., p. 114,) is used. According to him, when acting with nitrosulphuric acid it decomposed one equivalent of water, at the expense of 2·21 equivalents of zinc, in each plate, and of course of a corresponding quantity of acid; whereas were the whole of the electricity conveyed through the electrolyte, 2·21 of water ought to have been decomposed; so that there was a loss of about 55 per cent. of the electrolytic agent. It has been already mentioned that with sulphate of copper and nitre, while the quantity of water which should have been decomposed would have yielded 15·75 cubic inches of hydrogen, there were given off only 13·2, and with the mixture of sulphate and sea salt, only 12·6 cubic inches, making the loss in the former amount to about 16, and in the latter to about 20 per cent.

But in this case, where nitric acid was added to the sulphuric, with the view of increasing the power, the expense was considerably increased. When sulphuric acid alone was used, there was much less electrolyzation indicated by the volta-electrometer. For each equivalent of water decomposed, 4·66 of zinc in each plate, with of course a corresponding quantity of acid, were consumed; so that the loss of electrolytic power amounted to about 78 per cent. The loss was therefore less; in other words the electrolyzation was greater. But this, though it alters the comparative expense, yet it does so in a small degree; because, if the electrolytic action in this trough is greater with sulphuric acid, it will be greater also with the metallic solutions. But even allowing that it is so to a trifling extent, it has been shown that the comparative powers of oil of vitriol and of the mixed salts are as 1 to 8, and their expense as 2 to 1; so that if we suppose the electrolytic power with oil of vitriol in the experiments I have recorded as amounting to about 10 per cent., and that in Faraday's trough, with the same acid, as about 22 per cent., or rather more than double, this would reduce the power of the same trough with the mixed salts, compared to its power with oil of vitriol, to about as 4 to 1, which would make the expense for the same amount of electrolyzation very nearly the same in both cases. Viewing it in this light, still there would be a saving, because the same electrolytic action would be obtained, with less destruction of the trough, and consequently the trouble and expense would thus be diminished.

The peculiar condition into which iron is brought by the action of nitric acid, as discovered lately by Schœnbein, and proved also by Faraday and others*, seemed to exclude the prospect of its being used in the construction of voltaic troughs; but considering the ease with which it is acted on by sulphate of copper, I was induced to try it, in the hopes that the state of inaction does not depend on the want of power in the iron to convey the electric agent, but on some other cause, brought into play by the action of the acid. I accordingly had a trough constructed of copper and sheet iron, the plates being of the same number and size as those in the zinc battery. When diluted acid was poured into it, a smart electrolytic action took place, accompanied by the discharge of gas in the volta-electrometer; but in the course of a short time the action became feeble, and though it went on for some time,

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* See various papers in our last three volumes.—Edit.

yet it was very faint. The result was different when sulphate of copper was used. The electrolyzation commenced almost the instant that the trough was filled, and continued in the same way as when zinc was employed; indeed I could observe very little difference as to energy of action, and to the time that it continued, with this exception, that in general there was rather less gas evolved in the volta-electrometer. That evolved by the action of 1250 of the sulphate in the zinc trough being 180, that afforded by the same quantity in the iron trough was 173, or about $\frac{1}{26}$th part less. When the sulphate was mixed with nitre and with sea salt, there was an increase in power, similar to that when using them in the zinc trough. When therefore blue vitriol is to be used in voltaic arrangements, iron may be employed instead of zinc, with very nearly the same electrolytic power. It has been already mentioned that when the blue vitriol is mixed with nitre, but more particularly with sea salt, the expense is only about one half of that incurred by the use of oil of vitriol. If iron be employed in the construction of the trough, the expense must of course be still further reduced, sheet iron being only about half the price of sheet zinc. Though in many cases it may be thought advisable to use sheet iron, there is no necessity for always having recourse to it. I have found that cast iron answers nearly as well; and when the troughs are to be long and much in action, it may perhaps be more œconomical to use it, as there is always expense incurred by the repeated soldering of new iron plates to the copper ones; besides cast-iron is cheaper than the other.

Before using the trough when constructed of iron, it is always necessary to wash the iron plates with diluted sulphuric acid, which may be done before they are soldered to the copper, or after it, by placing them in the trough filled with the acid, and leaving them there for a few minutes.

Considering the cheapness of the materials used and the comparatively little waste of electrolytic power, I conceive that the trough as thus recommended may be employed advantageously for many purposes of decomposition. Supposing the iron trough to be used with sulphate of copper, metallic copper and green vitriol are the products in the trough, both of which may be turned to account; so that if by the electrolytic action of a powerful battery, a valuable ingredient could be obtained from the electrolyte, the decomposition might in this way be advantageously accomplished.

There is one advantage attending the use of metallic solutions. The adhesion of the precipitated copper does not seem to impede much the action in the trough, and consequently to retard the progress of the electrolytic agent. A trough which has been in action and consumed the whole of the fluid in its cells, so as to have its plates covered with flocculent precipitated copper, when emptied and again filled with the metallic solution, soon again comes into action, and the electrolytic action is the same, or nearly so, as at first. When the plates are allowed to dry, and are in that state put into the trough with the metallic solution, though there is at first no action, yet it begins in the course of a short time, and goes on effecting the electrolyzation of the electrolyte as before.

Though the trough acts in this way, it is better to wash off the precipitated copper, because the whole surface of the metal is thus exposed; whereas when not washed off, unequal surfaces are exposed in the different plates, from part of the precipitate above being carried off when the trough is emptied, and thus the action may not be the same in all the cells. When iron is used, the plates after being washed ought *to be dried*, before being set aside, because otherwise they become covered with rust, which retards the action.

As it is of the utmost consequence to avoid as much as possible expense in the fitting up of the trough, I have been led to adopt a modification of that in common use, which is easily renewed with little expense. Instead of using the plates soldered together, the copper plates are fixed into the sides of the trough, thus forming the partitions, on each of which is suspended an iron or zinc plate, by bending the upper extremity, so as to form a sort of hook. This trough acts as powerfully as the others in common use, and it has this advantage, that the iron or zinc plates are easily removed, and cleaned, when required, and are, when destroyed, easily replaced by others. It is necessary to dry the plates after being washed, to prevent oxidation at the upper extremity, by which the metallic contact would be rendered imperfect.

XVIII. *On the definite Combinations of Sugar with the Alkalies and Metallic Oxides.* *By* LOUIS HUNTON, *Esq., F.G.S.*[*]

WHEN lime is added to a solution of sugar it is dissolved in considerable quantity. Dr. Ure in the last edition of his Dictionary says: "Sugar dissolved in water at the temperature of 50° is capable of dissolving half its weight of lime." This I believe will be found too large a proportion, for after repeated trials I find its composition the same for every temperature between 50° and 130° at which the solution is made

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[*] Communicated by the Author.

and filtered; and from which solutions carefully evaporated under 180° (the compound being insoluble at higher temperatures), and then dried at 212°, 100 grs. give from 22½ to 23½ per cent. of lime. Now if, with Berzelius, we consider sugar dried at 212° as $\underline{C^{12}H^{10}O^{10}}+HO = 171$, from 23 per cent. we shall have 51·07 as the number for lime, too small for two proportions; but should the lime be as a hydrate, instead of 23 we shall have 30·4, leaving the sugar 69·6 per cent.; or for 171, 56·7 as the number for lime, equal to two proportions. A better way to obtain the composition, on account of the difficulty of preventing the formation of a little carbonate, is to add alcohol to the filtered solutions, and then wash the curdy precipitate with proof spirit; this precipitate when dried at 212° gave 22·65 per cent. of lime = 29·93 hydrate, which gives 55·27 as the proportion to one of sugar; hence

Saccharate of lime $(\underline{C^{12}H^{10}O^{10}+HO}+\underline{2\,CaO+2\,HO}) = 245$.

If a solution of the saccharate of lime be added to hydrated deutoxide of copper, we obtain a bright blue solution, much resembling the ammoniuret; this evaporated under 160° leaves a crystalline substance permanent in the air, which dried at 212° has the following composition: 10 grains, after accounting for a little carbonic acid absorbed during evaporation, gave

Lime	1·63	=	2·154	hydrate.
Deutoxide	2·33	=	2·586	do.
			4·74	

leaving 5·26 for the sugar, but which to be in proportion to the lime should be 4·98; the ·28 may probably have arisen from a little unexpelled water: thus we may consider the above as

1 Sugar	=	171	=	57·2
$2\,CaO+2\,HO$	=	74	=	22·15
$\underline{CuO^{2}+HO}$	=	89	=	26·65
		334		100·

The relations of this calcareo-saccharate to oxygen are rather singular; if we heat a little of the solution to 160°, a flaky blue precipitate is separated, that entirely redissolves again on cooling, which could not be the case had any protoxide been formed. This being apparently at variance with some of the results obtained by M. Becquerel in his experiments on the varieties of sugar (in which he shows that hydrated oxide of copper agitated with sugar and an alkali is dissolved, but precipitated again as protoxide when boiled,) I was induced to examine further, and found that though the solution when heated in an open shallow vessel, as a watch-glass, may be alternately raised to ebullition and cooled, for several times in succession, without forming any permanently insoluble precipitate, yet if heated but once, or twice at the furthest, in a deep narrow-mouthed vessel, as a test tube, a separation of protoxide takes place. The presence of free or uncombined sugar is also of much consequence, for if we add a little to any of the saturated solution and then heat, insoluble protoxide is formed even in a watch-glass; on the contrary, a free alkali appears to retard the deoxidation in the test tube, and to prevent the formation of a little which takes place after four or five heatings in a watch-glass, and resulting from the formation of a little carbonate of lime, and consequent setting free of sugar.

If instead of boiling, a few test tubes are filled with the solution, a result variable with the difference of exposure and presence of free sugar is obtained; thus,

In open tubes.	In closed tubes.
No. 1. Solution alone.	No. 4. Solution alone.
— 2. Do. and free sugar.	— 5. Do. and free sugar.
— 3. Do. and free potash.	— 6. Do. and free potash.

In the open tubes, No. 1 did not form any protoxide until it had stood a week, when a slight deposition, accompanied by carbonate of lime, took place; No. 2 formed protoxide after twelve hours, and gradually increased it; whilst No. 3 did not form any for a month, when a trace appeared, and at the same time crystals of carbonate of lime. But in the closed tubes, after twelve hours' standing, protoxide had formed in all; that in Nos. 4 and 6 was slight, whilst that in No. 5 was considerable. Carbonate of lime did not appear until they had stood a week.

In order to ascertain the exact action of sugar on the oxide of copper, a portion of the hydrate having been agitated with a syrup in the cold for three days without any effect was brought to boil, and though none was dissolved, yet the hydrate was prevented parting with its water and becoming brown as in ordinary cases; and continuing the boiling the peroxide was slowly converted to the yellow one, the change being complete after some hours; and the sugar, though having thus partially deoxidized the copper, appears in proximate properties to have itself suffered no change. If prior to the boiling the smallest quantity of potash or other alkali had been added, a part of the hydrate would have been immediately dissolved, acted on by the free sugar present, and precipitated as protoxide, the alkaline saccharate then renewing its action on the hydrate, which is no sooner dissolved than deoxidized by excess of sugar, the whole being thus quickly converted to the yellow oxide; and in this experiment copper is always present in solution, proportionate to the quantity of alkaline saccharate.

It is worthy of remark, that if the deutoxide has been previously deprived of its water and then heated with sugar, it resists the action; at least three hours' boiling did not affect it: but saccharate of lime (or any other alkaline saccharate) boiled with it is capable of dissolving and deoxidizing it, though with more difficulty than the hydrate.

A somewhat similar deoxidizing action occurs when sugar is boiled with acetate of copper, the whole of the metal being gradually deposited of a fine orange-red colour, very much resembling the outer coating of a bar of Japanese copper, and with it may possibly be the metal in a lower state of oxidation than any yet known: the acetic acid going off in vapour, leaves the syrup when cold equally free from acid and oxide.

On substituting an oxide of iron for that of copper it is equally well dissolved by the saccharate of lime, but in this case it is the protoxide; and owing to the metal being already at its minimum, the addition of free sugar causes when boiled no further deoxidation; but this solution is very liable to decomposition either in closed or open vessels, carbonates of lime and iron crystallizing from it. On evaporating and drying at 212°, 10 grains gave

Lime .... = 1·8 = Hydrate ...... 2·38
Protoxide of iron = 1·17 = Do. ...... 1·463

In this case 5 per cent. of carbonic acid had been absorbed during evaporation, which deducted from 10 grs. leaves the composition,

1 Sugar .... = 171 = 58·6
$2\,CaO + 2\,HO$...... = 76 = 26·
$1\,FeO + HO$ ...... = 45 = 15·4
292 100·.

The iron in this calcareo-saccharate, though insensible to ferrocyanates and alkalies, is precipitated by both succinate and benzoate of ammonia.

On adding a precipitated protoxide of lead to the saccharate of lime, a pale amber-coloured crystalline substance is produced on evaporation, 10 grains of which dried at 212°, give

Lime	1·5
Protoxide of lead	3·0

hence the following composition:

1 Sugar	=	171	=	46·72
$2\,CaO + 2\,HO$......	=	74	=	20·22
$PlO + HO$	=	121	=	33·06
		366		100.

Baryta and Strontia Saccharates.—By agitating a syrup with either of the hydrates of these earths, and then with the oxides of copper, iron, or lead, we obtain solutions which behave very much the same as the calcareous saccharates.

Potassa and Soda Saccharates.—When either of the fixed alkalies are united with sugar in their atomic proportions, we obtain solutions which dissolve the hydrated oxides, but which triple solutions continue soluble at 212°, though a further addition of free sugar causes a precipitation of protoxide when the solution of copper is heated.

Loftus near Guisborough, May 21, 1837. LOUIS HUNTON.

XIX. *A Report of the Progress of Phytochemistry in the year 1835, in reference to the Physiology of Plants. By* J. CL. MARQUART.*

[Continued from vol. x. p. 252.]

I EXAMINED with M. F. Nees von Esenbeck† the bloom of the fruit of *Benincasa cerifera*, which consisted mostly of a whitish wax (66 per cent.), of resin (29 per cent.), and of extractive matter (5 per cent.). The first possessed the same properties with regard to solvents as vegetable wax; but was remarkable particularly on account of its high melting point, viz. at 100—120° Reaum. We found as a discriminating character for this wax, by which it may be distinguished from the numerous resins of difficult solution, its reaction with sulphuric acid in the cold, as it is scarcely coloured by it, or not at all if the wax is very pure. We examined in regard to this the so-called Japanese wax of *Rhus succedanea*, the wax of *Corypha cerifera*, and the wax from the seed-lac of *Aleurites laccifera*, which corresponded with the bloom of the Benincasa. According to Boussingault‡ the composition of the wax-like envelope of *Ceroxylon andicola*, which entirely covers that palm, often reaching to the height of fifty feet, is the same

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* From Wiegmann's *Archiv für Naturgeschichte*, vol. ii. part iv. p. 139 *et seq.* Translated by Mr. Francis.

† Buchn. *Repert.*, vol. li. part 3.

‡ *Annales de Chimie et de Physique*, May 1835.

as that of the bloom of the fruit of this Cucurbitaceous plant. The wax of this envelope melts, however, at a much lower temperature, even below 80° Reaum., is little coloured, resembles bees' wax, and has the same elementary composition.

M. Mulder found in the husk or rind of coloured fruits, as those of *Pyrus Malus*, *Capsicum annuum*, *Sorbus aucuparia*, *Cucurbita Lagenaria*, invariably combined with the colouring matter, a wax which was always found to be a pure cerin *. By the experiment before mentioned, M. Du Menil obtained from the bark of *Pinus sylvestris* also 13 *per M.* of a whitish wax, the nature of which was not more exactly determined †.

M. Fr. Nees von Esenbeck and the author examined the milk-sap of several fig trees ‡, in order to find whether the *Lacca in granis* could originate from trees of this genus, as is said, but must declare it to be erroneous; and they were induced to consider, as the only plant producing this remarkable vegetable body, the *Aleurites laccifera*, which belongs to a family the chemical constitution of which allows us to infer the presence of similar resins to that which is contained in seed-lac.

At the same time they examined the milk-sap of *Ficus elastica* from the stem as well as from the young branches; and arrived at the result that the milk-sap of the young branches consists of resin, gum, wax, together with some extractive matter, a salt of lime, and a glutinous resin which was only soluble in æther, not in alcohol, and which they believed to be identical with the substance found by Macaire in *Atractylis gummifera*, and named by him Viscin §. The milk-sap of the old stem contained, on the contrary, caoutchouc instead of viscin, and also the other constituent parts of the milk-sap of the young branches, which is only observable when the milk-sap is allowed to flow immediately into the æther. If it coagulates in the air, traces only of resin, gum, and extractive matter can be separated from the mass which has become caoutchouc or elastic resin (*Feder harz*). The milk-saps of many other species of this genus contained viscin, but in a few cases only had it become caoutchouc in the old stems. M. Zeller ‖ found this viscin also in the berries of *Sambucus Ebulus*. Without doubt the seed-lac (*lacca in granis*) belongs to the coagulated milk-saps which have been drawn from the branches of *Aleurites laccifera*? by some peculiar lac Aphis. I ex-

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* *Bydragen tot de naturkundige Wetenshappen*, d. vii. No. 2.

† *Archiv für Pharmacie*, vol. i. part 1.

‡ *Annalen der Pharmacie*, vol. xiv. part 1.

§ *Mémoires de la Soc. de Physique de Genève*, tom. vi. "Sur la Viscin," &c.

‖ *Würtembergisches Correspondenzenblatt* 1834.

amined it more recently with Fr. Nees von Esenbeck* and found as its contents a peculiar wax which melts at 48° Reaum., and also a series of peculiar resins. The substance called the *insoluble lac-resin* (lac of John) must be considered as representing the caoutchouc in this milk-sap, which resists the common solvents, and is only affected by acidulated alcohol. Nearly related to this is the Beta-resin of the lac, which part the authors consider as insoluble in æther and weak alcohol, and only soluble in absolute alcohol, or that of 90°. By certain manipulations it passes over into the lac of John, and it may well be supposed that it owes its solubility in alcohol to the so-called lac acid which is mixed with it, the nature of which the authors were not able to ascertain on account of its rare occurrence. That part of the seed-lac which is soluble in spirits of wine and æther the authors named the Alpha-resin of the lac, and found the same to be again composed of the first alpha-resin, which is hard, friable, of a gold colour, and which gives with alkalies and oxides of lead beautiful purple red combinations, while the second alpha-resin is a yellow soft resin, and has in a very great degree the peculiar smell of shell-lac.

Scarcely any section of the vegetable formative parts is in greater want of a revision than that of the resins, whose diffusion is so general, and whose varieties almost keep pace with or appear even to surpass, the number of species of plants. We are convinced that a rational examination of this part of phytochemistry would give quite different results. In no analysis does there ever fail to be a resin among the educts enumerated; even in *Sphærococcus crispus* M. Herberger found two different ones, which however, as in many other analyses, are so dubiously characterized, that we may here pass them over in silence without keeping anything important from our readers. Not even is a consistent division followed in the description of these bodies; and it is probable that various mixed and changed substances are frequently cited as peculiar resins. We will therefore in this place only mention a few crystallizing resins, like that which Mr. Landerer† separated from the *Resina Guajaci nativa*, which crystallizes in fine needles, void of smell, soluble in æther and boiling alcohol, and *became by concentrated nitric acid of a lively grass green colour*, and whose spirituous solution had acid reaction. A similar resin was found by Geiger in the bark of the root of *Cornus florida*, the solution of which, however, possessed neither an acid nor an alkaline reaction. In general, those resins which are not very

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* Geiger and Liebig's *Ann. der Pharm.*, vol. xiii. part 3.

† Buchn. *Repert.*, lii. p. 93.

easily dissolved in alcohol are considered as sub-resins, with two of which we are already acquainted as possessing a crystalline nature, those from the Elemi and Euphorbium, which are isomeric or have the same elementary composition, and consist of $2\,(C_{10}\,H_{16}) + O$*. The resin discovered by Boussingault in the wax-like covering of *Ceroxylon andicola* before cited, possesses a similar fundamental composition; it was of a bright white colour and crystalline, melts at above 100° centig., is soluble in æther, æthereal oils, and alcohol. Nees von Esenbeck and the author found also in the bloom of the Benincasa fruit a white crystalline resin soluble in alcohol, and of a bitterish taste.

Th. Martius† showed some time back how to prepare, free from colour, the brown resin from the jalap root, by treating the spirituous solution with animal charcoal; according to his more recent experiments this decolorated resin, having been wrapped in paper, resumed after three years its brown colour. This colouring process appears in the course of time to change the resin in the root to the state in which we receive it; as according to the experiments of Nees von Esenbeck and the author, mentioned in last year's report (p. 220), the resin which had been extracted from jalap roots cultivated in Germany, and preserved only a short time, was scarcely coloured yellow.

During the past year chemists have directed their attention particularly to the *æthereal oils* of the black mustard-seed, and were occupied partly on methods of preparing it, as MM. Hesse‡, Hoffmann§, Fauré ||, Wittstock ¶, and Aschoff**. According to most of the observers, a great quantity of æthereal oil may be obtained from the meal of the black mustard-seed when it has been left for some time in cold water; from which it appears that, as in bitter almonds, not the æthereal oil but the radical is contained in the seed. Fauré observed that tincture of galls and chlorine prevented the evolution of the oil; and Aschoff found that by mixing the mustard meal with water ammonia was evolved. The produce amounted in general to 0·9—1 per cent. of a colourless oil, which is heavier than water = 1·002 at 14° Reaum. It is of the class of those containing sulphur, and does not explode with iodine; it does however with potassium. Ammonia combines with it, and forms

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* Poggendorff's *Annalen*, vol. xxxiii. p. 49.

† Buchn. *Repert.*, vol. li. part 3.

‡ Geiger's *Ann.*, xiv. part 1.

§ *Pharm. Convers-Blatt.* 1835. No. 44.

|| *Journ. de Pharm.* Sept. 1835.

¶ *Berliner Jahrbücher*, xxxv. part 2. p. 256.

** Erdmann and Schweigger-Seidel, iv. p. 314.

a substance which has much resemblance to *Sulpho-sinapisin.*

Bitter almonds and the leaves of the common laurel, from which is obtained an æthereal oil containing a great quantity of prussic acid, but which does not exist as such in the plants, exhibit similar characters. This part of phytochemistry has received, by Liebig and Wöhler's discovery of organic radicals, a direction the influence of which on many doctrines of physiology we have yet to await. We will here only mention as an instance the recent experiments of M. Winckler[*] on the products by distillation of bitter almonds and the leaves of the common laurel, and briefly remark that the æthereal oil of these bodies is a combination of benzoyl (the radical consisting of $C_{14} H_5 O_2$) with hydrogen; at the same time with this benzoyl hydruretted an evolution of a cyan-benzoyl takes place on distillation, which is the cause of the oil of bitter almonds containing prussic acid, and which may be separated from it. Proctor[†] found an oil similar to the oil of bitter almonds in the bark of *Prunus virginiana.*

M. Pagenstecher[‡] extracted from the flowers of *Spiræa Ulmaria* a very remarkable æthereal oil, heavier than water, of a yellow colour, and of a peculiar smell similar to prussic acid. Its peculiar constitution is evident from the property of its spirituous solution, which becomes of a cherry-red colour when mixed with chloride of iron. Caustic alkalies form with it yellow combinations, in consequence of a peculiar acid which accompanies the oil, and by which it is connected with valerian oil, cinnamon oil, and the heavy oil of pinks. M. Löwig[§] examined the oil further, and regards it as the combination of spiroil (a radical of $C_{12} H_{10} O_4$) with 2 M. G. hydrogen, but the spiroilic acid as the combination of spiroil with 4 M. G. oxygen. The *Spiræa* oil solidifies at −20° and boils at +85°.

Several æthereal oils were prepared and closely examined; one from the fruit of *Coriandrum sativum* by Trommsdorff[||]; it was colourless, and the spec. grav. was 0·859. M. Bley[¶] analysed the plant, flowers, and fruit of *Achillea nobilis.* The æthereal oils from the plant and the seeds were alike, of spec. grav. 0·970, and of a butter-like consistence; that however from the flowers was of a thin liquid consistence, and spec.

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[*] Buchn. *Repert.*, vol. lii. p. 289.

[†] *Journ. de Chim. Méd.* 1834.

[‡] Buchn. *Repert.*, vol. ix. p. 337.

[§] Poggendorff's *Annalen*, vol. xxvi. p. 383; and Scientific Memoirs, vol. i. p. 153.

[||] *Archiv für die Pharmacie*, ii. p. 113.

[¶] *Archiv für die Pharmacie*, vol. i. ii. iii.

grav. 0·983. All three did not fulminate with iodine. It is astonishing that the oils from the fruit and the flowers did not coincide more, since it is certain that in the distillation of the flowers M. Bley did not separate the germen. We therefore doubt the correctness of the statements, as in other respects the experiments of M. Bley have been conducted rather carelessly, which has been lately proved by all the analyses of *Achillea nobilis.* M. Landerer[*] prepared from the green part of *Conium maculatum* a small quantity of æthereal oil, in obtaining which no one had before succeeded in Germany. Does the hemlock which grows in southern latitudes (in Greece) form an exception?

James Martin[†] obtained from the leaves of *Cassia marylandica* also an æthereal oil, as did Zeller[‡] from the perisperm of *Abies pectinata.* The latter was limpid, spec. grav. = 0·839; fulminated with iodine, but left potassium unchanged; it therefore contained no oxygen. The æthereal oil from the leaves of *Myrica Gale*[§] solidifies, according to Rabenhorst, at 14° Reaum., and is at 15° a thickish dark yellow mass, which contains 70 per cent. of stearoptin. It does not fulminate with iodine, and has a spec. grav. = 0·876.

Elementary analyses have conducted us in the æthereal oils to very interesting results, and have made known to us, besides those oils accompanied by acids and sulphur and which have been mentioned above, a series of oils containing no oxygen, therefore containing only carbon and hydrogen; and another series containing carbon, hydrogen, and oxygen. To the first series belongs the oil of turpentine, consisting of $C_5 H_8$; and the oil of the black pepper, the juniper oil, the savin oil, and according to Dumas[||] the oil of the fruits of *Citrus medica* and *Citrus limetta* have been found to be isomeric with it. In addition to these, posssesing the same combinations, are the *light* parts of the oils of pink and valerian, which on their separation were also accompanied by a peculiar acid: further, the copaiva balsam oil, the basis of the cajeput oil, and of the turpentine-camphor; the cajeput oil is a hydrate, whose base, like that of the turpentine oil, consists of $C_5 H_8$: the turpentine-oil-camphor is another hydrate of this turpentine oil, which often separates itself from it in crystals. Colophony, copaiva resin, camphor, the caryophylline and lavendel-stearoptin have been acknowledged to be oxides

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[*] Buchn. *Repert.*, vol. iii. part 1.

[†] The American Journ. of Pharm., 1835. April.

[‡] *Archiv für Pharm.*, vol. iii.

[§] *Berliner Jahrbücher*, vol. xxxv. part 2. p. 256.

[||] *Journ. de Chim. Méd.*, June 1835.

of turpentine oil. The camphor oxide obtained by the distillation of the roots of *Iris florentina*, and which is crystalline, laminated, and of a pearly lustre, consists of $C_4 H_8 O$, and is therefore to be regarded as an oxide of oil of roses, $C_4 H_8$. Till now the attempt to isolate the odorous principle from several strongly and agreeably smelling flowers has proved unsuccessful. Among these is the *Narcissus Jonquilla*, which Robiquet* examined, and from which, by extraction with sulphuric æther, he obtained a yellow æthereal oil, which is very volatile, and when once volatilized is not easily condensed. It decomposes very easily, even in closed vessels; it becomes solid, and then scarcely melts at 100° centigr. and is a warty mass void of smell, which separated itself already with the æthereal oil, after evaporation of the æthereal extract. M. Herberger† obtained from the flowers of *Convallaria majalis* a small quantity of a camphor-like substance, in part of a radiated crystalline structure and possessing a very strong smell.

What was formerly considered as benzoic acid in the flowers of *Melilotus* is, according to Guillemette‡, similar to the substance from the tonquin bean which has been named Coumarine, and De Candolle mentions it in his Physiology (p. 352, Paris, 1832) under the hyperhydrogenic substances. It belongs rather, according to our views, to the camphoroids, on account of its volatility; it crystallizes, melts, volatilizes, dissolves in boiling water, in alcohol and æther. According to Henry, coumarine from the tonquin bean, as also from the *Melilotus*, consists of $C_5 H_6 O_2$.

We have in general very little to remark on the fat oils, and confine ourselves to the relation of their occurrence in various plants, according to the observations of last year.

M. Trommsdorff found§ in the fruit of *Coriandrum sativum* 13 per cent. of a fat oil insoluble in alcohol. It was void of smell, grayish green, thickish, and was easily decomposed into almost equal parts of stearine and elaine. J. Martin|| found in the leaves of *Cassia marylandica* a yellow fat oil, and a similar oil was found by Ch. Schreeve¶ in the bark of the root of *Gillenia trifoliata*. Semmola** determined the contents of the white fat oil in the tubers of the root of *Cyperus esculentus* to be 48 per m., and Fleuron found fat oil in the roots of *Astragalus escapus*; M. Zenneck found in his analysis of the fruit

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* *Journ. de Pharm.*, July 1835.

† Buchn. *Repert.* vol. lii.

‡ *Journ. de Pharm.* April 1835.

§ *Archiv für Pharm.*, ii. 2.

|| The American Journ. of Pharm. April, 1835.

¶ *Ibid.*

** *Journ. de Chim. Méd.* 1834.

of *Panicum miliaceum*[*] 4·37 per cent. of a green fat oil, 2 per cent. of which was contained in the pericarp, and 2·37 per cent. in the albuminous body. Millet, therefore, in this respect surpasses the oat and rice.

Th. Martius[†] found in the seeds of *Strychnos Nux vomica* 0·5 per cent. of an oil soluble in alcohol. The fat from the seeds of the East Indian species of *Bassia*, *B. latifolia*, *butyrea*, *longifolia*, of the family of the *Sapoteæ*, which belongs also here, is, according to C. Henry[‡], of a dirty yellow colour, and possesses an aromatic taste and smell. M. Koene extracted from the roots of *Anacyclus Pyrethrum*[§] two fat oils, one of which was soluble in oil of turpentine and alcohol, the other not. In the above-mentioned investigation of the flowers of *Narcissus Jonquilla* M. Robiquet also found an oil of some consistency belonging to this division, smelling something like fish-blubber. The albuminous body of *Abies pectinata* contains, according to Zeller[‖], a drying oil, soluble in alcohol, whose spec. grav. was 0·913; and M. Wurzer found in the same organ of *Pinus pinea* a fat oil, void of smell, and which did not dry up[¶]. J. Cockburn also found in the bark of the root of *Cornus florida*[**] a fat oil soluble in alcohol and in æther.

The acids are a class of vegetable substances which are as widely diffused as the resins; they have however occupied the attention of chemists more than the latter, so that we are scarcely able to give more than the newly-discovered occurrence of the most important acids in individual plants. Among others, gallic acid was found by M. Aschoff[††] in the leaves of *Rhus Toxicodendron;* by Cockburn[‡‡] in the bark of the roots of *Cornus florida;* by Joh. Tilhgmann[§§] in the roots of *Cimicifuga racemosa;* and by Proctor in the bark of *Prunus virginiana.*

It was formerly believed that the process of the formation of mould increased the quantity of gallic acid, whence it might be inferred that this acid was a product; the experiments of Winckler[‖‖] are opposed to this, although Aschoff states that he found a far greater quantity of gallic acid in the mouldy extract of the leaves of *Toxicodendron* than in the fresh sap.

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[*] Buchn. *Repert.*, vol. lxix.

[†] *Ibid.*, vol. li. part iii.

[‡] *Journ. de Pharm.*, Oct. 1835.

[§] *Ann. de Chim. et de Phys.*, July 1835.

[‖] *Archiv für Pharm.*, vol. iii.

[¶] Buchn. *Repert.*, vol. xlix. p. 303.

[**] The American Journ. of Pharm., July 1835.

[††] *Archiv für Pharm.* vol. i. part 2.

[‡‡] The American Journ. of Pharm., July 1835.

[§§] *Journ. de Chim. Méd.* 1834. November.

[‖‖] Buchn. *Repert.*, vol. li.

The peculiar substance of the Catechu prepared from the *Nauclea Gambir* is, as confirmed by Pfaff[*], identical with the tannic acid of Büchner. It dissolves easily in boiling water, less so in cold; the solution acts as an acid, becomes yellow when exposed to the air, colours solutions of iron green, and does not precipitate gelatine: it consists of $C_{18}\,H_8\,O_8$.

The peculiar acid fumar acid, discovered by Winckler in *Fumaria officinalis* was subjected by Horace Demarcay[†] to an elementary analysis, and was found to be isomeric with the paramalic acid, which, as is known, is a product of the decomposition of malic acid.

M. Geiger found in the bark from the roots of *Cornus florida*, an acid, cornic acid, which contained no nitrogen, was crystalline, easily soluble in water and alcohol, and possessed a bitter taste. The shiller substance (*Shillerstoff*) found in the bark of several dicotyledonous trees was more accurately examined by M. Trommsdorff, sen.[‡]: its properties indicate that it belongs to the class of acids. Trommsdorff, jun., found it to consist of $C_8\,H_9\,O_5$.

M. Trommsdorff, jun.[§], found in the so-called wormseed (the fruit-bearing calathiæ of *Artemisia glomerata*,) pure acetic acid; and M. Radig[||] found 11 per cent. of acetic acid combined with potash in the leaves of *Digitalis purpurea*. M. Bley[¶] affirms that he found in the herbaceous part as well as in the flowers and seed of *Achillea nobilis* acetic acid containing formic acid; we have however reason to doubt the truth of this. We have already mentioned in speaking of the æthereal oils the acid from the flowers of *Spiræa Ulmaria*.

M. H. Trommsdorff[**] lately examined the sylvic acid found in the resins of pines, and which was formerly considered, without any reason, as an oxide of oil of turpentine; according to M. Trommsdorff it is rather the oxide of a radical of $C_{10}\,H_{15}$. It crystallizes in large colourless rhomboidal tables, and melts at 120° Reaum. M. Liebig, who examined the other part of the colophony resin, the pinic acid, found it to have the same composition, by which the isomery of both has again been confirmed.

M. Voget found[††] in the dried leaves of *Asperula odorata*

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[*] Pfaff's *Mittheilungen aus dem Gebiete der Pharm., etc.* vol. i. part 3, 4.

[†] *Ann. de Chim. et de Physique*, August 1835.

[‡] Geiger and Liebig's *Ann. der Pharm.*, vol. xiv. part 2.

[§] *Annalen der Pharmacie*, vol. xi. part 2.

[||] *Pharm. Novellen* von Ehrmann, 1834, part 2.

[¶] Brand, *Archiv f. d. Pharm.*, vol. i. ii. and iii.

[**] Geiger and Liebig's *Ann. der Pharm.*, vol. xiii.

[††] *Archiv für Pharm.*, vol. iii.

benzoic acid; and M. Landerer[*] extracted from the roots of *Inula Helenium* scales similar to sebacic acid, which possessed a pearly lustre, were soluble only in æther and caustic potash, and devoid of smell and taste; the solution acted as an acid. Dumas describes a similar crystalline formation, which could be seen with the naked eye in the form of warty excrescences in the interior of the above-mentioned root[†], which consisted of $C_7 H_9 O$.

Prussic acid was found by Proctor[‡] in the bark of *Prunus virginiana*, and by O. Henry[§] in the sap of the root of *Jatropha Manihot*, or at least, as in most cases, a radical from which it might be formed.

Between these azotic acids and the alkaloids must be placed, on account of their containing nitrogen a division of vegetable formative parts, which for this reason have been termed indifferent, and among which many substances too little known have been classed: the following however have found their right place. Emetine may, according to Landerer[‖], be prepared in small white cubical crystals, the solution of which acts as an alkali and is precipitated by tincture of galls; it must for this reason take its place in the following division, the alkaloids. The microscopico-chemical experiments on the pollen by M. Fritsche[¶] have further shown that the Pollenin of authors deserves no place amongst the proper vegetable formative parts, being unaltered pollen, from the epidermis of which several soluble substances were extracted by means of various solvents, while the contents remained unchanged. He made his experiments with the pollen of *Corylus Avellana*. What was formerly described in this division under the name of *Asparagin* is, according to the experiments of MM. Wittstock, Regimbeau[**], and Schmidt[††], confirmed to be aspartate of ammonia, and is not contained as such in the roots of *Althæa officinalis*, nor in the young sprouts of *Asparagus officinalis* or *acutifolius*, but is a product of decomposition. The latter found this salt in the sap of the leaves of *Atropa belladonna* which had been evaporated to consistence. The bitter substance from *Cetraria islandica* may be justly considered to belong to this division. Rigatelli[‡‡] has described a method of preparing this new substance in cry-

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[*] Buchn. *Repert.*, vol. xlix. p. 275.

[†] *Journ. de Chimie Méd.* June 1835.

[‡] *Ibid.*, 1834.

[§] *Journ. de Pharm.*, 1834. Nov.

[‖] Buchn. *Repert.*, vol. lii.

[¶] Poggendorff, *Annalen*, vol. xxxii. n. 31.

[**] *Journ. de Pharmacie*, 1834. November.

[††] *Annal. der Pharm.*, vol. xii.

[‡‡] *Gazetta eclett. di Farmacia*, 1835. Nov. 11 et 12.

stals, which dissolve in water and in alcohol, but not in æther, and the solutions of which are thrown down as a red precipitate by iron and its salts.

[To be continued.]

XX. *Account of some Experiments made in different Parts of Europe, on Terrestrial Magnetic Intensity, particularly with reference to the Effect of Height. By* James D. Forbes, *Esq., F.R.SS. L. & E., &c., Professor of Natural Philosophy in the University of Edinburgh.*

[Continued from p. 66, and concluded.]

18. V. *Variations in the Needles' Magnetism.*—In all observations of this kind, this change gives rise to the most troublesome errors. The mode of ensuring an equable magnetic state is unknown, though an approximation may generally be obtained to it. Of the two needles sent to this country in 1827, by Professor Hansteen, one (No. 1.) has, after some slight variations, become almost stationary in its magnetism; the other "Flat" has been continually diminishing in intensity. We have seen in the last article that the earth's magnetic action, varying continually and being unknown, we can only properly compare observations made at the same time of the year, and of the day. The progress of change in the needles may be traced by the following tables*.

Table IV.

NEEDLE, No. 1.

Place.	Date.	Observer.	Log. Time 300 Vibrations.	Log. Ratio of Annual Change
Makerstoun,	1829, Jan. 20, 3^{h}	Dunlop.	2·90251	·99997
....	1830, Jan. 23, 3^{h}	....	2·90248	
Edinburgh,	1829, July 9, 11^{h}	....	2·90765	·00043
....	1832, June 2, 11^{h}	Forbes.	2·90890	·99956
....	1833, May 7, 5^{h}	....	2·90849	
.....	1835, May 4, 1^{h}	....	2·90915	·00066
Paris, . .	1833, June 11, 1^{h}	....	2·87102	·99995
......	1835, June 13, 4^{h}	....	2·87092	

The differences since June 1832 are probably imputable to the horary variations alone.

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* The mutual action of the needles is a point of importance. Before they came into my possession they were kept in their separate cases, but without further attention, being packed together in the same external case

[TABLE IV. *continued.*]

FLAT NEEDLE.

Place.	Date.	Observer.	Log. Time 300 Vibrations.	Log. Ratio of Annual Change
Makerstoun,	1828, Dec. 1, 12^h	Dunlop.	3·00582	·00707
....	1830, Feb. 14, 3^h	....	3·01435	
Edinburgh,	1829, July 9, 10^h	....	3·01240	·00546
....	1832, June 2, 12^h	Forbes.	3·02923	·00736
....	1833, May 7, 5^h	....	3·03607	
....	1835, May 4, 2^h	....	3·04579	·00488
Paris,	1833, June 11, 2^h	....	2·99871	·00500
....	1835, June 13, 4^h	....	3·00869	

19. In the case of No. 1, the magnetism has been considered as stationary throughout the period 1832–1835, with which we are now concerned. In the case of the Flat Needle, this cannot be assumed, nor can we admit the change to have been uniform. It seems probable that much movement, and especially alternations of temperature, accelerate the loss of magnetism, that loss having been greatest in 1832, when most of the following observations were made. This is confirmed by a more minute inspection. Observations were made at Geneva on the 20th August 1832, and again on the 10th November, and between these dates the whole of the alpine series is contained: Now the variation of the logarithms for that period is no less than ·00452, or at the rate of ·02001 per annum; whilst we have seen that during the period from June 2, 1832, to May 7, 1833, which includes the above, the mean change was only at the rate of ·00736 per annum. It is clear then that, in order to render the observations of 1832 comparable with one another, we must assume a much higher rate than the mean, for the months from June to November. Admitting some little doubt as to the Geneva comparisons due to the monthly change of intensity, and the great difference of temperature in the two cases; I think that I shall best satisfy the conditions by assuming the log. time to have increased by ·00100 for each month from June to November, leaving ·00684 — ·00500 = ·00184 for the whole of the remain-

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in which they came from Norway. This arrangement I have not changed, but in packing them I have taken pains to place the opposite poles nearest one another, an arrangement which seems to have been attended with good effect; and to show that needles may lie within an inch or two of one another without material injury, when we see the stationary condition of No. 1, and the diminishing rate of variation of the "Flat" Needle.

ing six months down to May 1833, during which the needle was in a state of almost perfect repose.

20. The mode of allowing for this is the following. All determinations of intensity are *relative*, referring to some intensity as a standard; but I have taken the horizontal intensity at Paris as unity (which is to that at the magnetic equator as 4,788 to 10,000 according to Humboldt)*. Hence, since the squares of the times of 100 vibrations are inversely as the magnetic forces, the terrestrial horizontal intensity at a station A is to that at Paris, or 1, as the square of the time observed at Paris (which time we may call T_P) is to the square of the time observed at station A (or T_A). Hence,

$$\text{Intensity at A} = \left(\frac{T_P}{T_A}\right)^2$$

If the magnetism of the needle change, we must therefore find by interpolation the time of vibration at Paris for the particular epoch of observation.

21. In the case of Needle No. 1, the magnetism being stationary, the time of 100 vibrations has been assumed from an observation made (in M. Arago's *Cabinet Magnetique*), 11th June 1833, as equal to

$247^{\text{sec}}70$; its log. 2·39392

22. In the case of the "Flat" needle, a subsidiary table has been calculated of the times for Paris, corresponding to the epochs when observations were made elsewhere, which appear amongst the details to be given in the sequel. On the 11th June 1833, the log. time of 100 vibrations at Paris was .... 2·52159

If, for the period from June 1832 to May 1833, we deduct ·006184 (by Art. 18) and for the month of May 1833, ·00040 being the rate of change for the current period (Art. 18), we have a change for one year, subtractive, ... ·00724

T_P, or Time at Paris,	June 11, 1832, Log.	2·51435
Adding ·001 per month, as proposed in Art. 19, we shall have nearly these values (neglecting trifling quantities) as approximations to the value of Log. T_P	July 11, ...	2·51535
	Aug. 11, ...	2·51635
	Sept. 11, ...	2·51735
	Oct. 11, ...	2·51835
	Nov. 11, ...	2·51935

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* Deduced from the measure of *total* intensity 1·3482 at Paris, given in the *Mémoires d'Arceuil*, tom. i. multiplied by the cosine of the dip, there also given (69° 12′).

Proceeding similarly with the mean (and very regular) ratios of change in Art. 18, we shall find

Log. T_P {
1833, May 7, ... 2·52114
1835, May 4, ... 2·53100
... July 20, ... 2·53203
... July 30, ... 2·53216
... Aug. 10, ... 2·53230

23. The various corrections now considered being fixed, the application of them, and the deduction of the horizontal intensities related to Paris as unity, becomes easy. I have employed printed forms for this purpose, arranged in pages each containing 5 reductions after the following model, and bound up in books.

MAGNETIC INTENSITY.—HORIZONTAL NEEDLE, No. 1.

Place, ...	GENEVA. Botanic Garden.		GENEVA. Botanic Garden.	
Date, ...	1832, Nov. 10.		1832, Nov. 10.	
Mean Time, ...	$11^h\ 33^m$		$11^h\ 42^m$	
No. of Vibrations observed.	100		100	
	Arg.	Log.	Arg.	Log.
Observed Time.	$\overset{s}{240·14}$	2·38046	$\overset{s}{240·00}$	2·38021
Rate Chronometer.	$+21^s$	9·99990	$+21^s$	9·99990
Arc, ...*	$\alpha = 10°$ $m = 110°$	9·99967	$\alpha = 10°$ $m = 110°$	9·99967
Temperature, ..	7°·0 R.	9·99863	7°·0 R.	9·99863
Corrected Time (T).	$\overset{s}{239·14}$	2·37866	$\overset{s}{239·06}$	2·37841
Time at Paris (T_P)	....	2·39392	....	2·39392
$\frac{T_P}{T}$	....	0·01526 2	....	0·01551 2
$\left(\frac{T}{T_P}\right)^2$ = Intensity.	1·073	0·03052	1·074	0·03102

* α = Initial semi-arc of vibration; m = number of vibrations which reduce the semi-arc to $\frac{\alpha}{2}$.

§ 3. *Observations on Magnetic Intensity.*

24. It now remains to give the observations which have been made, and reduced in the way already detailed. These consist chiefly of two series. One was made in the year 1832, intended to form part of a very general investigation in physical geography, which I meant to pursue throughout a journey of several years. Having been diverted from this by the opening of other prospects, the series remains incomplete as a general investigation, but embraces a connected examination of a great district of the higher and central Alps, calculated to elucidate a question which I had particularly proposed to myself, as to the supposed diminution of magnetism with height. It likewise includes some observations as to the influence of extinct volcanos on the Rhine. The second series was made in the Pyrenees in 1835, with almost an exclusive view to the influence of height, and is confined to one small district. One other important point gained was a very accurate determination of the comparative horizontal intensities at Edinburgh and Paris. The choice of the stations was regulated very much by the views just mentioned: the particular spots of observation, together with the geographical position and elevation of the place, will be given in Table VII. I have thought it better to record in the first place in separate Tables for the two needles, the details of the observations in the order in which they were made, the data for correction, and the corrected numbers. These are contained in Tables V. and VI. I have thought it needless to incur the labour and expense of printing the individual numbers on which the mean results are founded. They are however preserved in a condition adapted for immediate reference*.

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* Many of the numerical calculations contained in the remainder of this paper, have been made by two of my pupils Messrs. Irvine and Edward, under my own inspection and revision.

Table V.

NEEDLE, No. I.

Place.	Date.	Mean Time.	No. of Vibrations observed.	Observed Time.	Rate Chronometer.	Arc.* α.	Arc.* m.	Temp. Reaum.	Corrected Time 100 Vibrations.	Intensity. Paris = 1.
	1832.	h m		s	s			s	s	
Edinburgh	June 2.	10 22	300	817·66	+13	20	80	16·4	270·26	·840
Brussels[1]	July 9.	10 29	300	766·67	+17	20	70	20·5	252·97	·959
Brussels	July 9.	10 53	300	766·87	+17	20	90	21·6	252·83	·960
Spa	July 17.	5 53	100	252·27	+23	10	90	17·35	250·09	·981
Spa	July 17.	6 7	100	252·80	+23	10	90	17·0	250·69	·977
Königstuhl, near Heidelberg	July 28.	11 4	100	247·60	+23	10	90	17·15	245·48	1·018
Königstuhl	July 28.	11 17	100	247·54	+23	10	90	16·75	245·46	1·018
Heidelberg	July 28.	2 24	100	247·46	+23	10	100	15·4	245·51	1·017
Brühl	Aug. 1.	10 55	100	251·54	+23	10	100	17·6	249·28	·987
Brühl	Aug. 1.	11 7	100	251·76	+23	10	90	17·7	249·51	·985
Brühl	Aug. 1.	11 17	100	251·76	+23	10	90	17·55	249·51	·985
Laach	Aug. 4.	1 17	100	252·71	+23	10	80	21·7	249·97	·982
Laach	Aug. 4.	1 33	100	252·21	+23	10	80	21·0	249·62	·985
Mont Salève, near Geneva	Aug. 17.	1 43	100	240·61	+20	10	110	17·0	238·51	1·078
Mont Salève	Aug. 17.	1 53	100	240·79	- 20	10	110?	17·1	238·71	1·077
Geneva	Aug. 20.	11 21	100	241·17	+27	10	100	21·8	238·60	1·078
Geneva	Aug. 20.	11 39	100	241·17	+27	10	90	21·9	238·60	1·078
Mont Breven	Aug. 22.	2 12	100	239·27	+27	10	90	15·7	237·36	1·089
Chamouni	Aug. 23.	12 39	100	240·06	+27	10	110	18·4	237·82	1·085
Jardin	Aug. 25.	12 29	100	239·40	+29	10	120	11·0	237·95	1·084
Chamouni	Aug. 26.	1 25	100	239·70	+27	10	110?	15·0	237·83	1·085
Col des Fours	Aug. 28.	9 28	100	238·19	+27	10	110?	4·8	237·43	1·088
Aoste	Aug. 29.	4 8	100	238·71	+27	10	130	16·7	236·64	1·096

* α. indicates the initial semi-arc of vibrations; m. the number of vibrations required to reduce it to half its amount.

[1] Rather windy.

TABLE V. (*continued.*)

NEEDLE, No. I.—(*continued.*)

Place.	Date.	Mean Time.	No. of Vibrations observed.	Observed Time.	Rate Chronometer.	Arc.		Temp. Reaum.	Corrected Time 100 Vibrations.	Intensity. Paris = 1.
						α.	*m.*			
	1832.	h m		s	s			s	s	
St. Bernard	Aug. 30.	5 22	100	239·07	+27	10	120	6·8	238·08	1·082
Martigny[1]	Sept. 1.	8 39	100	239·87	+27	10	90	14·8	238·05	1·083
Interlaken	Sept. 10.	4 49	100	241·41	+14	90	90	14·3	239·66	1·068
Schmadribach, Valley of Lauterbrunnen	Sept. 12.	12 34	100	240·11	+14	10	100	12·3	238·63	1·077
Schmadribach[2]	Sept. 12.	12 44	100	239·71	+14	10	110	11·5	238·25	1·081
Grindelwald	Sept. 14.	9 59	100	240·80	+14	10	90	14·35	239·08	1·074
Grindelwald	Sept. 14.	10 8	100	240·51	+14	10	100	14·35	238·75	1·076
Meyringen	Sept. 16.	5 9	100	240·70	+14	10	120	12·45	239·12	1·073
Meyringen	Sept. 16.	5 17	100	240·31	+14	10	110	11·95	238·80	1·076
Grimsel	Sept. 17.	5 57	100	240·00	+14	10	110	7·45	238·96	1·074
Münster[3] (Vallais)	Sept. 18.	5 26	100	239·96	+14	10	110	12·5	238·40	1·080
Münster	Sept. 18.	5 43	100	240·04	+14	10	100	10·4	238·72	1·077
Gemmi, summit	Sept. 21.	8 46	100	239·71	+14	10	120	9·9	238·41	1·079
Gemmi	Sept. 21.	8 54	100	239·83	+14	10	120	9·6	238·60	1·078
Frütigen (Kanderthal)	Sept. 21.	5 2	100	240·56	+14	10	110	12·45	239·00	1·074
Frütigen	Sept. 21.	5 11	100	240·56	+14	10	110	11·6	239·09	1·073
Grindelwald	Sept. 23.	8 50	100	240·14	+14	10	110	6·5	239·21	1·072
Faulhorn	Sept. 24.	8 13	100	240·57	+14	10	110	8·0	249·48	1·070
Faulhorn	Sept. 24.	8 22	100	240·39	+14	10	110	7·25	239·37	1·071
Engelberg	Sept. 27.	5 22	100	241·13	+14	10	110	11·3	239·70	1·068
Engelberg	Sept. 27.	5 30	100	240·54	+14	10	110	10·6	239·20	1·073
Surennes	Sept. 28.	10 49	100	240·81	+14	10	120	12·8	239·20	1·072
Surennes	Sept. 28.	10 59	100	240·99	+14	10	110	12·8	239·40	1·071
Klus, near Altorf	Sept. 28.	5 24?	100	240·81	+14	10	110	11·8	239·31	1·071

Klus	Sept. 28.	5 34	100	240·63	+14	10	110	10·7	239·25	1·072
St. Gothard	Sept. 30.	8 35	100	240·47	+14	10	100	7·1	239·50	1·070
St. Gothard	Sept. 30.	8 50	100	240·09	+14	10	110	6·3	239·16	1·072
St. Gothard	Sept. 30.	8 58	100	239·64	+14	10	110	6·1	238·76	1·076
Locarno	Oct. 2.	2 34	100	240·20	+14	10	110	18·95	238·50	1·084
Orta	Oct. 4.	12 38	100	239·11	+14	10	110	18·45	236·91	1·093
Orta	Oct. 4.	12 46	100	239·26	+14	10	110	18·85	237·0	1·092
Bellaggio	Oct. 8.	8 5	100	238·16	+14	10	100	12·85	236·60	1·096
Bellaggio	Oct. 8.	8 16	100	238·41	+14	10	90	12·8	336·82	1·094
Reichenau[4]	Oct. 10.	3 32	100	240·43	+14	10	110	11·85	238·93	1·075
Reichenau	Oct. 10.	3 41	100	240·07	+14	10	100	11·25	238·65	1·077
Wallenstadt	Oct. 12.	9 39	100	241·01	+14	10	110	12·05	239·41	1·070
Wallenstadt	Oct. 12.	9 49	100	241·17	+14	10	110	12·4	239·62	1·068
Lucerne	Oct. 15.	10 11	100	240·83	+14	10	120	8·6	239·66	1·068
Rigi Culm	Oct. 16.	7 55	100	240·71	+14	10	110	3·15	240·36	1·062
Geneva	Oct. 29.	12 9	100	240·31	+21	10	100	10·95	238·97	1·075
Geneva	Nov. 10.	11 33	100	240·14	+21	10	110	7·0	239·14	1·073
Geneva	Nov. 10.	11 42	100	240·00	+21	10	110	7·0	239·06	1·074
	1833.									
Edinburgh	May 7.	4 33	300	217·86	+ 3	20	90	18·13	270·10	·841
Edinburgh	May 7.	4 50	100	272·29	+ 3	10	90	17·9	269·91	·842
Paris[5]	June 11.	12 38	300	750·94	...	20	110	20·27	247·70	1·000
Paris[6]	June 11.	12 55	100	250·13	...	10	120	20·2	247·67	1·000
	1835.									
Edinburgh	May 4.	1 18	100	271·8	...	10	90	9·8	270·42	·839
Edinburgh	May 4.	1 33	100	271·16	...	10	90	9·2	270·40	·839
Paris	June 13.	3 41	100	249·96	−40·5	10	100	20·4	247·62	1·001
Paris	June 13.	3 51	100	250·0	−40·5	10	100	20·15	247·63	1·000
Pic de Bergons, near Luz, Hautes Pyrénées[7]	July 18.	10 16	100	236·26	+20	10	100 ?	14·75	234·50	1·116

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[1] Local disturbance suspected. [2] Superior to the last. [3] Suspension not quite free. [4] Indifferent observation. [5] Chronometer very near needle. [6] Chronometer three feet from needle. [7] These observations being not quite unexceptionable, owing to a small compass needle being accidentally retained in the pocket, were repeated three days after.

TABLE V.—(*continued.*)

NEEDLE, No. 1.—(*continued.*)

Place.	Date.	Mean Time.	No. of Variations observed.	Observed Time.	Rate Chronometer.	Arc.		Temp. Reaum.	Corrected Time 100 Vibrations.	Intensity, Paris = 1.
						a.	*m.*			
	1835.	h m		s	s			s	s	
Pic de Bergons[1]	July 18.	10 38	100	236·33	+20	10	110	14·2	234·60	1·115
Pic de Bergons[1]	July 18.	10 51	100	236·56	+20	10	100	14·1	234·82	1·113
Luz[2]	July 20.	11 9	100	235·84	+20	10	100	17·65	233·74	1·123
Luz[3]	July 20.	11 25	100	235·67	+20	10	90	18·25	233·54	1·125
Luz[4]	July 20.	11 34	100	236·03	+20	10	80	18·7	233·86	1·122
Luz[4]	July 20.	11 45	100	235·14	+20	10	80	19·05	232·94	1·131
Pic de Bergons	July 21.	10 38	100	236·29	+20	10	120?	15·25	234·34	1·117
Pic de Bergons	July 21.	10 47	100	236·57	+20	10	100?	15·0	234·74	1·113
Pic de Bergons	July 21.	10 59	100	236·17	+20	10	110	15·1	234·34	1·117
Luz	July 28.	11 10	100	235·69	+20	10	80	18·85	233·50	1·125
Luz	July 28.	11 23	100	235·80	+20	10	100	18·9	233·60	1·124
Gavarnie	July 29.	11 52	100	235·62	+20	10	100	19·2	233·38	1·127
Gavarnie	July 29.	12 4	100	235·60	+20	10	100	19·0	233·38	1·126
Ste Marie, valley of Campan	Aug. 7.	10 4	100	236·19	+20	10	110	19·45	233·91	1·121
Ste Marie	Aug. 7.	10 16	100	236·30	+20	10	100	19·3	234·04	1·120
Pic du Midi	Aug. 8.	10 4	100	235·01	+20	10	100	9·45	233·88	1·122
Pic du Midi	Aug. 8.	10 18	100	235·13	+20	10	110	9·35	233·90	1·121
Pic du Midi	Aug. 8.	10 38?	100	235·19	+20	10	110	9·65	233·90	1·121
Brèche de Roland[5]	Aug. 11.	10 40	100	235·33	+20	10	110	12·35	233·80	1·122
Brèche de Roland[6]	Aug. 11.	10 53	100	235·34	+20	10	120	12·6	233·76	1·123

[1] See note (7) preceding page.

[2] Windy, but observation good.

[3] Windy.

[4] The best observations at Luz.

[5] A knife in the pocket.

[6] Unexceptionable.

[To be continued.]

XXI. *On Murio-carbonate and Native Muriate of Lead.*
By H. J. Brooke, *Esq., F.R.S., &c.*[*]

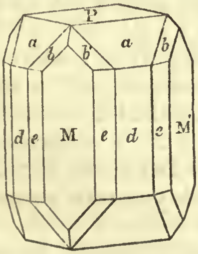
A SPECIMEN I obtained some time since from Cornwall contains both these substances. I have not observed any specimen of either of them in any of the collections that I have seen in Cornwall, nor am I aware of the existence of any others from that country, except a few single crystals of the *murio-carbonate* in the collection of the late Wm. Phillips, which came into his possession after the publication of his "Mineralogy."

The figure and measurements of this mineral given in page 343 of his work (edition of 1823)[†] do not afford data for ascertaining the dimensions of the prism.

I annex a figure of one of the crystals from Cornwall, from which those elements may be deduced.

The primary form is well known to be a square prism, and from the inclination of P on *a*, the ratio of a terminal edge to a lateral edge is found to be as 35 to 38 very nearly. There are bright cleavages parallel to all the primary planes and to both the diagonal planes of the prism.

The symbols of the planes are	$\overset{1}{A}$	A_2	$\overset{1}{G}$	$\overset{2}{G}$
The planes being	*a*	*b*	*d*	*e*



P, M = 90°
P, *a* = 123 6′
P, *b* = 112 22
M, *a* = 126 20
M, *b* = 145 47
M, *d* = 135
M, *e* = 153 26

Count Bournon gives 121° 52′ as the inclination of P on

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[*] Communicated by the Author.

[†] I cannot without injustice to the memory of my late friend William Phillips refer to the recent edition of his Mineralogy, alterations of such a nature having been made in it as to render it no longer his work.

The editor admits in his preface, with equal candour and truth, his incapacity to do justice to the work, as indeed is apparent throughout the book, for he has not only omitted to correct any of the errors, as far as I have observed, but he has interpolated many additional mistakes, and in a manner which might lead any reader to suppose them the blunders of the author. Several however of these the editor might have avoided by using the ordinary diligence of a compiler.

The second angular measurement given in the work, P on *g* 1 of quartz, is more than 30° wrong, by a mistake of W. Phillips in copying from Haüy, some plane, probably *a*. The planes of the crystal I have measured are brilliant and perfect, several of the measured angles having agreed exactly with the calculated ones.

The *muriate* or *chloride of lead* occurs on my specimen in the form of very thin and irregularly curved translucent crystals, without any well defined lateral or terminal planes. The colour is yellowish white.

The transparent and colourless crystals described as *antimonial phosphate of lead* from Horhausen on the Rhine, have the form and angular measurements of *phosphato-arseniate of lead*; but on examination they appear to consist wholly of *chloride of lead*. H. J. B.

XXII. *On certain Points of Chemical Philosophy and Nomenclature. By* ROBERT HARE, *M.D., Professor of Chemistry in the University of Pennsylvania; with a Letter from* M. Berzelius.

[*To the Editors of the American Journal of Pharmacy.*]

DEAR SIRS, Philadelphia, March 4, 1837.

IN September, 1833, I published in your Journal, together with some encomiums upon the Treatise of Chemistry by the celebrated Berzelius, certain objections to his nomenclature, and some suggestions respecting a substitute, which I deemed to be preferable. In the following June I addressed a letter to Professor Silliman upon the same topics, in which my criticisms and suggestions were amplified and corrected in obedience to more mature reflection. A printed copy of that letter having been sent by me to Berzelius, I received in answer an epistle, of which I furnish you with a translation.

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and there are in other places, in Rutile, for example, impossible values of angles given, through the inattention of the author in copying from his rough memorandums, but which a very slender knowledge of the subject would have enabled the editor to set right. His omissions, however, in this respect would be less injurious to the reputation of the author than the altered and interpolated passages. In sulphuret of copper, for instance, the author gives a *regular six-sided prism*, either simple or modified, as the crystalline form, but the editor has inserted that the *primary* form is a *cube*. Now an editor so little acquainted with mineralogy as not to know that a cube cannot be the primary form of a regular six-sided prism, ought not to have attempted any alteration in W. Phillips's text, but should have reprinted it exactly as it stood, and have placed any additions of his own in an appendix.

* We reprint this article at the request of our friend Dr. Hare.—EDIT.

Since the period of that correspondence, so demonstrative of candour and good feeling on the part of the great Swedish chemist, I have published two editions of my Compendium of Chemistry, in which I have pursued a course corresponding with my criticisms above alluded to. I am therefore desirous, in addition to the letter of Berzelius to lay before the public a recapitulation, review, and an additional explanation of the grounds upon which I have ventured to employ a language, and an arrangement inconsistent with the practice and opinions of a chemist by whose authority in other respects I am usually influenced. But before proceeding with the ungracious task of endeavouring to establish the correctness of my views in opposition to those of my friend, I feel that it will be no more than justice to repeat an acknowledgement, already made in my text book, that if De Bonsdorff, myself, and others are right in considering the *double* salts of Berzelius as *simple* salts, it is to the light afforded by his investigations, that we owe the power of seeing the subject correctly. I believe the idea, that any other body besides oxygen could produce both acids and bases capable of forming salts, originated with Berzelius, in the instance of sulphur.

Recapitulation and Review of the Grounds of his deviating from the Language and Arrangement of Berzelius, and other distinguished Chemists; with some additional Explanations and Suggestions, by R. Hare, *M.D., Professor of Chemistry in the University of Pennsylvania.*

According to the Berzelian nomenclature, bodies which produce salts by a union with radicals are called *halogen* or *salt producing bodies*, while those which with radicals form both acids and bases, capable by their union of constituting salts, are called *amphigen bodies* or *both producers.* Salts, produced by the first mentioned class are called haloid salts; those produced by the other are called amphide salts.

I objected to this classification, that the words *salt*, *acid* and *base*, were broad, vague and unsettled in their acceptation, having, by chemists in general, and especially by Berzelius, been employed to designate substances differing in composition, and extremely discordant in their properties; that no method of defining a salt had been devised, which had not been founded either on properties or composition; that in the nomenclature of Berzelius properties were disregarded, since among his haloid and amphide salts were found substances differing extremely in this respect. Thus, for instance, common salt, Glauber's salt, Epsom salt, vitriolated tartar, and cream of tartar, were associated with the fuming liquor of

Libavius, the butyraceous chlorides of zinc, antimony, and bismuth, plumbum corneum, luna cornea, fluor spar, and the acid fluorides of silicon and boron. I objected also that composition could not be resorted to consistently with his classification; since, agreeably to it, a salt might be either a binary compound of a halogen body with a radical, or consist of two binary compounds, each containing the same amphigen body.

To the terms *acid* and *base*, as employed in his nomenclature, I objected, that neither by the celebrated author, nor by any other chemist, had any definition been adhered to which could, consistently with his plan, restrict the meaning of those appellations to the binary compounds formed by the union of his amphigen bodies with radicals.

Acidity and basidity* had sometimes been distinguished by an appeal to properties, sometimes to composition, but to neither had there been any consistent attention. In order to demonstrate the total neglect of properties latterly displayed, it was only necessary to contrast substances bearing generally the name of acids; as for instance sulphuric acid with rock crystal, acetic acid with tannin, and prussic acid with margaric; or to contemplate simultaneously the admission of the hydracids formed with the halogen bodies into the class of acids, while alleged incapable of combining with bases, with the exclusion from that class of nitrous acid, upon the plea of the same incapacity.

In reference to neglect of composition in forming the class of acids, it will be sufficient to advert to the association in that class, of compounds formed with radicals both by the halogen and amphigen bodies; so that the halogen bodies are in one case producers of salts, in the other producers of acids; in one case act as supporters, acidifiers, or electro-negative principles, in another as radicals to the comparatively electro-positive hydrogen, pre-eminently a radical by the definition of that word given in the treatise of the distinguished author of the nomenclature.

After stating my objections to the basis of the Berzelian nomenclature, I proceeded to mention those to which I considered the superstructure as liable.

Having designated the acid compounds of his amphigen class, by prefixing syllables indicating their electro-negative ingredients; having also in some instances, as in those of the fluosilicic, and fluoboric acids, adopted this course in relation

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* For the use of the words basidity and salidity, I have no authority; but conceive that through their analogy with acidity their meaning is so obvious as to make it expedient to employ them.

to halogen bodies; I objected to the use of the word hydracid, in which the electro-positive radical is made to act as if coordinate with oxygen.

Moreover, the termination in *ide* having been generally attached to the electro-positive compounds of oxygen, acting as bases, I condemned the employment of that termination, to distinguish the electro-negative, and acid compounds of sulphur, selenium, and tellurium. I considered it inconsistent to give precedence to the syllable designating the radical in the acids formed with hydrogen; as in hydrochloric, hydrobromic, hydriodic, hydrofluoric, hydrofluoboric, hydrofluosilicic, preferring the terms chlorohydric, bromohydric, iodohydric, fluohydroboric, fluohydrosilicic, &c., in which I have been sanctioned by Thenard and others.

I proposed a definition of an acid, and a base, which I conceived to be the only one which could be adopted, consistently with the use made of those words by Berzelius, and other distinguished chemists; and advanced that, agreeably to that definition, his double haloid salts must be considered as *simple* salts, severally formed of an acid and a base.

I objected to his treating the words combustion, and oxygenation as synonymous.

Having thus made the reader acquainted with the substance of my criticisms upon the Berzelian nomenclature, I will subjoin his letter in answer to them, and will then state, and endeavour to justify, the conclusions at which I have arrived.

Letter from J. J. Berzelius *of Stockholm, to* R. Hare, *M.D., Professor of Chemistry in the University of Pennsylvania, acknowledging the receipt of a Communication respecting Nomenclature, and replying thereto.*

Sir, Stockholm, Sept. 23, 1834.

I am very much obliged to you for the remarks, which, under the date of June 21st, you had the friendship to communicate to me, respecting the nomenclature which I have employed in my Treatise of Chemistry.

I perceive that having contemplated chemical phænomena under different points of view, we differ as to the nomenclature which is the most appropriate for their description. I consider the combinations of metals with chlorine, bromine, &c., as salts; whilst you, in accordance with Mr. De Bondsdorff, consider them as bases and acids, capable of forming salts by their union.

If it were expedient that chemical classification should be dependent on the number of simple bodies which enter into each combination, this idea of Mr. De Bondsdorff would without doubt be preferable; but if attention be due to the chemical properties which characterize combinations, we cannot adhere to an arrangement founded on the number of the elements. Yet so essential is it in chemistry to have reference to properties, that a system of chemistry in which common and analogous properties should not affect the arrangement, would present a mass of facts so chaotic, that no memory would be competent to retain them. In a system thus strictly conformable to the ideas of Mr. De Bondsdorff, cyanogen, though in its properties resembling chlorine or bromine which are simple bodies, ought to be considered, also, as a base or as an acid having azote for its radical—I am persuaded you would not approve of extending the system of De Bondsdorff so far; but if it be correct, it would be inconsistent not to make this extension.

But let us return to the combinations of the metals with chlorine, fluorine, &c., and make, in imagination, the following experiment. Let us take two portions of caustic potash, a base in which the *basic* characters are more striking than in any other. To one, let us add a sufficiency of sulphuric acid to extinguish entirely its *basic* property; we shall then have a neutral body of a saline taste. You will admit it to be a salt. Now let us add to the other portion, hydrofluoric acid. At a certain point the *basic* properties of the potash will disappear, and we shall have a resulting compound quite as neutral as the sulphate of potash, endowed with a saline taste entirely analogous to that of the sulphate. The basic properties of the potash are destroyed by the hydrofluoric acid, as well as by the sulphuric acid. But you will allege the resulting combination is not a salt, but a base which has exchanged one basifier (oxygen) for another basifier (fluorine). In proof you may add as much more hydrofluoric acid, which combining with the new base will form with it a crystallized salt. But this salt is not neutral, it has almost the same acidity of taste as the hydrofluoric acid employed. The new base does not destroy then the acid reaction.

Let us make a further addition of sulphuric acid to the sulphate of potash. A salt equally acid will result, in which the sulphate of potash acts the same basic part towards the sulphuric acid, as the fluoride of potassium towards the hydrofluoric acid. Should it be desired to extend the comparison further, it will be found that for each less electro-positive fluoride, susceptible of combination with the potassic fluoride, there will be, with but very few exceptions, a corresponding sulphate, susceptible of combination with the sulphate of potash. The analogy is then complete, it exists not only in the perfect neutrality of the two potassic salts, in their saline taste, but also in their manner of forming combinations with other bodies; notwithstanding one of them, the sulphate, contains one element more than the other. If, instead of potash, potassium were employed to saturate our two acids, the analogy of the operation in both cases, would be still more complete. The same quantity of metal would displace equal volumes of hydrogen. When the visible results of our experiments are so perfectly analogous, it is to be presumed that the invisible process which we do not see, may also be perfectly analogous, and that if facts exactly alike are explained differently, there must be a defect in the explanation. If, for instance, the true electro-chemical composition of the sulphate of potash should not be $KO + SO^3$, as is generally supposed, but $K + SO^4$ *, and it appears very natural that atoms, so eminently electro-negative as sulphur and oxygen, should be associated, we have, in the salt in question, potassium combined with a compound body, which, like cyanogen in $K + C^2 N$ †, imitates simple halogen-bodies, and gives a salt with potassium and other metals. The hydrated oxacids, agreeably to this view, would be then hydracids of a compound halogen body, from which metals may displace hydrogen, as in the hydracids of simple halogen bodies. Thus we know that SO^3, that is to say, anhydrous sulphuric acid, is a body whose properties, as respects acidity, differ from those which we should expect in the active principle of hydrous sulphuric acid.

The difference between the oxisalts and the halosalts is very easily illustrated by formulæ. In K|FF—fluoride of potassium, there is but one single line of substitution, that is to say, that of K|FF, whilst in KOOOOS (sulphate of potash) there are two, K|OOOOS and KO|OOOS of which we use the first in replacing one metal by another, for instance, copper by iron; and the second in replacing one oxide by another.

I do not know what value you may attach to this develop-

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* In the Berzelian symbols, K stands for kalium, or potassium, S for sulphur, O for oxygen, and O^3 for three atoms of oxygen, O^4 for four atoms of oxygen.

† That is to say, if the salt called sulphate of potash, be considered as a compound of potassium, and a quadroxide of sulphur, instead of being viewed as a protoxide of potassium, or potash, and tritoxide of sulphur, or sulphuric acid.

This is the formula for cyanide of potassium, consisting of potassium, K, and cyanogen, or two atoms carbon and one of nitrogen, $C^2 N$.

ment of the constitution of the oxysalts (which applies equally to the sulphosalts and others): but as to myself, I have a thorough conviction, that there is therein, something more than a vague speculation; since it unfolds to us an internal analogy in phænomena, which, agreeably to the perception of our senses, are externally analogous. If these phænomena are to be considered agreeably to the ideas of Mr. De Bondsdorff, how does it happen that sulphur, phosphorus, arsenic, and other radicals of the strongest oxacids, when united with chlorine, bromine, iodine, &c., do not combine with the chlorides*, bromides, &c., of the metals of the alkalies and of the earths; whilst the chloride and bromide of potassium combine easily with those of magnesium, iron, and manganese? Should then the chloride of magnesium, or that of manganese, be a stronger acid than the chloride of sulphur, or chloride of phosphorus? How is it consistent with these ideas that we can obtain crystallized salts as well with as without water, of combination, composed of chloride of calcium and of oxalate, or of acetate of lime? Should the oxysalt be here the acid, or the base? I have now displayed to you, the considerations which have guided me, and which I think are not destitute of foundation.

I cheerfully admit that it would be preferable to employ the word chlorohydric, instead of hydrochloric. My motive for retaining this last, is, that I have ventured to propose a new nomenclature in a language foreign to me, in which it was inexpedient to make changes, which could be avoided without inconvenience. I also agree with you, that we ought not to use combustible and oxidable, as having the same meaning. I have deserved your strictures for this inconsistency in my language; but I must suggest as an apology, that the two words were formerly used as synonymous, and that the work, in which you have recently noticed this oversight, was first published in 1806, having been from time to time remoulded for new editions, without its having been possible to eradicate all that has not kept pace with the progress of science.

Accept the assurance of my perfect esteem, and of the sentiments of sincere friendship with which I have the honour to be, Yours, &c.

An Examination, by the Author of this Article, of the Suggestions in the preceding Letter of Berzelius, *and how far the Objections made to his Nomenclature are therein answered.*

So far as my strictures were founded on the alleged diffi-

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* I have translated chlorure, fluorure, bromure, by chloride, fluoride, and bromide, agreeably to the practice of the British chemists.

culty of defining the terms acid, salt, and base, in any mode consistent with his classification, they are not met by any facts or reasoning in the much-esteemed letter of my illustrious correspondent. The impracticability of defining a salt, he does not deny; and with great candour he admits that, in his definition of acidity, he has not been consistent. He concedes that it would be preferable to give the syllable, indicating the electro-negative ingredient, the precedence, as nothing but unwillingness to innovate prevented him from pursuing that course.

He acknowledges that as combustion, in many instances, takes place without the presence of oxygen, the application of the word combustible, should not be confined to bodies which are susceptible of oxydizement.

My definition of acidity was as follows :—

" *When, of two substances capable of combining with each other so as to form a tertium quid*, and having an ingredient common to them both, one prefers the positive, the other the negative pole of the voltaic series, we must deem the former an acid, and the latter a base. Also all substances having a sour taste, or which redden litmus, must be deemed acids, agreeably to usage.*" This definition I would now amend by leaving out the last sentence, and substituting therefore, the following: *Also when any substance is capable of forming a tertium quid with any acid or base agreeably to the preceding definition, it must be considered as an acid in the one case, a base in the other.* The definition, thus amended, takes in the organic acids and bases. In the form in which it was at first proposed, it has not been alleged defective by Berzelius; but he has striven to show an incongruity in the attributes of his double salts, when contrasted with those resulting from the union of some of the acids and bases of his amphigen class; which incongruity is, in his opinion, a sufficient reason for not considering them as *simple* salts, and their ingredients as acids and bases, agreeably to the opinions of De Bondsdorff and myself.

Berzelius errs in confounding my opinions with those of De Bondsdorff. However I may have admired the sagacity with which that chemist investigated the pretensions of some haloid salts to certain attributes of acidity or alkalinity; in my letter on the Berzelian nomenclature, I signified my unwillingness to rest my opinions upon a basis so narrow, as that which

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* This term *tertium quid* has been used by chemists, more formerly than of late, to designate a compound resulting from the union of two bodies, but in its properties resembling neither.

he had endeavoured to establish. I stated that I did not deem it necessary to appeal to his excellent observations, proving certain attributes of acidity to exist in one case, those of alkalinity in the other. I alleged my definition to be founded on the conviction that the property of affecting vegetable colours, on which that sagacious chemist lays so much stress, has not, latterly, been deemed necessary in acids; and that in bases it never was required. As respects them, it only served as a mean of subdivision between alkaline oxides and other oxibases.

I am at a loss to discover in what part of my letter there was any language which could convey the erroneous impression, that, in defining acids and bases I proposed to overlook properties, and to be regulated by attention to the number of atoms in a compound. Certainly nothing was more foreign to my thoughts.

It is assumed by Berzelius that the saturation of the fluobase of potassium by fluohydric acid, cannot be considered as analogous to the saturation of the oxybase of potassium by sulphuric acid; because the resulting compound is to the taste in one case neutral, in the other sour. In reply I suggested that if the salidity of the biborates and bicarbonates was not to be questioned on account of their alkaline taste, nor that of the protochloride of tin on account of its sourness, it was not consistent that the pretensions to salidity of the fluohydrate of the fluobase of potassium should be denied on account of its sour taste. I will now add that if the fluosilicate of potassium be a double salt, the fluoride of silicon one of its two constituents must be a simple salt, and yet it is sour. If a simple salt may be sour, why may not a double salt have this attribute; and how in fact can its presence be inconsistent with salidity? Is not the absence of this characteristic in silica and tannin, and many other acids, as much against their claims to acidity, as its presence in other compounds is an objection to their association with saline bodies? It is considered by Berzelius an objection to the views which I have espoused, that the halogen bodies, while forming acids with various metallic radicals which oxygen does not acidify, do not form acids with sulphur, phosphorus, and arsenic which oxygen does acidify; yet what is there in this, more difficult to reconcile with the established results of chemical combinations, than in the fact that oxygen forms with sulphur, phosphorus, and arsenic, strong acids, with hydrogen water; while with hydrogen the halogen bodies all form compounds which Berzelius describes as having the highest pretensions to acidity? The highly active acid properties of the fluorides of boron and silicon, would lead us to expect similar compounds to be formed by the same radicals, with the other halogen bodies, contrary to experience. Chemistry makes us acquainted with many similar discordances. How is it that oxygen forms aëriform compounds with an extremely fixed body in the instance of carbon; while in that of phosphorus or arsenic, both volatilizable, it forms acids which are comparatively insusceptible of volatilization? Wherefore does not hydrogen produce an acid with phosphorus and arsenic, as well as with sulphur?

According to Berzelius, all the halogen bodies produce with hydrogen combinations which are as highly endowed with the attributes of acidity, as the strongest acids into which oxygen enters as a constituent. It is conceded in his letter that his language respecting these combinations cannot be reconciled with his declaration in one place that they do not combine with oxybases, and in another that a body which cannot so combine is not an acid. It strikes me, that the only way in which the admitted inconsistency of his description of these bodies, with his definition of acidity, can be avoided, is by assuming that they combine as acids with haloid bases, although decomposed by oxybases.

I will now proceed to comment on a new subject for consideration, presented in Berzelius's letter in reply to mine.

It must be evident that every oxysalt, composed of an oxacid and an oxybase, must consist of an atom of each radical, and as many atoms of oxygen as exist both in the acid and in the base. Thus sulphate of potash consists of an atom of potassium, an atom of sulphur and four atoms of oxygen, and may be represented either by SOOO KO or SOOOOK.

Berzelius in his letter repeats an ingenious suggestion previously advanced in his treatise, that SOOOO, (sulphur with four atoms of oxygen,) may act, as a compound halogen body like cyanogen, and thus form a salt by union with an atom of any radical. He conceives that the apparent want of analogy, which induced him to separate into two classes, the amphigen and halogen bodies, disappears under this view of the phænomena; and that his amphide salts might be considered as constituted of a compound halogen body and an elementary radical. But however we may admire the ingenuity of these suggestions, ere, in obedience to them, we extend the limits of the halogen class, I would request that the word salt should be defined, and that it be shown that consistently with any definition which can be devised, there is any class of bodies in nature which merit the appellation of salt-producers. Before enlarging the superstructure, let it be shown that the basement has been well grounded.

Berzelius lays some stress on the community of effect, in the evolution of hydrogen, both by acids formed by hydrogen with halogen bodies, and by diluted hydrous sulphuric acid, as evincing a similitude of composition justifying the suggestion above quoted from him. But I conceive that this common result is better explained by ascribing it to the tendency of radicals to displace each other from combination, whether existing in a simple or a complicated compound. If water exists as a base in hydrous sulphuric acid; as I have elsewhere suggested, we may consider this hydrous acid as a sulphate of the oxybase of hydrogen*; and that when it reacts with zinc or iron, the proneness of hydrogen to the aëriform state enables either metal to take its place, agreeably to the established laws of affinity.

It may be proper, before concluding, to explain more particularly the nomenclature which I have adopted.

The amphigen and halogen bodies of Berzelius, as they produce acids and bases according to my definition, are all classed as basacigen bodies. Of course oxygen, chlorine, bromine, iodine, fluorine, cyanogen, sulphur, selenium, and tellurium, are included in this class.

The general designation of a binary compound of a basacigen body, is the termination in *ide;* the special, the termination in *acid*, when the compound acts as an acid; in *base*, when it acts as a base.

Hence an oxide, may be an oxacid, or an oxybase;

a chloride,	a chloracid,	or a chloribase;
a bromide,	a bromacid,	or a bromibase;
an iodide,	an iodacid,	or an iodobase;
a cyanide,	a cyanacid,	or a cyanobase;
a sulphide,	a sulphacid,	or a sulphobase;
a selenide,	a selenacid,	or a selenibase;
a telluride,	a telluracid,	or a telluribase.

Compounds which consist of radicals only, are distinguished by the term *uret* equivalent to the French *ure.* Hence *carburet*, *phosphuret*, *boruret*, *silicuret*, *&c.*

Of any two binary compounds containing each the same basacigen body and forming one compound, the more electronegative is an acid, the other a base. Hence all the electronegative haloid compounds in the Berzelian double salts, are acids, and the electro-positive, bases. Where there are two such compounds, one containing one basacigen atom, the other two atoms or one and a half, the former has a termination in

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* See Lond. and Edinb. Phil. Mag. vol. vi. p. 328.

ous, the latter in *ic*. As for instance the *chlorureplatinoso-potassique* of Berzelius, is a compound of *chloro platinous acid*, and the *chlorobase of potassium*, and is the *chloro-platinite of potassium*. The *chlorureplatinico-potassique* of the same author, is the *chloroplatinate of potassium**.

By analogy the intelligent reader may easily make these examples a clue to designate any other of the double salts of Berzelius so as to accord with the plan in question. He may have a *bromoplatinate* or *bromoplatinite*, a *iodoplatinate* or *iodoplatinite*, a *fluoplatinate*, *&c.*; or changing the radical a *chloroaurate* or *chloroaurite*, a *bromoaurate* or *bromoaurite*, *&c.*

The terms amphigen and halogen being employed both from expediency, and in honour of their author, we may use his terms haloid and amphide, to distinguish the acids or bases severally formed by these classes, the abbreviations *halo* and *amph*, being employed in composition. Thus I designate the acids formed by the halogen bodies with hydrogen, as halohydric acids; those formed with that radical by the amphigen bodies, as amphydric acids. As the same radical will in other cases be found to form acids with several of the halogen bodies, platinum for instance, the acids thus produced may be called haloplatinic acids; or if gold were the radical, they would be called haloauric acids. These examples will suggest to the chemical reader a series of names, as for instance *haloargentic*, *halocupric*, *halostannic*, *halopalladic*.

I consider prussian blue as a cyanoferrite of the cyanobase of iron, or briefly a cyanoferrite of iron. The diversity of properties which enables two cyanides of iron to exist in combination in this cyanoferrite, one as an acid, the other as a base, is one among many other instances in which compounds constituted of the same elements in the same ratio, have different properties, and are said in consequence to be *isomeric*, or to afford cases of *isomerism*.

The salt designated by Berzelius as the "*cyanure ferroso-potassique*," is the well-known test for iron heretofore called ferroprussiate of potassa; under the idea that it consisted of

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* In designating salts of the metals proper, as for instance, the *nitrate of mercury*; the idea of the oxydizement of the metal is always understood, although usually not expressed. In the instance above cited, we actually mean the *nitrate* of the *oxide*, or *oxybase* of mercury. By analogy, I here use the term *chloroplatinate of potassium*, for *chloroplatinate* of the *chlorobase* of *potassium*. It is in fact well known to chemists, that acids do not unite directly with metals. The only alleged exception to this rule, of which I have any knowledge, is that of tellurium and sulphuric acid. It is inferred, therefore, that when an acid is combined with a metal, the latter must exist in the state of a base formed with the basacigen body which enters into the composition of the acid.

prussic acid, iron, and potassa. As the prussic acid was viewed at the same time as a compound of hydrogen and cyanogen, the ferroprussic acid was considered as a compound of cyanogen, hydrogen, and iron. According to Berzelius, the supposed *ferroprussiate* is a compound of a "*protocyanure*" of iron, and a "*cyanure* of *potassium*;" each being a simple haloid salt, and the aggregate a double "*cyanure*." Agreeably to my nomenclature, the "*protocyanure*" of iron is considered as cyanoferrous acid, and the "*cyanure*" of potassium as a cyanobase; the aggregate being a cyanoferrite of the cyanobase of potassium, but designated briefly as a cyanoferrite of potassium.

I infer that the "*ferroprussic*" acid is analogous in constitution to the triple compound of fluorine, silicon and hydrogen, improperly called hydrofluosilicic acid; and that, consistently with the hypothetical views under which the latter received its name, the former should be called hydrocyanoferric acid. Even admitting the correctness of the hypothetical impression, to which I have alluded, agreeably to which such compounds are acids with a double radical, I urged that the appellations of such compounds should be so altered as to give precedency to the electro-negative ingredient. Hence the one would be called cyanohydroferricacid; and the other, fluohydrosilicic acid. But in my letter to Silliman, already cited, I advanced a new hypothesis respecting the constitution of the fluohydrosilicic, and fluohydroboric acids. I suggested that they should be considered as compounds in which the fluorides of silicon or boron acted as acids, the fluoride of hydrogen as a base. Consistently with that doctrine, I would consider the *protocyanide* (or "*cyanure*") of iron in the alleged *ferroprussic acid*, as acting as *cyanoferrous acid*, the *cyanide* of *hydrogen* (*prussic acid*), as a *cyanobase*, forming, by their union, a cyanoferrite of hydrogen.

As compounds, consisting of a basacigen body, hydrogen and a radical, do not, when presented to bases, enter into combination; but are, on the contrary, decomposed so as to allow another radical to take place of their hydrogen, it is inconsistent with chemical law, as stated by Berzelius*, or my definition of acidity, (page 183,) to designate them as acids.

I have called the electro-negative "*protocyanure*" of iron of Berzelius, cyanofer*rous* acid, because there is "sesquicyanure" in the "*cyanureferrico-potassique*" of that author, which, by analogy with the nomenclature of the oxacids, is entitled to the appellation of cyanoferric acid.

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* *Traité*, page 41, vol. ii.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 8.—A Paper was in part read, entitled "Observations on the minute structure of the higher forms of Polypi, with observations on their classification." By Arthur Farre, M.B., Lecturer on Comparative Anatomy at St. Bartholomew's Hospital. Communicated by Richard Owen, Esq., F.R.S.

June 15.—The reading of Dr. A. Farre's paper was resumed and concluded.

After a short account of the labours of preceding naturalists in that department of zoology which comprises the various kinds of polypes, and of the different characters on which they have founded the classification of these animals, the author proceeds to the statement of his own observations on several species which had not been previously investigated with sufficient minuteness and care. Two of the species described he believes to be entirely new, and he has accordingly given them the names of *Bowerbankia densa,* and *Lagenella repens.* The other species which are the subject of the author's investigation, are *Vesicularia spinosa, Valkeria cuscuta, Alcyonidium diaphanum, Membranipora pilosa,* and *Notania loriculata.*

He then discusses the principles on which the classification of this tribe of zoophytes should be founded, and proposes on these principles to give the name of *Ciliobrachiata* to the whole group of polypes characterized by the possession of ciliated tentacula, and a free alimentary canal with two orifices : this group again he divides into two subordinate groups, namely, the *Hydriform* and the *Actiniform,* or *Zoanthiform* polypes. Under the title of *Nudibrachiata* he proposes to comprehend all those polypes which partake of the nature of the hydra, and whose tentacula are unprovided with cilia, corresponding to the *Anthozoa* of Ehrenberg.

"On the Temperature of Insects, and its connexion with the functions of Respiration and Circulation." By George Newport, Esq. Communicated by P. M. Roget, M.D., F.R.S.

The author states at the commencement of his paper, that, although it has been long known that insects living in society, as the bee and the ant, maintain in their habitations a temperature higher than that of the open air, the fact had never yet been established that individual insects of every kind possess a more elevated temperature than that of the medium in which they reside, and that in each species the degree of elevation varies in the different stages of their existence. He was first led to study the temperature of insects in consequence of the curious results which he had met with in some observations he had himself made, in the autumn of the year 1832, on a species of wild bee in its natural haunts, with a view to ascertain, as had been suggested to him by Dr. Marshall Hall, the relation between the temperature of these insects during their hybernation, and the irritability of their muscular fibre : but the fact of the existence of a higher temperature in individual insects had been ascertained by himself prior to these observations; the results of which observations, together with other facts connected with the physiology of insects, he subsequently communicated to Dr. M. Hall.

Since the time when the author has been engaged in the prosecution of this inquiry, some observations on the same subject have been published by Dr. Berthold, of Göttingen, who expresses it as his opinion that insects ought not to be regarded as cold-blooded animals, but who does not appear to have detected the existence of a temperature higher than the surrounding medium in any individual insect. The author also notices the observations on this subject made by Hansmann, Juch, Rengger, Dr. John Davy, and others, some of whom have detected, while others have not observed, the existence of an increased temperature in this class of animals. He then gives a detailed account of the precautions to be taken for ensuring accuracy in making observations of this kind; and remarks that greater reliance is to be placed on those made on the external than on the internal temperature of the animal, seeing that comparative results are all that can be obtained, and that the injury inflicted on the insect by its mutilation very materially interferes with the correctness of the conclusions as to the degree of internal temperature.

After premising these introductory remarks, the author gives a detailed account of his observations on the temperature of insects in their several states of larva, pupa, and imago, from which it appears that those which possess the highest temperature are always volant insects, and are chiefly diurnal species, residing almost constantly in the open air. He shows that the larva has a lower temperature than the imago, and that the energy of its respiration is also less, regard being had to the activity of the insect, and to the size of its body. In lepidopterous insects the average elevation of temperature above that of its surrounding medium, is in the larva from 0°·9 to 1°·5; while in the imago it is from 5° to 10°. Among the hymenoptera it is from 2° to 4° in the larva, and in the imago from 4° to 15° or even 20°; but in all cases the amount of this elevation is shown to depend on the degree of activity, and the quantity of air respired during a given period. The author then inquires into the influence of various circumstances, such as inactivity, sleep, hybernation, and inordinate excitement, on the temperature of insects; and shows that the evolution of heat gradually diminishes in a degree corresponding to the length of time during which the insect remains in a state of repose, but that it is immediately increased as soon as the insect is roused into action. He adverts also to the remote cause of hybernation, which he ascribes, in every state of the insect, to accumulations of adipose matter, or of nutrient fluid, which, being stored up in the system, induce a plethoric state, from which the animal is aroused when this store of materials has been exhausted. A variety of experiments are related, tending to prove that a large proportion of the heat evolved by an insect, when in a state of great activity, is dissipated into the surrounding medium, and that the quantity of heat so generated bears definite relations to the habits, the locality, and the energy of respiration in each respective species of insect. Volant insects, he finds, have the highest temperature; and of these the diurnal bear a higher temperature than the crepuscular; next to these must be placed the diurnal terrestrial, and last of all the nocturnal terrestrial species.

In the next division of this paper the author considers the temperature of those insects which live in societies; and in particular of the humble bee and the hive-bee. His observations are confirmatory of many of those of Huber relating to the incubating habits of the former of these species; and he has farther ascertained that during the act of incubation the bees possess a voluntary power of generating heat, whereby the temperature of their bodies is raised, apparently for the purpose of imparting warmth to the young in the cells; that this process is accompanied by accelerated respiration; and that the amount of heat evolved is proportional to the quantity of air respired. The law established by Dr. Edwards in the case of the young of mammiferous animals, namely, that they possess less power of generating heat, and that for a certain time they are unable to maintain their usual temperature, is shown by the author to be equally applicable to the early stage of insect life, and also to the perfect insect immediately after its development from the pupa.

The temperature of the hive-bee is next examined, and it is shown, contrary to the statements of Reaumur, Huber, and others, that bees do not maintain a very high temperature in their hives during winter, but that they are disposed, when not disturbed by any occasional vicissitudes of atmospheric temperature, to assume the state of hybernation; although, on the other hand, when the bees are much disturbed, the temperature of the hive may, even in the midst of winter, become greatly raised. The temperature of the hive is lowest in January, and gradually increases up to the period of swarming, in May or June, after which time it diminishes. A table is given exhibiting the results of successive observations on the influence of the diminution of heat and of light which attended the progress of the annular eclipse of the sun on the 15th of May, 1836, on the temperature of the hive.

It appears from the inquiries of the author that different parts of the hive do not preserve the same relative heat among one another at different periods, and also that the amount of free heat in the hive is often 10° or 15°, even in the months of July and August.

The remaining division of the paper is devoted to the consideration of the connexion existing between the development of heat and the functions of respiration, circulation, and digestion. The state of the pulse during all the different stages of the larva until its metamorphosis into the pupa is examined with great minuteness, and the results are given in a tabular form. The author traces the rate of pulsation during different conditions of repose and activity, and the corresponding frequency of respirations, and finds that although there is a general accordance between the activity of these two functions, yet that the activity of respiration and the quantity of heat evolved do not depend primarily on the velocity of the circulation, but that under all circumstances the quantity of heat developed is exactly proportional to the quantity of respiration. While the insect is feeding, and digestion is going on, the evolution of heat increases, and while it is fasting it diminishes; but this diminution has a limit, whereas increased respiration is invariably attended by increased heat. Gaseous matter is exhaled in great abundance from the surface of the body of an insect, and contributes to regulate and equalize its temperature; but the quantity diminishes in proportion to the length of time during which it has been deprived of food. The author maintains that animal heat is not an effect of mere nervous influence, either general or ganglionic; an opinion which he derives from the following considerations: first, that in many insects in which considerable degrees of heat are evolved, and the respiration is energetic, the nervous system is small compared with that of others in which the respiration is less vigorous; and secondly, that if the evolution of animal heat were dependent on the existence of ganglia, the leech ought to generate more heat than the larva of the Lepidoptera, since it has a much greater number of ganglia. Hence he is disposed to draw the general conclusion that animal heat results directly from the changes which take place during respiration; and that the reason why so large a quantity passes off so rapidly from the body of an insect is because it does not become latent, since the circulating fluid, unlike what takes place in the higher animals, is neither completely venous nor completely arterial, but of a character intermediate between both.

Twenty-one tables are annexed exhibiting the records of the experiments referred to in the paper on the respiration, temperature, and circulation of insects.

"Observations on the Dry-rot of Ships, and an effectual method to prevent it pointed out." By James Mease, M.D. Communicated by Charles Kœnig, Esq., For. Sec. R.S.

The method recommended by the author for preventing the occurrence of the dry-rot in ships is to impregnate the timbers and planks with common salt, as is practised by the ship-builders in Philadelphia. For this purpose all the spaces between the timbers and the outside and inside planks are to be filled with Spanish or Portugal salt, driven down as the filling proceeds. The salt is found to penetrate thoroughly, and completely to saturate the wood, combining with its native sap and preventing fermentation and the consequent evolution of foul air. The principal inconvenience attending this method is the dampness of the ships; an evil for which the author suggests various remedies.

"Experimental Researches on the conducting powers of wires for Electricity; and on the heat developed in metallic and liquid conductors." By the Rev. William Ritchie, L.L.D., Professor of Natural Philosophy in the Royal Institution of Great Britain, and of Natural Philosophy and Astronomy in University College, London.

In a former communication, published in the Philosophical Transactions for 1833, the author endeavoured to show that the quantity of voltaic electricity conducted, or the force of the current, was a function of a greater number of variables than had been previously supposed. As the theory which he proposed for estimating the conducting powers of substances has been controverted by M. Lenz*, he has been induced to reconsider the subject, and finds reason to be satisfied with the correctness of his former views. He farther finds that with feeble magnetic needles the deflecting forces are not proportional to the force of the current, but approach nearer and nearer to that proportion by increasing the magnetic power of the needles; a result which the author thinks is strictly deducible from the universal law of nature, that the attraction mutually exerted by two bodies is measured by the sum of their masses. He shows that the formula of Ohm, expressive of the conducting powers of wires, and of the resistances which they offer to currents of voltaic electricity, is an approximation to the truth only in the case of feeble currents, and that with the same metal, the conducting powers are not as the lengths of the wires.

The author next inquires into the relation between the heat developed, which he finds to be, in the same wire, as the square of the intensity of the current; and in wires of the same diameter, and conducting equal quantities of electricity, it is inversely as the conducting power, or directly as the resistance which they oppose to the current. The facts he has adduced in this paper seem to be at variance with the generally received theory of caloric, and to be in perfect accordance with the undulatory theory.

He concludes by describing an experiment confirming the views he has elsewhere advanced with regard to the difference between the physical, the physiological, and the chemical effects resulting from the employment of coils formed of wires of different lengths, being dependent on the time required by the conductor for returning to its natural state.

"On the Ipoh or Upas poison used by the Jacoons and other aboriginal tribes of the Malayan Peninsula." By Lieut. T. S. Newbold, Aide-de-Camp to Brigadier-General Wilson, C.B. Communicated by P. M. Roget, M.D., Sec. R.S.

The author gives an account of the process by which the Jacoons, an aboriginal tribe inhabiting the mountains and forests of the Malayan Peninsula, prepare the poison applied to the points of the slender arrows which are propelled from the *Simpitan* or blow-pipe. Three preparations are employed for this purpose, distinguished by the names of *Krohi*, *Tennik* or *Kennik*, and *Mallaye*; the last of these is more powerful than the other two, and is obtained from the roots of the *Tuba*, the *Perachi*, the *Kopah*, and the *Chey*, and from that of the shrub *Mallaye*, whence it derives its name. The *Krohi* poison is prepared from the root and bark of the *Spoh* tree, and the roots of the *Tuba* and *Kopah*, with the addition of red arsenic and the juice of limes; and the *Tennik* from the same ingredients, omitting the Kopah root. A few experiments are related, made by the author with a view to ascertain the effects of the poisoned arrows on

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* See SCIENTIFIC MEMOIRS, vol. i. p. 311.

living animals, from which it appears that the train of symptoms commences in a few minutes after the infliction of the wound, and terminates fatally with more or less rapidity, according to the size of the animal.

"Della Velocità del Vento. Memoria diretta alla Regali Società di Londra per essere inscritta nelle Transazioni Filosofiche, et per concorso del premio annuale di fisica: di Luigi Dau, Dottore in Matematica e Fisica." Communicated by Charles Kœnig, Esq. For. Sec. R.S.

The author endeavours to investigate the relation which he believes exists between the velocity of the wind and the oscillations of the barometer, and thence to derive rules for calculating the former from observations of the latter.

"Considérations physiques sur le passage Nord-ouest;" by the same. Communicated by the Right Hon. the Earl of Minto, G.C.B. F.R.S.

The author of this memoir, considering that the practicability of a North-west Arctic passage must depend on the mean summer atmospheric temperature of the most northern point of the continent of America being above that at which the congelation of sea water takes place, applies himself to the determination of these temperatures. The results of his calculations are given in a table, exhibiting the extreme and the mean temperatures of the atmosphere for each of the summer months, from May to September, at all degrees of latitude, from 60° to 80° inclusive. According to this table, the temperature of zero, which is about the freezing point of sea water, prevails, at 60° of latitude, on the 10th of May; at 61° lat. on the 20th of May; at 63°, on the 1st of June; at 65°, on the 10th of June; at 67°, on the 20th of June; and at 71°, during the whole of the months of July and August. The author concludes that navigators can reach, without danger of being obstructed by ice, the latitude of 71° during these latter months: and that since the American continent does not probably extend beyond 70° north latitude a passage to the North-west is then open. He recommends, however, that instead of attempting it by the dangerous navigation of the polar sea, a coasting voyage between the continent and the numerous islands which exist in that ocean should be undertaken; or, what he thinks still more promising of success, an expedition by land for exploring the country intervening between the Coppermine River and Hudson's Bay.

"Causes de la Variation diurne de l'Aiguille aimantée, de la Lumière zodiacale, des Aurores Boreales, et Méthode simplifiée pour le relevement des Longitudes, Mémoire soumis à la Société Royale de Londres, pour le concours du prix d'Astronomie. Par Demonville."

The author's speculations proceed on the hypothesis he has adopted, that the Sun, Moon, Jupiter and Mars perform a diurnal and perfectly circular revolution round the earth.

"On the elementary structure of the Muscular Fibre of Animal and Organic Life;" by Frederic C. Skey, Esq., Assistant Surgeon to St. Bartholomew's Hospital, F.R.S.

The author having withdrawn the paper bearing the same title which he had formerly communicated, and which was read to the

Society on the 9th and 16th of February last*; and having made in it several alterations and additions, consisting chiefly in notices of the discoveries of preceding anatomists in the same field of inquiry, again presents it to the Society, with these improvements.

"Sequel to an Essay on the Constitution of the Atmosphere published in the Philosophical Transactions for 1826†; with some account of the Sulphurets of Lime;" by John Dalton, D.C.L., F.R.S.

The author communicates in this paper an account of the investigations on the constitution of the atmosphere, which have engaged his attention during a long period of years. He enters into an examination of the comparative advantages of the three methods which are most in use for analysing common air, namely, firing it with hydrogen in Volta's eudiometer, or abstracting the oxygen by means of nitrous gas and quadrisulphuret of lime; and details the precautions to be taken in the employment of each of these methods, and the degree of accuracy to be expected from the results under different circumstances. He then relates numerous experiments made on air obtained from great heights, from which he is led to the conclusion that the proportion of oxygen to azote in the atmosphere on the surface of the earth is not precisely the same at all places and times; and that in elevated regions this proportion is somewhat less than at the surface of the earth, but not nearly so much as the theory of mixed gases would require, and that the reason for this is to be found in the incessant agitation of the atmosphere produced by winds and other causes.

"Researches on the Tides. Eighth Series. On the progress of the Diurnal-Inequality-wave along the coasts of Europe." By the Rev. William Whewell, F.R.S., &c.

In the seventh series of these researches ‡, the author pointed out the laws which the diurnal inequality of the height of high water follows, and showed that those laws are modified so as to exhibit very remarkable differences at different places, and to occasion some difficulty in conceiving the mechanical propagation of the tide-wave. He then suggested what appeared to be a possible solution of the difficulty; but as this suggestion was founded on facts from a few places only, he resolved to attempt to trace the progress of the wave which brings the diurnal inequality on some of the coasts, on which simultaneous observations were made at his request in June 1835; and the present memoir contains an account of the conclusions to which he has been led by this investigation. The details which he gives of the observations made, with this view, at nineteen different stations, appear to establish the conclusion, that the differences of diurnal inequalities at different places are governed by local circumstances, and do not form a progressive series.

"Note on the Fluctuations of the Height of High-water due to changes in the Atmospheric Pressure." By J. W. Lubbock, Esq., F.R.S.

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* See our last volume, p. 377.

† An abstract of Dr. Dalton's Essay on the Constitution of the Atmosphere will be found in Phil. Mag. First Series, vol. lxviii. p. 310.

‡ See our last volume, p. 380.

The author verified, both at Liverpool and at London, the existence of a fact similar to that which M. Daussy had ascertained at Brest, namely, the rise of the ocean when the barometer is depressed; and remarks that the correction due to changes in the atmospheric pressure is by no means inconsiderable. He suggests the question whether the surface of the ocean rises in narrow seas simultaneously with the depression of the barometer, or otherwise. With a view to the solution of this question, he gives a tabular diagram showing the correspondence between the calculated and the observed heights, in their relation to the heights of the barometer at Liverpool and at London, from which it would appear that the effect of changes in the atmospheric pressure on the tide is immediate.

"On an improved mode of constructing Magnets." By James Cunningham, Esq., Member of the Cork Scientific and Literary Society. Communicated by North Ludlow Beamish, Esq., F.R.S., President of that Society.

The material recommended by the author for the most economical, as well as effectual method of constructing magnets, is cast iron, which should be formed in small castings in the form of a horse-shoe, each weighing about seven ounces; these he finds, on being touched in the usual manner by a small compound magnet, received and retained the impregnation better than any which he had previously constructed of steel.

The Society then adjourned over the long vacation, to meet again on the 16th of November next.

ZOOLOGICAL SOCIETY.

December 13, 1836.—Part of a paper by M. Frederick Cuvier was read, on the Family of the *Dipodidæ*, including the *Jerboas* and *Gerbillas*[*].

Notes on the anatomy of the Spermaceti Whale, (*Physeter macrocephalus*, Auctorum,) principally relating to its dentition, and to the structure and appearances presented by the soft parts, by Mr. F. Debell Bennett, Corresponding Member of the Society, were then read.

Mr. Bennett remarks that a greater disproportion exists between the sexes in this species of *Whale* than is observed in any other cetaceous animal; for while the usual length of the largest male *Cachalots*, taken in the South Seas, is about 60 feet, that of full-grown females is only 28, and rarely, if ever, exceeding 35.

When the young male *Cachalot* has attained the length of 34 feet, its teeth are perfectly formed, though not visible until it exceeds 28. The upper jaw usually described as toothless, has on either side a short row of teeth, sometimes occupying the bottom of the cavities which receive the teeth of the lower-jaw, but generally corresponding to the intervals between them. The entire length of these teeth is about three inches; they are slightly curved backwards, and elevated

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[*] The abstract of this and the concluding part of the Memoir will be found in the Proceedings for December 27, 1836.

about half an inch above the soft parts, in which they are deeply imbedded, having only a slight attachment to the maxillary bone. Their number is not readily ascertained, because the whole series are not always apparent; but in two instances Mr. Bennett found 8 on each side. These teeth exist in adult *Whales* of both sexes, and though not visible externally in the young *Cachalots*, may be seen upon the removal of the soft parts from the interior of the jaw.

"The eye of the *Cachalot* is small, and placed far back on the head, above and between the pectoral fin and angle of the lower jaw. Its situation is chiefly marked by a raised portion of integument around it. The aperture for vision does not exceed 2 inches in the longitudinal, and 1 inch in the vertical direction. The eyelids are without *cilia* and tarsal cartilages; they are composed of two horizontal bands of integument, each, in the example from which I describe (viz. a half-grown male), two inches in depth, and connected with each other at the inner and outer *canthus*. Between each of the eyelids and the blubber exists a distinct line of separation, marked by a somewhat deep groove, having a duplicature of thin membrane, serving as a surface or hinge on which the lids move. At these lines of demarcation all integument partaking of the nature of fat ceases, and the texture of the *tarsi* thus insulated is composed solely of common skin and cellular and other membranes, together with a dense layer of muscular fibres deposited in its centre. The *conjunctiva* of the lids is highly vascular, injected with blood, and covered with orifices of mucous ducts. At the inner canthus of the eye it forms a thick duplicature, of crescentic form, constituting a rudimental third eyelid, not unlike the haw of the horse. The globe of the eye is chiefly lodged in the soft parts, but little if any of its substance entering the bony orbit. It is deeply set within the lids, and does not in size much exceed that of an ox. Its size in an adult female was $2\frac{1}{2}$ inches in the longitudinal, and the same in the vertical direction. The interior or cavity was $1\frac{1}{2}$ inch in each of the last-named directions, and its depth $\frac{2}{3}$rds of an inch only.

"The globe at its greatest circumference was $7\frac{1}{2}$ inches: the transparent *cornea* at its transverse or broadest diameter measured 1 inch, and in its vertical or narrowest $\frac{3}{4}$ths of an inch. The muscles of the globe formed a dense mass surrounding the sheath of the optic nerve, and were inserted in one continuous line over the circumference of the globe at its greatest convexity.

"The optic nerve before penetrating the sclerotic is continued to some length. It does not exceed the circumference of a crow's quill, but is surrounded by a dense fibrous sheath nearly 4 inches in perimeter, and which, where the nerve perforates the globe, terminates on the posterior surface of the latter. Around the globe and its muscles much cellular tissue and true fat are deposited. The eyeball in shape is not a perfect sphere; its anterior and posterior surfaces are flattened: that portion of the *conjunctiva* of the globe immediately surrounding the *cornea*, and the only portion exposed between the aperture of the lids, is of an intense black hue. It is possible this dark portion may be a membrane distinct from the *conjunctiva*, since around the extent it occupies, it terminates by an irregular margin, and is capable of being detached from the *conjunctiva*, when it presents the form of a delicate layer of cuticle, with a black pigment deposited beneath its surface*.

"The *cornea* of the Cachalot is dense, and composed of many layers; when divided, a small quantity of limpid aqueous humour flows forth: the anterior chamber of the eye is very limited, and the crystalline lens projects into it through the pupillary aperture. The iris is a coarse membrane of a dull-brown colour, with a narrow zone of lighter hue surrounding its outer margin. Its inner and free margin is very thin, and embraces the protruding convexity of the lens.

"The lens is small, certainly not exceeding in size that of the human eye: it forms nearly a perfect sphere: the vitreous humour tolerably abundant. The retina was spread with beautifully delicate arborescent vessels, and afforded a small bright spot at the insertion of the optic nerve. Beneath the retina was spread a *tapetum* of dense membranous texture, and yellow-green or erugo-green colour. The sclerotic at its posterior third is thick, fibrous, and resisting, whilst its anterior third is thin and flexible; no lachrymal apparatus exists."

In the description of the organs of generation, the cavity in the head containing the spermaceti, and some more of the soft parts, Mr. Bennett's observations coincide with those of Hunter and other comparative anatomists.

A *fœtus* apparently of mature growth, taken from the *abdomen* of a *Sperm Whale*, measured 14 feet in length and 6 in girth; its position in the *uterus* was that of a bent bow.

Mr. Reid brought before the notice of the Meeting a new species of the genus *Perameles*, and read a paper giving some account of its habits, and pointing out its distinguishing characters.

The author states that he was indebted to William Holmes, Esq., of Lyon's Inn, for the opportunity of exhibiting this specimen, which was brought from Van Diemen's Land, where these animals are said to be common. The same species is also found in Western Australia, and is there called by the natives *Dalgheit*, and by the colonists the *Rabbit*, under which name it is mentioned by Cunningham in his work on New South Wales. Widdowson, in his account of Van Diemen's Land, notices it; but neither of these writers has given any description of the animal. From its resemblance to the Rabbit, Mr. Reid proposes for it the specific name of *Lagotis*.

PERAMELES LAGOTIS. *Per. griseus, capite, nuchâ, et dorso, castaneo lavatis; buccis, lateribus colli, scapulis, lateribus, femoribus extus, caudâque ad basin, pallide castaneis; mento, gulâ, pectore, abdomine, extremitatibus intùs anticèque, antibrachiis postice, pedibusque suprà albidis; antibrachiis externè pallidè griseis, femoribus extus posticeque saturatè plumbeis; caudâ, pilis longis albescentibus ad partem basalem, indutâ, dein pilis nigris tectâ, parte apicali albâ, pilis longis supra ornatâ. Vellere longo molli. Caudâ pilis rudis vestitâ; pilis ad pedes brevissimis. Labio superiore, buccisque, mystacibus longis sparsis. Auriculis longis,*

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* A slight dark tint around the cornea is not uncommon amongst the dark-skinned natives of warm countries.

ovatis, intùs nudis, extus pilis brevissimis brunneis, ad marginem, albescentibus indutis, pilis ad bases eos plumbeis, apicibus albis aut castaneis, illis in abdomine omninò albis. Marsupio ventrali magno, mammis novem, in faciem posticam; quarum una centralis est, reliquis circumdata, intervallis æqualibus, gyrumque facientibus, transversìm unciam cum quadrante reddentem.

	poll.	lin.
Long. capitis	5	3
—— corporis	13	0
—— caudæ	10	0
—— auriculæ	3	10
—— antibrachii	4	0
—— pedis antici	1	8
—— tibiæ	3	9
—— pedis postici	4	6
—— ab auriculæ basi usque ad oculum	2	0
—— ab oculo usque ad nasum	2	8
Latitudo auriculæ	1	9

Hab. In Australiâ Occidentali et in Terrâ Van Diemen.

"The ears are long, broad, and ovate, having several semitransparent dots scattered over their surface (the remains of sebaceous glands). On the anterior extremity the nails are much elongated; the second and third are about ¼th of an inch longer than the first; they are all flattened at the tips, thus furnishing the animal with a very efficient apparatus for burrowing. The tail offers many differences from that of the other species of the genus *Perameles.* The basal fourth is clothed with hairs about the same length and colour as those of the body. The middle half is black, the hairs on the upper part being elongated; the remaining part is white, with a ridge of long white stiff hairs forming a crest.

"The pouch in this specimen (a female) is large, and has 9 nipples on its posterior surface; one being placed in the centre, and the remainder at equal distances form a circle, the diameter of which is 1 inch 3 lines.

"The skull is perfect, but the state of the skin was such as totally to prevent its removal, and the description is therefore defective in particulars concerning the bones of the face. The interparietal and occipital crests are clearly defined and large. The bulla of the ear is large, and its shape that of a flattened ovoid. The tympanum was entire, and on removing it the manubrium of the malleus was found to be twice the length of its body. The zygomatic arch is imperfect for about the space of ½ an inch. The lower-jaw is slender, with a salient process at its angle. Dent.: Prim. $\frac{5-5}{6}$, Can. $\frac{1-1}{1-1}$, Mol. spur. $\frac{3-3}{3-3}$, Mol. ver. $\frac{4-4}{4-4}$, = 48.

"The two front superior incisors are nearly a line apart, small, and quadrangular; a small space intervenes between these and the three succeeding, which are larger, and placed in a continuous series. The fourth and fifth incisors are about the same distance from each other as the two anterior. Posterior to the incisors is a space about 5 lines in width, for the reception of the inferior canines. The canines are well developed: another space intervenes between them and the false molars, which latter are all rather widely separated, of a conical shape, and have a small tubercle anterior to the body of the tooth.

"The molars of *Perameles*, as figured by M. F. Cuvier in his '*Dents des Mammifères*,' consist of two prisms fixed to a slightly curved base, with the concavity towards the inside of the jaw; but in this species the molars are quadrangular, having had but two sets of tubercles, and in the present specimen these teeth are worn down and present a square surface, inclosed by enamel, having a band of the same running transversely across the middle of the tooth. The two last molars of the upper jaw approximate so closely, as to require careful examination to detect the line of separation. The teeth of the lower jaw, except in number and in the circumstance of all the incisors forming a continuous series, do not differ from those of the upper. When the jaws are closed, the posterior molars of the upper and lower jaws are in contact.

"A friend of Mr. Gould's, residing in Western Australia, states that these animals are found beyond the mountains of Swan River, in the district of York. They feed upon large maggots and the roots of trees, and do considerable damage to the maize and potato crops by burrowing. A specimen kept by him in confinement became in a few days very docile, but was irritable, and resented the slightest affront or ill usage. It took bread, which it held in its fore-paws. A young one to which it gave birth unfortunately escaped, after being carried in the mother's pouch for several days."

Mr. Reid considers the distinctions between this and the rest of the species belonging to the genus *Perameles* so marked, that should more of the same form be discovered, the above characters would constitute a subgenus to which the name of *Macrotis* might be applied.

Mr. Waterhouse exhibited a second specimen of *Myrmecobius*, and directed the attention of the Meeting to certain differences existing between it and the one upon which he had founded the characters of the genus, and described under the specific name of '*fasciatus*[*].'

The present animal differs from the one previously described in having the black and fulvous colouring of the back less decided, owing to a larger proportion of interspersed white hairs. The fasciæ, instead of being white, are of a yellowish cream-colour, and they also differ in number and arrangement. Commencing from the tail, the three first are distinct and uninterrupted, the intermediate spaces being about ½ an inch in width, black, with white hairs interspersed, and a few of an ochraceous colour. The fourth is also distinct, but instead of being continued across the back, it is met by two fasciæ from the opposite side. The two following are continuous, but less distinct than either of the foregoing. Beyond these, the fasciæ are

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[*] Mr. Waterhouse's former description of this genus will be found in Lond. and Edinb. Phil. Mag., vol. ix. p. 520.

almost obsolete, there being only faint indications of them on the sides of the body.

The most important distinction, however, exists in the teeth, the present specimen possessing altogether four more molars than the one brought before the notice of the Society on a previous occasion. The entire number of teeth is 52, (26 in each jaw), and the 5 posterior molars are placed closely together, differing in that respect from those of the previously examined specimen.

The animal was brought from Van Diemen's Land, and others similar to it were observed scratching at the roots of trees, and feeding upon the insects which are generally abundant in such situations. Their favourite haunts are stated to be the localities in which the Port Jackson willow is most plentiful.

Mr. Waterhouse remarked that although the differences between the two animals were considerable, yet he did not consider the distinctions such as to justify his characterizing the one then before the Meeting as a second species.

GEOLOGICAL SOCIETY.

March 22.—A paper "On the Ancient State of the North American Continent;" by Thomas Roy, Esq., Civil Engineer, Toronto, Upper Canada, and communicated by Charles Lyell, Esq., F.G.S., was read in part.

April 5.—The reading of Mr. Roy's paper was concluded.

The author having in the course of his professional duties, discovered in the lake district of Upper Canada terraces or level ridges which agreed in elevation at considerable horizontal distances, he was induced to extend his inquiries and to ascertain how far similar phænomena have been observed in other parts of North America,—what may have been the probable extent of the lake or sea by which the ridges were formed,—and by what operations the waters were drained off, leaving only the present detached Canadian lakes.

With a view to ascertain the probable extent of the sea, Mr. Roy traced upwards from Lake Ontario the successive ridges or terraces*, and ascertained that their greatest height was 762 feet above the lake, or 996 feet above the ocean; he therefore assumed, that the boundary of the ancient sea must have had an elevation of at least 1000 feet, and in consequence, that it must have been formed on the west by the rocks and mountains ranging from the table-land of Mexico to the parallel of 47° of latitude; on the north by the barrier which separates the head-waters of the lakes from those of the Arctic rivers, and extending to Cape Tourmente below Quebec; on the east by the hills stretching through the United States to the Gulf of Mexico; and on the south by a mountain ridge which has been destroyed. The area thus circumscribed is calculated to be 960,000 square miles.

The chief geological feature connected with Mr. Roy's observations

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* These parallel ridges were exhibited on a section extending obliquely from the mouth of the Niagara to the south of Lake Ontario, and over the ridge north of that lake, to Lake Simcoe.

and described in the paper, is the high land which separates Lake Ontario from Lake Simcoe. The distance between these bodies of water is 42 miles, and the greatest elevation of the ridge is 762 feet above Lake Ontario, or 282 above lake Simcoe. The lowest visible formation is a stratified blue clay which effervesces freely and is of unknown depth. Above this are immense masses of clay, sometimes resembling fuller's earth, and sand most irregularly associated. The central and northern divisions of the ridge are thickly strewed, even to the highest peaks, with a great variety of boulders, many of them of immense size, and for the greater part derived from primary or transition formations. Many of them are rounded, and others decayed by weathering, whilst the edges of some are perfectly entire. On the southern side of the ridge boulders are not so common.

The manner in which the materials composing this ridge are arranged, resembles, in Mr. Roy's opinion, that in which drifted matter is now disposed along the margin of the lakes at the breaking up of the ice; and hence he conceives, that the ridges may, to a considerable extent, have been accumulated in a similar manner.

The author then enters into a calculation of the quantity of water hourly discharged by the Saint Lawrence, the Mississippi and the Hudson, amounting, according to his estimate, to 4000 millions of cubic feet; and afterwards proceeds to show, that in order to reduce the ancient lake 30 feet, the distance between two of the highest parallel ridges, fifteen years would be required, supposing the discharge to be double that at present.

Mr. Roy next details with considerable minuteness, the processes by which he supposes this vast sea was drained; but as his descriptions cannot be successfully followed without the aid of diagrams, they do not admit of being given in the Society's Proceedings.

A paper "On the Geology of the neighbourhood of Smyrna;" by Hugh Edwin Strickland, Esq., F.G.S., was then read.

The district described consists of two ranges of high land, running from east to west, separated by the bay of Smyrna and the alluvial plain of Bournabat, at the eastern termination of which is a transverse ridge uniting the two ranges.

The rocks belong to the formations described by the author in his former paper on Asia Minor (Proceedings of Geological Society, vol. II, p. 424, &c. or L. & E. Phil. Mag., vol. x. p. 68), and are: 1. Hippurite limestone and schist; 2. Tertiary limestone and marl; 3. Trachytic rocks.

The author supposes that mountains of hippurite limestone formed the boundaries of a lake in which tertiary beds were deposited, which were afterwards broken up by the eruption of trachytic rocks, and that the same event may have produced the drainage of the lake subsequently carried on by the denudation of its outlet, which is now traversed by the Meles.

1. The hippurite limestone on the south side of Smyrna bay is accompanied by much black, greenish, or cream-coloured sandstone, analogous to the macigno of Italy; but it is difficult to decide whether the schist or limestone is highest in geological position. The

latter extends from 15 to 20 miles to the east along the north side of the Tmolus range; but in Mount Corax the limestone is commonly absent, and schistose rocks prevail, in some places yellowish and friable, in others dark-coloured and compact; a fine-grained quartzose conglomerate also occurs.

2. The tertiary lacustrine limestone on the south side of the bay forms a table-land extending south from Smyrna about 15 miles. It consists of whitish limestone in horizontal beds containing nodules and layers of *quartz resinite.* Planorbes and paludinæ abound at certain points, but are not generally diffused. Greenish marls alternate with the limestone, and prevail in the central parts of the area where they contain beds of rolled gravel of nummulitic limestone, schist, and red trachyte. Conglomerate beds also prevail around the margin.

3. A vast mass of trachyte appears near Smyrna, and from thence to spread over the lacustrine beds. It consists chiefly of reddish porphyritic trachyte, in some places stratified on a small scale. A breccia of fragments of blackish trachyte in a red paste is also frequent. The aqueous and igneous rocks do not alternate; the lacustrine beds do not contain igneous materials, and no dikes are observed. On the south side the trachyte overlies the lacustrine formation like a lava coulée, the uppermost bed of the latter in contact with the trachyte being a conglomerate of hippurite limestone and schist with no traces of igneous matter.

Mount Pagus, south of Smyrna, is chiefly trachyte overlying the lacustrine beds, which crop out on the north side and dip towards the centre of the hill, consisting of sandy strata with vegetable remains and shells of helix and unio, and between them and the trachyte is the limestone conglomerate. At the north-east foot of the hill is a small isolated mass of yellowish schist and hippurite limestone about an acre in extent.

The north side of Smyrna bay presents appearances analogous to those on the south. The grey hippurite limestone of Mount Sipylus is accompanied as at Mount Tartali by black and greenish schists. The lacustrine deposits are overlaid by a great mass of trachyte which forms the lofty mountain of Cordileon. The relation of the lacustrine beds to the grey hippurite limestone is well exposed in a ravine north of Bournabat; and a few yards further north they contain beautiful impressions of leaves referred to the genera Laurus, Nerium, Olea, Salix, Quercus, and Tamarix; also shells of Cyclas, Paludina, Planorbis, and Cypris. In ascending hence to the west the lacustrine marls are surmounted by non-volcanic conglomerate, identical with that of Mount Pagus. Above this are beds of tufaceous conglomerate, and at the top the brown trachyte of Cordileon, resembling that near Smyrna, and like it sometimes laminated, but presenting little variety except in a long ridge opposite Smyrna which consists of whitish decomposed earthy trachyte.

A letter was next read addressed to Sir Charles Lemon, Bart., F.G.S., by R. W. Fox, Esq., "On the process by which Mineral Veins have been filled*."

[* See Lond. and Edinb. Phil. Mag., vol. ix. p. 387, vol. x. p. 394.]

Mr. Fox admits that the non-mechanical deposits in mineral veins may be due, in part, to infiltration from the enclosing rocks; but it appears to him, that such deposits might have been derived in almost indefinite quantities, by the circulation or ascension of currents of heated water from the deeper parts of the original fissures. He says, that water in this state possesses very great powers as a solvent, and would therefore become highly charged with earthy and metallic salts; but that in ascending it would lose its temperature, and consequently deposit the solutions against the sides of the veins.

He is of opinion, that the arrangement of the ore in different rocks, cannot be due to simple chemical affinity only, because the accumulation of the metallic masses is not found, in Cornwall at least, to depend on the nature of the containing rock, the ore of a given metal being sometimes found in granite, sometimes in elvan, sometimes in killas. This preference of one rock to another he conceives may have resulted from the relative position of the mineral masses, but to be chiefly due to electricity.

Extracts from two letters "On the Earthquake in Syria in January last;" addressed by Mr. Moore, his Majesty's Consul-General at Beyrout, to Viscount Palmerston, and communicated by J. Backhouse, Esq., and the Hon. W. T. H. Fox Strangways, F.G.S., Under Secretaries of State, were also read.

The first letter, dated Beyrout, Jan. 2nd, 1837, announces that the earthquake was felt in that city at thirty-five minutes past four o'clock in the afternoon of the preceding day. It was accompanied by a rumbling noise, which lasted about ten seconds, and appeared to proceed from the north. No buildings were thrown down in the town, but seven or eight without the walls, and one or two lives were lost. In the neighbourhood of Beyrout the course of the river Ontilias was suspended, and mills built on its banks were deprived of water for some hours. When the stream returned it was turbid, and of a reddish sandy colour.

During the day of the earthquake the atmosphere was close and charged with electricity. Fahrenheit's thermometer stood at 66°, but five minutes after the earthquake it rose to 70°. Four or five minutes after the shock the compass was still agitated. The oldest inhabitants did not remember so severe an earthquake.

The second letter was written also at Beyrout, partly on the 9th of January, and partly on the 23rd. It contains detailed accounts of the damage which had been done to numerous towns and villages. At Damascus, four minarets and several houses were thrown down; and at Acre, part of the walls and some buildings. Saffet was entirely destroyed, and nearly all the population, amounting to between four and five thousand, perished. The ground near the city was rent into fearful chasms, and up to the last accounts shocks were felt daily. Tiberias was also entirely destroyed, except the baths; and the lake rose and drowned many of the inhabitants. The dispatch contains a list of thirty-nine villages which had been totally destroyed, and six partially; and Mr. Moore says, it had been ascertained that the earthquake was felt on a line of five hundred miles in length by ninety in breadth. It was also perceived in the island of Cyprus.

April 19.—A paper was read, entitled "A description of the Cranium of the *Toxodon Platensis*, a gigantic extinct mammiferous species, referrible by its dentition to the *Rodentia*, but with affinities to the *Pachydermata* and the *Herbivorous Cetacea*;" by Richard Owen, Esq., F.R.S., Hunterian Professor of Anatomy to the Royal College of Surgeons*.

The author premises his anatomical description of the present fossil, by an abstract from Mr. Darwin's account of the geological structure of the district in which the cranium was found, from which it appears that it was imbedded in a whitish argillaceous earth, forming part of the banks of the Sarandis, a small stream entering the Rio Negro, and about 120 miles distant to the north-west of Monte Video.

The foundation of the whole surrounding country is granitic, but covered, often to a considerable thickness, by a reddish argillaceous soil, containing small calcareous concretions.

The cranium in question equals in size that of the hippopotamus, measuring two feet four inches in length, and one foot four inches in extreme breadth.

The form of the skull is elongate, depressed, and chiefly remarkable for the strength and wide expanse of the zygomatic arches, and the aspect of the occipital foramen and occipital region of the skull, which slopes from below upwards and forwards. The maxillary portion of the skull is compressed laterally, narrow, with large intermaxillary bones, slightly dilated at their extremity.

The teeth consist of molars and incisors. The latter are four in number in the upper jaw, the two middle ones very small, the two external ones very large, curved, and with their sockets extending backwards in an arched direction, through the intermaxillary bones to the maxillary, and terminating, without diminishing in size, immediately anterior to the grinding teeth, where the large persistent pulps of these incisors were lodged. In form and relative size these teeth must have resembled the *dentes scalprarii* of the *Rodentia*.

The molar teeth no less present a close approximation in their form and structure to the molar teeth of the herbivorous rodents; as is demonstrated in the detailed descriptions of one of these teeth found by Mr. Darwin in another locality, but belonging to the same species of *Toxodon*, and to an individual of the same size as that to which the cranium here described belonged; and of a portion of another molar lodged in one of the sockets of the same cranium. The molar teeth are seven in number on each side of the upper jaw, and from the form of the sockets appear to have corresponded with each other in structure.

After this description of the teeth, the form, proportions, disposition and connections of the different bones of the cranium are pointed out; and the structure of the osseous cavities subservient to the organ of

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[* See our last volume, p. 404.]

sense is adverted to; and deductions as to the aquatic habits of the Toxodon are founded on these observations.

So far as regards the form and position of the external aperture of the bony nostrils, and of the occipital condyles, and the slope of the plane of the occipital region of the skull, the same arguments might be advanced for referring the *Toxodon* to the mammiferous group containing the Dugong, as have been recently urged in reference to the *Dinotherium,* but the existence of air-cells or sinuses in the superior parietes of the cranium in the *Toxodon,* show that the cranial characters above alluded to, are not conclusive as to the cetaceous nature of an extinct mammal.

The general conclusions respecting the affinities which the *Toxodon* bears to existing orders of mammalia, so far as opinions can be formed from the portion of the skeleton preserved, are summed up by the author as follows:

So far as dental characters have weight, the *Toxodon* must be referred to the rodent order; but from this order it deviates in the relative position of the supernumerary incisors, and in the number and direction of the curvature of the molars.

It again deviates in the transverse direction of the joint of the lower jaw, and in the relative position of the glenoid cavities and zygomatic arches. In the aspect of the plane of the occipital foramen, and occipital region of the skull, in the form and position of the occipital condyles,—the aspect of the plane of the bony aperture of the nostrils, and in the thickness and texture of the osseous parietes of the skull, the *Toxodon* deviates both from the *Rodentia* and existing *Pachydermata,* and manifests an affinity to the *Dinotherium* and the *Cetaceous* order.

The author observes, however, that the development of the nasal cavity and the presence of frontal sinuses, render it extremely improbable that the habits of the *Toxodon* were so exclusively aquatic as would result from the total absence of hinder extremities, and concludes, therefore, that it is a quadruped, and not a Cetacean; and that it manifests an additional step in the gradation of mammiferous forms leading from the *Rodentia,* through the *Pachydermata* to the *Cetacea;* a gradation of which the water-hog of South America (*Hydrochærus Capybara*) already indicates the commencement amongst existing *Rodentia,* of which order, it is interesting to observe, that this species is the largest, while at the same time it is peculiar to the continent in which the remains of the gigantic *Toxodon* were discovered*.

May 3.—A paper was first read, entitled "A Sketch of the Deposits containing extinct Mammalia in the neighbourhood of the Plata;" by Charles Darwin, Esq., F.G.S.

Mr. Darwin premised his account of the geological features of the district in which the remains of the *Toxodon,* described at the meeting on the 19th of April by Mr. Owen, as above, were found, by remarking that as the other mammalia and the fossil shells had not yet been accurately examined, the notice was necessarily imperfect.

[* See note *, p. 208.]

To the westward and southward of the great estuary of La Plata, extend those level and almost boundless plains which are known by the name of the Pampas. Their geological constitution over many hundred square miles does not vary. It consists of a reddish argillaceous earth, which generally contains irregular concretions of a white aluminous limestone, or indurated marl, often passing irregularly into a compact calcareous stone, traversed by small linear cavities, similar to those which occur in many of the freshwater limestones of Europe. In the province of Entre Rios, the formation which composes the surface of the Pampas overlies and passes into a series of beds of sand, clay, and crystalline cellular limestone; containing sharks' teeth, gigantic oysters, and other shells belonging to the genera arca, venus and pecten. These shells, with the exception of the oyster, have a general resemblance to existing species. To the northward and eastward of the Plata, the province of Banda Oriental, though very low and level, consists of gneiss, granite and primary slate. These rocks are generally concealed by a considerable thickness of a reddish earth, which, though at first sight like ordinary detritus, belongs to the same formation with that composing the Pampas. This deposit, extending over so wide an area on both sides of the Plata, abounds with very numerous remains of various extinct mammalia; among which the *Toxodon*, *Megatherium*, *Mastodon*, an animal covered with an armadillo-like case, and as Mr. Darwin believes, the horse, co-existed in the same district.

Proofs of the elevation of the land within a recent period, occur in several parts. Mr. Darwin stated that he had seen in the possession of Sir W. Parish, marine shells which occur near Buenos Ayres in great beds, elevated several yards above the level of the river; and these same species the author had found living on the mud banks on another part of the coast. He, therefore, inferred, that at no very remote period a great bay occupied the area both of the Pampas and of the lower parts of Banda Oriental; and that into this bay the several rivers, which now unite to form the Plata, poured down reddish sediment, resulting, as at the present day, from the decomposition of the granites of Brazil, and charged with carbonate and sulphate of lime, perhaps derived from the Cordillera. On the cliff-formed shores of Entre Rios, the line can be distinguished where the estuary mud first encroached on the deposits of the ocean. The author also supposed that the ancient rivers, like those of the present day, carried down the carcases of land animals, which thus became entombed in the accumulating sediment. Since that period, by the gradual rising of the land, the bottom of the great bay has been converted into plains, almost as level as the surface of the former sea; and the rivers now hollowing out courses for themselves, have exposed, in many places, the skeletons of those ancient inhabitants of the neighbouring land.

Mr. Darwin then briefly alluded to a small formation of mud and shingle at Bahia Blanca, some hundred miles south of the Plata, in which the remains of several extinct quadrupeds have been discovered. Amongst these he enumerated the *Megatherium Cuvieri*, the remains perhaps of a smaller species of *Megatherium*; a quadruped closely allied to the armadilloes, but nearly as large as a horse; some small rodents, and other animals. These remains are embedded with one species of terrestrial, and several of marine shells, the latter being identical with some existing in the adjoining bay. It is, therefore, certain that the greater number of the above mammalia found at Bahia Blanca lived within a very recent epoch; and from the position of the bed in which they occur, it is equally certain that the form of the land has undergone, since that period, very little change, even of level, with respect to the ocean.

Several hundred miles further southward, Mr. Darwin found the remains of an animal which Mr. Owen says has an affinity with the Llama or Guanaco, but was of a gigantic size: this animal likewise existed since the Atlantic has been peopled by the shells now living.

The author observed in conclusion, that the comparative recentness of the epoch at which the fossil mammalia lived, is shown, first by the shells associated with them; secondly, by the recent tertiary character of the strata underlying the deposit containing those remains; and thirdly, from the little altitude of such beds above the level of the sea; for in this country, according to the author's observations, the movements seem to have been so regular, that the amount of elevation becomes a measure of time.

These facts relating to the former existence of the inhabitants of a part of the globe so remote from Europe, fully confirm the remarkable law, often insisted upon by Mr. Lyell, that "the longevity of the species" among mammalia has been of shorter duration than among molluscs. The author finally remarked, that although several gigantic land animals, which formerly swarmed in South America, have perished, yet that they are now represented by animals, confined to that country; and which, though of diminutive size, possess the peculiar anatomical structure of their great extinct prototypes*.

An extract of a letter, dated Saharunpore, 18th November, 1836, from Captain Cautley to Dr. Royle, was next read; permitting the announcement of a fact which had long been communicated to the latter, of the finding of the remains of a quadrumanous animal in the Sewaliks, or Sub-Himalayan range of mountains. An astragalus was first found, but latterly a nearly perfect head, with one side of the molars and one orbit nearly complete. The animal must have been much larger than any existing monkey, and allied to Cuvier's Cynocephaline group†. Captain C. also announced the discovery by Major Colvin of a specimen of the head of the Sivatherium, in which, in conformity to the conjectures of Dr. Falconer and himself, in their paper, it is found that the animal had four horns, two in front and

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[* The relation between the extinct and living animals confined to America was first noticed, we believe, by Mr. Brayley, in some remarks on a fossil vertebra from Eschscholtz Bay; probably referable to a species of *Megatherium*. Phil. Mag. and Annals, vol. ix. p. 418.]

[† See Lieuts. Baker and Durand's accounts of this fossil in our last number, p. 33.]

two large trifurcated ones behind. He considers the animal to be allied to the Dicranocerine group of Major Hamilton Smith*.

Capt. C. also mentions the discovery of a large bear, as well as of a camel, respecting which he had, in conjunction with Dr. Falconer, published a paper. Mastodon's heads were also making their appearance, perfectly different in form from the proboscidean Pachydermata of the present day. There appeared to be altogether two if not three species, besides the variety of *M. angustidens.*

A paper was then read "On some recent elevations of the Coast of Banffshire; and on a deposit of clay, formerly considered to be lias;" by Joseph Prestwich, jun., F.G.S.

That an uplifting of the shores of the Moray Firth has taken place subsequent to its having assumed its present outline, is proved by the existence, in several places, of a raised beach. In Banffshire this beach varies from six to twelve feet above the present high water level; and occasionally abounds in shells now inhabiting the adjacent seas, as *Patella vulgata, P. levis, Trochus ziziphinus, Littorina littorea,* and *Turbo retusus.* To this upheaving of the land the author attributes the draining of the former lowlands, as he conceives is indicated by the remains of drained peat-mosses. A section of one of these presented a total thickness of about four feet, including two irregular layers of gravel, of quartz grit, with freshwater and land shells.

In a paper on the Gamrie Ichthyolites, read before the Society in April 1835†, Mr. Prestwich stated, that having been informed of the occurrence of lias fossils in the dark clay and sand, which in many parts of Banffshire cap the old red sandstone and schistose rocks, he had inferred that these beds might be outliers of lias. Having however subsequently visited the country, and examined that deposit at Blackpots and Gamrie, he found the lias fossils in separate masses and associated with rolled fragments of the older rocks. He also met with at Gamrie, in a bed of light coloured sand, alternating with dark clay and beds of gravel, the following recent shells, *Astarte Scotica, Tellina tenuis, Buccinum undatum, Natica glaucina, Fusus turricola, Dentalium dentalis,* &c. They were extremely friable, but perfect. This deposit or drift attains, in places, a thickness of 250 feet, and rises to a height of 350 feet.

In conclusion, the author attributes the origin of this drift to a denudation of the lias and older formations; and he infers, from the perfect preservation of the fossils, and the superposition of the beds, that its accumulation was gradual.

A paper was afterwards read, entitled "An account of a Tertiary Deposit near Lixouri, in the island of Cephalonia;" by William John Hamilton, Esq., F.G.S., and Hugh Edwin Strickland, Esq., F.G.S.

The authors state that most of the island of Cephalonia which they had an opportunity of examining, consists of a hard white limestone

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[* Dr. Falconer's and Capt. Cautley's Memoir on the *Sivatherium* will be found in Lond. and Edinb. Phil. Mag.. vol ix. p. 193, *et seq.*]

† Geological Proceedings, Vol. ii. No. 40, or Lond. & Edinb. Phil. Mag., vol. vii. p. 325.

or scaglia, occasionally more or less crystalline, which forms the principal rock of the Ionian islands. It is generally without fossils, but near Argostoli contains numerous organic remains, principally small spiral univalves; and near the middle of this vast formation are two beds of oyster shells, about a foot thick each, and parallel to the stratification which dips to the north-east at an angle of 25°.

The tertiary formation extends for two or three miles to the north and south of Lixouri, forming several parallel lines of hills, sloping to the east according to the dip of the strata, and presenting a succession of steep escarpments toward the west. The beds are all conformable, dipping a few degrees to the north of east by compass, at an angle varying from 45° to 55°. These beds are remarkable for their great thickness, the beauty and number of their fossils; and for the variety of strata through which they extend. The beds, of which sixteen are enumerated, may be classed under three principal heads. First, the calcareo-arenaceous; second, the argillaceous; third, the gypseous beds. Of these, the first is a loose sandy alluvium, rising very gradually from the sea side, and resting unconformably upon the other beds which are conformable. The fossils belong to numerous genera; and some of the species have been identified with those at present existing in the Mediterranean.

May 17.—"A description of the Geological character of the Coast of Normandy;" by S. Peace Pratt, Esq., F.G.S., begun at the meeting on the 3rd of May, was concluded.

The author commences his paper by observing that the fall of a cliff or the opening of a quarry throws light upon an obscure locality and clears up previous difficulties; and that from frequently examining the coast from Point Antifer to near Grenville, he has had opportunities of correcting some of the views of M. De la Beche and M. De Caumont in their descriptions of the same line of coast.

The chalk cliffs from Point Antifer to Cap la Heve are composed of *craie glauconeuse* equivalent in position to chalk marl. These rest on a bed 40 to 50 feet thick of green sand, containing numerous fossils; the lower part, of a dark olive green, is also full of shells and corals. To these succeed two argillaceous beds with an intermediate one of ferruginous sand, each five to seven feet thick, resting upon a fourth 20 to 25 feet thick of sand, in which the plates of mica are numerous and large. These beds are interesting as indicating the presence of the gault, lower green sand, and Hastings sand. The absence of the characteristic fossils may be explained by the beds appearing to have thinned off towards the coast.

The ferruginous deposit rests upon an argillaceous limestone, separated into thin beds by partings of clay, then rising to the surface at a small angle to the north-east of Cap la Heve. The upper layers of the clay contain *Gryphæa virgula*, *Ostræa deltoidea*, &c., while in the marl stone, though there are few fossils, a *Pholadomya*, a *Terebratula*, &c., were occasionally found in the upper parts with Trigonia and Perna, in the lower parts generally in distinct beds. These therefore represent the Kimmeridge clay, though found immediately under the iron sand.

On the south bank of the Seine, though the cliffs appear similar, yet in consequence of a fault the lower argillaceous bed which covers the ferruginous sand has been brought down to the level of the shore; and has led to the error that the argillaceous beds on the two shores were of the same age; and that the *argile d'Honfleur*, like that on the north bank, was identical with the Kimmeridge clay, though it actually overlies the iron sand.

Beyond Honfleur a marshy plain succeeds, composed to the depth of two feet of a marl containing such freshwater shells as Lymnæa, Cyclas, Planorbis, &c. At Cricque-bœuf a bed of clay, similar to that at Havre, rises to the surface, covered by a few inches of the green and ferruginous sands; and as the characteristic shells are found in its upper part there can be no doubt of this also being analogous to the Kimmeridge clay. About a mile to the westward it is seen resting on a calcareous close-grained rock, which from its nature and fossil shells, and abundance of coral, the author considers equivalent to the coral rag; and states that the upper part cannot represent the Portland beds, for the whole distinctly underlies the Kimmeridge clay. Near the mouth of the Toncque a deposit of clay rises from beneath these calcareous strata, alternating with their lower beds and forming bold cliffs from near Villers-sur-mer to Dives. Fossil remains, both animal and vegetable, are abundant; in the upper part *Gryphæa dilatata* and *Ostrea gregarea* in numerous thin beds. The remains of vertebrated animals are not very numerous, but bones of saurians and fishes are occasionally found. Mr. Pratt gives a list of the fossils found in the Argile de Dives, which he assimilates to the Oxford clay.

This clay is again seen at a short distance from the mouth of the Orne overlying a calcareous oolitic group, consisting of numerous fissile beds full of shells and fragments of coral, usually considered identical with the cornbrash; but which Mr. Pratt considers as approaching much nearer to the forest marble of the west of England. They pass into others less oolitic, containing numerous genera of corals and shells, and overlie two beds of marly rubbly stone passing in their lower parts into a clay filled with fossils chiefly of *Terebratula digona* and *plicata*, *Avicula inæquivalvis*, *Apiocrinites rotundus*, &c. Together they form a mass about eight feet thick, but varying at short distances and resting upon a hard crystalline limestone, very little oolitic, composed of broken shells and corals, stems of Encrinites and Pentacrinites, and the corals for which this locality is famous in the fissures. This stone is generally thought to pass into the freestone of Caen; the author thinks it more probable that it overlies it, but thins off towards the S.W. The Caen stone is usually considered to represent the great oolite, but Mr. Pratt remarks that the few fossils found in it, as *Terebratula spinosa*, &c., resemble those of the inferior oolite. In examining the cliffs near St. Honorine, the lias, in consequence of a fault, may be seen for a few hundred yards forming their base. The contact of the lias filled with Belemnites with the inferior oolite is here marked by a highly ferruginous oolitic bed, only a few inches in thickness, but containing almost as many fossils as when of its greatest thickness. These are generally identical with those found at Dundry. Upon this bed rests the sandy calcareous rock, 25 feet thick, which has been described by M. De la Beche as lias. It contains three or four beds full of sponges, Alcoynia, a few shells, and numerous Echini, and is surmounted by an argillaceous deposit varying from light grey to dark blue, and assuming at Port en Bessin a thickness of more than 100 feet, but is not separately distinguished in M. De la Beche's section. From geological position it appears to represent the Fullers' earth. This clay is covered by a calcareous rock slightly oolitic, containing few or no fossils, and forming the summit of the cliffs from St. Coine to Granville.

Mr. Pratt concludes his paper by observing that nearly the whole of the strata found between the chalk and the lias in England are found on the coast of Normandy, though somewhat modified; and that nearly all the characteristic shells are found in each. No bed of any consequence found in Normandy is wanting in the English series; while the Portland and perhaps the Purbeck beds with the Kelloway rock are not seen in this part of France.

Extracts from a letter from Sir John F. W. Herschel to C. Lyell, Esq., dated Fredhausen, Cape of Good Hope, 20th February, 1836, were then read.

The author commences by inquiring, whether it had ever occurred to Mr. Lyell to speculate on the probable effect of the transfer of pressure from one part to another of the earth's surface by the degradation of existing, and the formation of new continents, on the fluid or semifluid matter beneath the outer crust? Supposing the whole to float on a sea of lava, the effect would merely be an almost infinitely minute flexure of the strata; but supposing the layer next below the crust to be partly solid and partly fluid, and composed of a mixture of fixed rock, liquid lava and other masses in various degrees of viscidity and mobility; great inequalities may subsist in the distribution of pressure, and the consequences may be local disruptions of the crust where weakest, and escape to the surface of lava, &c.

Referring to the phænomena of volcanos, Sir J. Herschel observes, that it has always been his greatest difficulty in geology to find a *primum mobile* for the volcano, taken as a general and not as a local phænomenon; and referring to the different theories given on the subject, which he considers insufficient, wanting in explicitness and as not going high enough in the inquiry or up to its true beginning, and also as giving in some respects a wrong notion of the process itself;—inquires, how came the gases which are evolved to be condensed?—why did they submit to be urged into liquefaction?—if they were not originally elastic, but have become so by subterranean heat—whence came the heat, and why did it come?—how came the pressure to be removed, or what caused the crack?

It seems clear that if the gases or aqueous vapour were once free at so high a degree of elasticity as is presumed, there exists no adequate cause for their confinement. We are forced therefore to admit that the elastic force has been superadded to them during their sojourn below by an accession of temperature.

Assuming a high central temperature, which many geologists admit, and with which all are familiar; the author agrees with Mr. Lyell's observations, that the ordinary repose of the surface argues a wonderful inertness in the interior, where in fact he conceives that everything is motionless; debarred therefore from the invasion of a circulating current or casual injection of intensely hot liquid matter from below, he conceives that the phænomena may be explained as follows:

Granting an equilibrium of temperature and pressure within the globe, the isothermal strata near the centre will be spherical; but where they approach the surface they will by degrees conform themselves to the configuration of the solid portion, that is, to the bottom of the sea and the surface of continents. If we suppose therefore a state of equilibrium, and that under the concave bottom of any great ocean the lines of equal temperature be parallel to its concavity; when this comes to be filled up by the deposition of matter brought down by rivers, &c., the formerly concave bottom may become horizontal or even convex, and the equilibrium of temperature will immediately be disturbed; because the form of a stratum of temperature depends essentially on the bounding surface of the solid above it, that form being one of the arbitrary functions which enter into its partial differential equation. The temperature, therefore, will immediately begin to migrate from below upwards, and the isothermal strata will gradually change their forms from the concave to the horizontal or convex form. The former bottom of the ocean will then (after the lapse of ages, and when a fresh state of equilibrium is attained) acquire a temperature corresponding to its then actual depth; while a point as deep below it, as itself is below the surface, will have acquired a much higher temperature, and may become actually melted, and this without any bodily transfer of matter in a liquid state from below. But if the temperature of this supposed deep stratum be already at the melting point, then will this rise to the former bottom of the ocean and the strata become melted, *water included,* with which, from the circumstances of the case, they must be saturated.

If the process of deposition go on, until by accumulation of pressure on the bottom or sloping sides, some support gives way,—a piece of the solid crust breaks down and is plunged into the liquid below, and a crack takes place, extending upwards. Into this the liquid will rise by simple hydrostatic pressure. But as it gains height it is less pressed; and if it attain such a height that the ignited water can become steam, the joint specific gravity of the column is suddenly diminished and up comes a jet of mixed steam and lava; till so much has escaped that the deposited matter takes a fresh bearing, when the evacuation ceases and the crack becomes sealed up.

By taking this view of the process of heating from below, we have a strictly theoretical explanation of the effects of heat on newly deposited strata; and this, simply because the fact of new strata having been deposited, the conditions of the equilibrium of temperature become altered, and they draw the heat to them, or rather retain it in them in its transit outwards; the supply from the centre being supposed inexhaustible, and its temperature of course invariable.

As the greatest transfer of material to the bottom of the ocean is produced on the coast line by the action of the sea, while the quantity carried down by rivers from the surface of continents is comparatively trifling; hence therefore the greatest local accumulation of pressure is in the central area of deep seas, but the greatest local relief takes place along the abraded coast lines: here, therefore, according to this view should occur the chief volcanic vents.

In this view the effects of the removal of matter from above to below the sea, are, 1st. It produces a mechanical subversion of the equilibrium of pressure. 2nd. It also, and by a different process, produces a subversion of the equilibrium of temperature. The last is the most important. It must be an *exceedingly slow process*, and will depend, 1st. On the depth of matter deposited; 2nd. On the quantity of water retained by it under the great pressure; 3rd. On the tenacity of the incumbent mass—whether the influx of caloric from below, which *must take place*, acting on that water, shall either heave up the whole mass as a continent; or shall *crack* it and escape as a submarine volcano; or shall be suppressed until the main weight of the continually accumulating mass breaks its lateral supports at or near the coast lines, and opens there a chain of volcanos.

Thus the circuit is kept up—the *primum mobile* is the degrading power of the sea and rains (both originating in the sun's action) above, and the inexhaustible supply of heat from the enormous resources below, always escaping at the surface, unless when repressed by an addition of fresh clothing at any particular part. In this view of the subject the tendency is outwards. Every continent deposited has a propensity to rise again, and the destructive principle is continually counterbalanced by a reorganizing principle from beneath. Nay, it may go further; there may be such a tendency in the globe to swell into froth at its surface, as may maintain its dimensions in spite of its expense of heat, and thus preserve the uniformity of its rotation on its axis*.

An Extract of a letter from Sir John F. W. Herschel to R. I. Murchison, Esq., in explanation of the former, to C. Lyell, Esq., dated Fredhausen, 15th November, 1836, was afterwards read.

In this letter the author recapitulates the views given in the foregoing abstract, stating that his views are not so much a theory as a pursuing into its consequences, according to admitted laws, of the

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[* On the subject of the views here enunciated by Sir John Herschel, see a notice of a paper by Mr. Babbage, Lond. and Edinb. Phil. Mag. vol. v, p. 213. Mr. Babbage has since published the substance of his paper in his work entitled "The Ninth Bridgewater Treatise," together with the extracts from Sir J. Herschel's letters, abstracted above. He observes, in reference to the similarity of Sir J. Herschel's views to those he had himself previously started, "I feel, that the almost perfect coincidence of his views with my own, gives additional support to the explanations I have offered; whilst the reader will perceive, from the different light in which my friend has viewed the subject, that we were both independently led to the same inferences by different courses of inquiry."—Edit.]

hypothesis of a high central temperature; and his object to get a geological *primum mobile* in the nature of a *vera causa,* and to trace its workings in a distinct and intelligible manner, so that in future, instead of saying as heretofore, "*Let heat* from below *invade* newly deposited strata, then they will expand, melt," &c., we shall commence a step higher, and say, "*Let strata be deposited,* then, as a necessary consequence, and according to known regular and calculable laws, heat will gradually invade them from below and around; and according to its due degree of intensity at any assigned time will expand or melt them as the case may be," &c. The phænomena of earthquakes, volcanic explosions, &c., may arise, but if all goes on in quiet, the only consequences will be the obliteration of organic remains and lines of stratification, and the formation of new combinations of a chemical nature, &c.; in a word, the production of *metamorphic* or stratified primary rocks.

In the formation of these therefore there is nothing casual; all strata once buried deep enough, and due time allowed, must assume that state. None can escape; all records of former worlds must ultimately perish.

"An account of a Well at Beaumont Green in the County of Hereford, fifteen miles from London, about a mile to the west of the road to Ware;" by J. Mitchell, LL.D., F.G.S., was last read.

This well was dug in 1833, in the premises of Mr. Munt, a magistrate of the county, and the information respecting it obtained from two gentlemen accustomed to collect evidence with the strictest scrutiny.

The strata passed through, were one foot vegetable mould, 15 feet gravel, one foot sand with flints, 83 feet gravel clay, 15 feet blue sand with black pebbles, 10 feet blue clay, 1½ feet fine soft white sand, or 126½ feet down to the chalk, which was penetrated for 40 feet when a spring was met with; but the digging continued 17 feet lower to form a reservoir of water, and this was favoured by making the excavation in the chalk of a bell shape, but above this the well was 4½ feet in diameter.

When the well was dug the weather was dry, but on this becoming very wet the 15 feet stratum of blue sand and black pebbles began to emit foul air, by which one of the well-diggers was suffocated in descending. A hawk flying over the well fell into it, and a similar fate befell smaller birds, also wasps, bees and flies. On closing up the mouth of the well, with the exception of an orifice an inch in diameter, so powerful was the force of the issuing current of foul air that it raised a weight of twelve ounces of lead. In fine weather there was on the contrary a strong draft down into the well.

Eight months afterwards the well was again entered when the stratum of blue sand and black pebbles appeared forming into plum-pudding stone. The well was rendered safe by bricking it down to the chalk, applying a thick coating of compost over the whole. Dr. Mitchell explains the phænomena, by observing that the foul air was no doubt sulphuretted hydrogen, produced by the decomposition of water and iron pyrites. After long-continued rain, water penetrating into the bed dislodged the gas accumulated in the interstices where it was formed; while, after dry weather had continued for some time, the openings produced in this bed on drying up would draw for a short time a supply of air to fill up the vacuities, and hence the draft observed to pass down into the well. The whole of the neighbouring district, to the extent of four miles, is called by the well-diggers, foul country. Similar phænomena were observed in digging a well on the opposite hill at Applebury, and also in forming some wells in the immediate vicinity of London.

XXIV. *Intelligence and Miscellaneous Articles.*

ON THE ACTION OF IODINE UPON THE VEGETABLE ALKALIS.

BY M. PELLETIER.

[Continued from vol. x. p. 503 and concluded.]

Iodine and Brucia.—Iodine when put into contact with brucia renders it of a brownish yellow colour; when heated with water the mixture at first yields iodine, the compound softens like a resinous body, but without perfectly melting; on cooling it becomes brittle. The solution when filtered and evaporated leaves a brown substance with some traces of crystals.

The mass is totally soluble in hot alcohol; on cooling a very light brown powder separates; the solution yields by slow evaporation a further quantity of it, and towards the end of it, crystals of hydriodate of brucia in transparent quadrilateral prisms, are obtained.

The formation of the hydriodate of brucia appears to be derived from the reaction of the iodine upon alcohol, which is well known to produce hydriodic acid. A brown matter is also formed when water is employed, but in this case there are mere traces of hydriodate.

The brown matter is iodide of brucia; with chemical agents its appearances are similar to those of iodide of strychnia, except such as depend upon the difference of the bases: heated with the diluted mineral acids it yields salts of brucia, and with concentrated nitric acid it gives the fine red colour which characterizes brucia.

It has already been shown that the iodide of strychnia is a neutral compound, but that of brucia is a bi-iodide composed of

Two equivalents of iodine	252	or	47·72
One equivalent of brucia..........	276	,,	52·28
	528		100·

A neutral iodide of brucia was obtained by mixing and agitating cold tinctures of iodine and the alkali, not adding enough of the former to constitute a bi-iodide. An orange-coloured precipitate is obtained, which is a neutral iodide, composed of nearly

One equivalent of iodine	126	or	31·34
One equivalent of brucia	276	,,	68·66
	402		100·

Iodate of Brucia.—Iodic acid combines directly with brucia; if the acid be in excess a red colour appears, but otherwise it is colourless. By evaporating the solution two salts are obtained: one, which is opaque and silky, with excess of base, and which restores the colour of litmus reddened by an acid; the other is transparent, hard, and in four-sided prisms, this reddens litmus paper. The subsalt has such a disposition to form that it may frequently be obtained in crystals, even from a solution which is slightly acid.

Hydriodate of Brucia.—This salt may be obtained by directly treating brucia with hydriodic acid; it has a different appearance from hydriodate of strychnia, the crystals are transparent square laminæ or short four-sided prisms. Though slightly soluble in cold water, it is more so than hydriodate of strychnia; hot water dissolves more than cold, and crystals are obtained on cooling. It is more soluble in alcohol than in water. Hydriodate of brucia may be prepared by double decomposition by adding hydriodate of potash to sulphate of brucia. It is a subsesquihydriodate, composed of

One equivalent of hydriodic acid	127	or	23·47
One and a half equivalent of brucia ..	414	,,	76·53
	541		100·

The phænomena which have been noticed when treating of hydriodate of strychnia with respect to the action of iodic acid or of an acid poured into a mixture of iodate and hydriodate, also take place when hydriodate of brucia is similarly treated.

Iodine and Cinchonia.—To obtain iodide of cinchonia the alkali must be triturated with about half its weight of iodine and treated with alcohol; the whole dissolves, and by spontaneous evaporation iodide of cinchonia is obtained in plates of a saffron colour; the iodide separates before all the spirit evaporates; towards the end of the operation, crystals of hydriodate of cinchonia are deposited in the form of mushrooms. On treating the whole with boiling water the hydriodate dissolves and fused iodide remains.

In mass the iodide of cinchonia is of a very deep saffron colour; in powder its colour is lighter, its taste is slightly bitter. When heated to 77° Fahr. it softens, but does not fuse below 176°. It is insoluble in cold water, and very slightly dissolved by it when boiling. It dissolves in alcohol and in æther. It is decomposed by the alternate action of acid and alkaline solutions.

It is a di-iodide, composed of ,

One equivalent of iodine	126	or	29
Two equivalents of cinchonia	308	,,	71
	434		100

Iodate of Cinchonia.—This salt has been described and analysed by Serullas. M. Pelletier observes that he has only to add to his account of it that it is very soluble in water, which is remarkable in a salt that is insoluble in alcohol.

It is a di-iodate composed of

One equivalent of iodic acid	166	or	35
Two equivalents of cinchonia	308	„	65
	474		100

Hydriodic Acid combines very readily with cinchonia. It crystallizes in transparent and slender needles of a pearly lustre. It is slightly soluble in cold water, more soluble in hot, and on cooling it crystallizes. Its taste is at first but slight; but it is developed in a short time and is bitter and metallic. The hydriodate and iodate of cinchonia may remain some time together without decomposing; but eventually, and especially when the solutions are concentrated, iodide is deposited; when acid is present the decomposition occurs rapidly.

Iodine and Quina.—Iodine acts upon quina similarly to cinchonia; it is difficult to distinguish the iodides from each other on account of their great resemblance as to appearance, colour, taste, and fusibility.

It is probably composed of

One equivalent of iodine	126	or	27·75
Two equivalents of quina	328	„	72·25
	454		100·

Iodate of Quina.—This salt is less soluble than the iodate of cinchonia. M. Pelletier did not make a direct analysis of it, but concludes from the constitution of the iodide that it is composed of

One equivalent of iodic acid........	166	or	33·6
Two equivalents of quina..........	328	„	66·4
	494		100·

Hydriodate of Quina may be obtained either by direct action or double decomposition. Its crystals are more slender but less transparent and soluble than those of hydriodate of cinchonia; they have a tendency to the mammillated form.

Iodine and Morphia.—Iodine acts in a much more complicated manner upon this than the other alkalis. If morphia be triturated with one fourth of its weight of dry iodine, the matter becomes of a reddish brown colour, without yielding any smell of iodine; but after some hours have expired, its colour changes to a violet brown and even to a black, and a smell of iodine is perceptible. It seems as if the iodine separates after being sometime combined with the morphia.

Morphia, triturated with half its weight of iodine, exhibited similar phænomena, but with greater rapidity. The product was insoluble in cold water but plentifully dissolved by it when boiling. The solution was acid when half a part of iodine was employed, but neutral with a smaller proportion; but much hydriodate of morphia was held in solution. This formation of hydriodic acid is remarkable, and does not occur with the other vegetable alkalis.

A mixture of equal parts of iodine and morphia totally dissolved in boiling alcohol. The solution was acid, and yielded by spontaneous evaporation a red brown substance as the alcohol was dissipated. There remained, at length, an aqueous, slightly coloured liquor, which when poured off and set to evaporate in another vessel, yielded crystals of hydriodate of morphia deeply coloured with iodine. Various means were tried to determine the nature of the red brown substance, but it never yielded morphia when a sufficient quantity of iodine had been employed. When treated with sulphuretted hydrogen in order to convert the iodine into hydriodic acid, and obtain an hydriodate from which the morphia or the substance which replaced it might be separated, ammonia, added to the clear and colourless solution, rendered it immediately of a dirty red colour. The precipitate formed was exceedingly light, not at all corresponding to the quantity of morphia employed, and contained no trace of it; but the solution contained an organic matter.

If instead of treating the solution with an alkali, it be concentrated with heat and the sulphuretted hydrogen expelled, a brown substance, similar to that subjected to the action of the sulphuretted hydrogen, is reproduced. It appears therefore that by the combined agency of air, heat, and the organic matter, the hydriodic acid returns to the state of iodine in order to combine with this matter (the altered morphia) and to precipitate again with it.

Hydriodate of morphia may be prepared by direct action, and when chlorine is passed into a solution of it, not in excess, a very light and bulky precipitate of a reddish colour is immediately obtained; in a few seconds, however, the precipitate becomes black, diminishes considerably in volume, and iodine separates in abundance, as proved by its colour and smell. The filtered solution is colourless; but by evaporation it becomes yellow, and hydrochloric acid is disengaged. If ammonia be added to it, an extremely light precipitate is obtained which contains no trace of morphia. The organic matter remains in the liquor, which becomes of a red brown by exposure to the air. The action of iodine upon morphia is not prevented by an acid; thus when sulphate of morphia is treated with iodine, sulphuric acid is set free, and the reddish brown matter already described is formed.

After various attempts no definite or permanent compound of iodine and morphia was obtained, on account of the separation of the hydrogen of the alkali.

Iodic Acid and Morphia.—It has been shown by Serullas that iodic acid, which combines with other organic salifiable bases, acts upon the elements of morphia and its salts; that at the moment of contact the mixture became yellowish brown, that iodine was set free, as evinced by its odour and action upon starch; two substances are formed: one of a rose colour and soluble in water, and the other yellowish brown and but slightly soluble. Serullas does not state the nature of the former, but the latter he considers as a compound of altered morphia with iodine and iodic acid. The facts stated by Serullas are important; the action of iodine upon morphia supplies an excellent method of ascertaining the presence of the smallest trace of morphia, and *vice versâ*, of detecting iodine in the state of iodic acid.

M. Pelletier adds, that when iodic acid is put in contact with morphia, the first effect is that of reducing the iodic acid. The oxygen of the acid acts upon the elements of the morphia, probably upon its hydrogen; hence the formation of the rose-coloured substance, as when morphia is treated with concentrated nitric acid. As soon, however, as the iodine is set free, if any uncombined morphia remains, the iodine combines with it, to form the orange brown matter already described. If this brown matter, obtained as above, or by the direct action of iodine upon morphia, be again treated with iodic acid, it is acted upon and the rose-coloured compound is again formed. M. Pelletier remarks that the red substance derived from the oxygenating action of iodic acid upon morphia merits a minute comparison with the red matter obtained by the analogous action of nitric acid upon it.

Hydriodic acid combines directly with morphia and forms a white silky salt, which is more soluble than the hydriodates of the other vegetable alkalis. In the hydriodic acid, the iodine being saturated with hydrogen, cannot act upon that of the morphia. But if the hydrogen of the hydriodate be separated, the reaction of the iodine on the morphia then commences and continues.

Iodine and Codeia may be combined by direct action. The iodide is brown and but slightly soluble in water. When codeia is treated with an alcoholic solution of iodine, the iodide is also formed, but the hydriodate is obtained with it. Of all the vegetable alkalis codeia gives the greatest quantity of hydriodate when treated with an alcoholic solution of iodine; probably on account of the greater solubility of the codeia.

Hydriodate of morphia exists in the mother waters, and it is coloured by the iodide. It may be obtained very white by directly combining the codeia with the hydriodic acid. In external characters it greatly resembles hydriodate of morphia, but differs from it essentially in not having its base separated by ammonia. Iodic acid combines directly with codeia. The crystals contained excess of acid; they are very fine flattened needles and fan-shaped.

Although this salt was prepared with codeia, which had been redissolved in æther, the crystals were slightly coloured by a brown yellowish matter; this is, however, separated by crystallizing the iodate repeatedly. The colour appears to be owing to a little morphia retained by the codeia. It is probable that the differences existing in the ultimate analyses which have been performed are derived from the presence of a little morphia. M. Pelletier is decidedly of opinion it is neither a compound containing morphia nor derived from it, as has lately been endeavoured to be proved.—*Ann. de Chim. et de Phys.* vol. lxiii, p. 164.

ON HYDROBROMATE OF CARBOHYDROGEN (MÉTHYLÈNE).

The hydrobromate of carbohydrogen discovered by M. Bonnet is a colourless and extremely volatile liquid of an agreeable and penetrating odour. Although water causes a precipitate in a solution of it in pyroxylic spirit, yet a considerable quantity is retained in solution; it is also soluble in alcohol and æther, from which it is precipitated by water. It is decomposed by heat. The constitution of this compound is represented by the formula $Br^2\ H^2,\ C^4\ H^4$.—*L'Institut, Feb.* 1837.

ON THE PREPARATION OF SULPHURET OF CARBON.

M. Mulder directs, in an iron bottle in which mercury is imported, that besides the hole which is already there, another should be bored near it. Into the first of these openings a copper tube bent twice at right angles is to be screwed, and into the second a straight tube, also of copper, is to be introduced. Then the bottle is to be filled with pieces of charcoal, recently heated to redness, of such a size that they can easily pass down the tube. After having firmly screwed in the straight and curved tubes, place the bottle in a furnace and heat it, after having closed the opening of the furnace with a stone cut in halves to prevent inconvenience to the operator from the ascending heat.

Adapt to the curved tube a Woulff's bottle half filled with water and surrounded with a freezing mixture, and when the iron bottle is sufficiently heated, introduce by the straight tube fragments of sulphur and immediately close the mouth of the tube with a plug; the sulphur fuses, and falling upon, penetrates the pieces of charcoal, and when the sulphur is gradually added, but little gas is evolved and abundance of sulphuret of carbon obtained.—*Journal de Pharmacie,* Jan. 1837.

SOLUBILITY OF OXIDE OF LEAD IN WATER.

M. De Bonsdorff states that oxide of lead is entirely soluble in water when it is formed by the action of damp air, or by the decomposition of nitrate of lead by the application of heat*.—*L'Institut,* May 1837.

ANHYDROUS CAMPHORIC ACID, CAMPHOVINIC ACID, AND CAMPHORIC ÆTHER.

According to M. Malaguti, crystallized camphoric acid obtained by the prolonged action of nitric acid on camphor, and possessing all the properties of pure camphoric acid, affords by combustion with oxide of copper:

Carbon.................	60·20
Hydrogen	8·00
Oxygen	31·79

which indicates the formula $C^{20}\ H^8\ O^4$.

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* On this subject see Lond. and Edinb. Phil. Mag., vol. v. p. 81.

By boiling camphoric with sulphuric or hydrochloric acid and alcohol, a bitter substance of the consistence of syrup is obtained, which is insoluble in water, but dissolves in alkalis, from which it is precipitated by acids; it is also soluble in alcohol. This substance, after having remained some days in vacuo, afforded the following composition: $C^{48} H^{20} O^{8}$, which gives the formula $(C^{40} H^{14} O^{6} + C^{8} H^{5} O + OH)$, A.

If camphoric acid be represented by the formula $C^{20} H^{7} O^{3} + OH$, the formula A will be equal to two equivalents of camphoric acid, each deprived of an equivalent of water, and replaced by one equivalent of æther and one of water, which is the exact composition of the vinic acids. When this substance, camphovinic acid, is distilled in a glass retort over a lamp, there are produced a butyraceous substance, inflammable carburetted hydrogen gas, and a carbonaceous residue. The butyraceous matter treated with boiling alcohol and the solution gradually cooled, crystals of an extraordinary length are obtained possessing neither taste nor smell. The crystals are perfectly neutral, fusing and volatilizing without undergoing decomposition, and combining, notwithstanding their neutrality, with bases, forming crystallized salts. They possess a great number of characteristics, both physical and chemical, which completely distinguish them from camphoric acid.

The crystals are composed of $C^{20} H^{7} O^{3}$, and this formula is completely confirmed by the analysis of the compounds of this crystalline body with the oxides of copper and silver; so it may be considered as camphoric acid minus an equivalent of water. But these crystals, although not possessing an acid re-action, are nevertheless an acid, forming æther by the combined action of alcohol and a strong [mineral] acid, and producing the same vinic acid which is produced by crystallized camphoric acid; this also is a proof that crystallized camphoric acid, $C^{20} H^{8} O^{4}$, contains an equivalent of water, which it parts with when combining with certain bases. What follows confirms this opinion.

The alcoholic mother liquors from which the anhydrous camphoric acid is precipitated, treated with water, afford a dense oily product, which boiled for a few minutes with a little potash becomes very fluid. This has a singular odour, a disagreeable taste, and volatilizes without decomposition.

The composition of this substance, according to experiments, is $C^{28} H^{12} O^{4}$, corresponding with camphoric æther $C^{20} H^{7} O^{3} + C^{8} H^{5} O$. In whatever way this æther is obtained, whether by crystallized camphoric acid or by anhydrous acid, it appears that in the process of ætherification the common camphoric acid parts with an equivalent of water and becomes anhydrous, consequently the formula is $C^{20} H^{7} O^{3} + OH$, and that when it enters into combination it loses an equivalent of water, as in the following formulæ:

Common camphoric acid = $C^{20} H^{7} O^{3} + H O$.
Anhydrous .... = $C^{20} H^{7} O^{3}$.
Camphovinic acid...... = $C^{40} H^{14} O^{6} + C^{8} H^{5} O + HO$.
Camphoric æther...... = $C^{20} H^{7} O^{3} + C^{8} H^{5} O$.

Note.—M. Liebig, *Ann. de Chimie et de Physique*, tom. xlvii. p. 98, gives the following as the composition of camphoric acid:

Carbon	56·29
Hydrogen	6·89
Oxygen	36·82

which indicates the formula $C^{40} H^{15} O^{10}$.—*Journal de Pharmacie*, Feb. 1837.

GIGANTIC CARP.

In the Proceedings of the Zoological Society for Nov. 8, 1836, inserted in our number for June last, (Lond. and Edinb. Phil. Mag., vol. x. p. 481.) is recorded the exhibition of a *Carp* weighing 22 pounds, which had been taken in a piece of water called the Mere, near Payne's Hill, in Surrey. It is also stated that "Mr. Yarrell observed, that he could find no record of any *Carp* so large having before been taken in this country." On this account the following notice which I have just now accidentally met with may be worth citing.

In the "*Historical Chronicle*" of the Gentleman's Magazine for September, 1771, (vol. xli. p. 424), it is related, under the head of *Monday*, [Sept.] 30, that "A carp, weighing 23 pounds, was lately caught in a pond belonging to Sir John Filmour [Filmer], at East Sutton, in Kent." This *Carp* seems to have been the largest specimen on record; for Mr. Yarrell, in his History of British Fishes, (vol. i. p. 309,) notices a brace weighing 35 pounds, and two single fish weighing 18 and 19½ pounds respectively, as the largest which he could then refer to.

The above, like most of the relations in that department of the Gentleman's Magazine, is probably newspaper intelligence. The circumstance does not appear to be mentioned in either edition of Hasted's History of Kent.

E. W. B.

July 21, 1837.

CURTIS'S ENTOMOLOGY.

We have the pleasure of announcing the appearance of the Second edition of Mr. Curtis's Guide to an Arrangement of British Insects. It is printed on the novel plan of the former edition, contains several hundreds of new species and genera, and has the advantage of an Alphabetical Index of the latter. We need scarcely add that this Guide is indispensable to all Entomologists.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1837.

Chiswick.—June 1. Slight rain: showery: clear and fine. 2. Overcast and fine. 3. Hazy. 4. Fine. 5. Very fine: rain at night. 6, 7. Very fine. 8. Dry haze. 9. Fine: heavy rain. 10. Cloudy: showery. 11. Rain: very fine. 12. Slight rain. 13. Very fine. 14. Showery. 15—17. Very fine. 18. Rain: fine. 19—26. Very fine. 27. Dry haze: fine: clear and cold. 28. Cloudy; fine. 29. Very dry. 30. Fine: clear and cold at night.

Boston.—June 1. Fine. 2, 3. Cloudy. 4. Fine. 5. Fine: rain with thunder and lightning P.M. 6. Cloudy. 7, 8. Fine. 9. Fine: rain P.M. 10. Cloudy: rain P.M. 11, 12. Fine: rain P.M. 13. Fine. 14. Cloudy: rain early A.M. 15. Fine. 16. Fine: rain P.M. 17. Fine. 18. Rain: rain early A.M.: rain P.M. 19. Cloudy. 20. Cloudy: rain P.M. 21—27. Fine. 28. Cloudy. 29, 30. Fine.

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

Days of Month. 1837. June.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Self-registering. Max.	Max.	Min.								
Th. 1.	29·850	30·010	29·913	29·32	53·4	53·7	63·3	62	44	51	WSW.	NW.	N.	·130	·06	...	50
F. 2.	29·956	30·038	29·984	29·54	56·3	48·8	61·3	61	44	48	WSW.	NW.	calm	·041	·14	...	50
● S. 3.	29·895	30·025	29·985	29·55	51·3	49·7	59·2	63	35	53	SW.	NW.	calm	·083	...	...	48
⊙ 4.	30·056	30·121	30·000	29·70	53·4	45·4	60·2	71	49	57·5	E.	NW.	calm	...	...	...	48
M. 5.	30·067	30·152	30·100	29·67	58·7	52·5	65·6	75	49	60	S.	NW.	N.	...	·11	...	52
T. 6.	30·029	30·173	30·103	29·61	60·3	53·4	69·9	69	40	56	SSW.	NW.	NE.	·102	...	·20	55
W. 7.	30·114	30·201	30·083	29·75	54·3	45·2	65·7	60	39	55	E.	NE.	calm	...	...	...	46
Th. 8.	29·860	29·938	29·861	29·59	53·2	43·2	56·0	69	47	52·5	E.	E.	NE.	...	...	...	45
F. 9.	29·647	29·777	29·564	29·40	58·4	50·0	59·2	71	51	56	E.	E.	calm	...	·36	...	54
S. 10.	29·565	29·660	29·631	29·10	61·2	55·9	66·3	68	48	62	SSW.	S.	W.	·094	·09	·39	55
☽ ⊙ 11.	29·702	29·776	29·681	29·20	59·4	56·3	68·2	68	49	64	SW. var.	SW.	W.	·044	·01	·10	56
M. 12.	29·860	29·940	29·859	29·37	61·2	53·3	66·3	70	56	64·5	S.	S.	W.	...	·03	·03	55
T. 13.	29·750	29·830	29·740	29·24	66·6	58·4	67·5	75	59	66	SSW.	S.	W.	...	·20	·07	59
W. 14.	29·706	29·853	29·772	29·16	65·7	61·2	71·5	75	47	64	SSW.	S.	calm	·186	·11	·57	60
Th. 15.	29·906	29·992	29·954	29·42	66·2	56·2	75·0	80	53	69	SW.	S.	calm	...	...	...	59
F. 16.	29·871	30·001	29·857	29·39	62·5	55·5	75·8	80	53	66·5	E.	E.	calm	...	...	...	60
S. 17.	29·868	29·939	29·913	29·29	65·2	56·5	74·0	75	53	65	SSW.	W.	W.	·036	·20	·26	61
○ ⊙ 18.	29·717	29·857	29·766	29·21	60·8	57·5	72·0	71	53	59	S.	NW.	calm	·111	...	02	58
M. 19.	29·864	29·940	29·924	29·33	64·7	56·2	68·2	74	47	63	S.	SW.	calm	·063	...	·10	57
T. 20.	29·841	29·928	29·915	29·33	66·6	55·9	75·4	76	53	64	S. var.	S.	calm	...	...	...	60
W. 21.	29·968	30·173	30·047	29·38	64·9	57·3	72·8	75	47	67	SSW.	W.	calm	...	...	·05	59
Th. 22.	30·217	30·299	30·295	29·67	65·3	56·4	71·8	81	50	69·5	S.	NW.	calm	...	...	...	58
F. 23.	30·267	30·361	30·279	29·72	67·4	58·8	73·2	76	47	73	ENE.	NE.	calm	...	...	...	60
S. 24.	30·136	30·241	30·120	29·64	66·6	54·8	74·3	79	48	70	E.	SE.	calm	...	...	...	60
☾ ⊙ 25.	29·995	30·088	30·075	29·50	70·2	58·3	75·2	79	50	71	N.	E.	calm	...	...	...	62
M. 26.	30·135	30·239	30·215	29·68	63·7	56·6	75·0	70	44	64	ENE.	E.	E.	...	...	...	58
T. 27.	30·150	30·234	30·192	29·73	56·7	50·2	68·4	68	40	62·5	NE.	SE.	E.	...	...	...	51
W. 28.	30·106	30·188	30·154	29·68	58·6	48·3	63·0	71	42	58	ENE.	NE.	calm	...	...	...	52
Th. 29.	30·062	30·154	30·021	29·60	60·4	51·3	67·4	78	46	64	E.	NE.	calm	...	...	...	55
F. 30.	30·055	30·234	30·136	29·57	68·8	58·2	72·3	78	45	65·5	N.	NE.	calm	...	...	...	60
	29·941	30·045	29·911	29·47	61·4	53·8	68·5	72·2	47·6	62·0				Sum 0·890	1·31	1·79	55·4

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[THIRD SERIES.]

SEPTEMBER 1837.

XXV. *Experiments made during a Voyage, and at Bermuda, on the Carbonic Acid in the Atmosphere.* By Lieut.-Col. Emmett. *Communicated by* John Dalton, *D.C.L., &c.*

Notes on Carbonic Acid in the Atmosphere in a Voyage to Bermuda.

1836, April 28.	Lat.	46° 0′	Long.	14° 51′
29.	—	44 7	——	18 21
May 1.	—	42 58	——	23 8
3.	—	42 9	——	27 55
11.	—	39 39	——	37 4
13.	—	39 8	——	38 6
25.	—	30 53	——	61 9

Carbonic acid found in all these trials made. Lime-water was the test. The quantity apparently fluctuated, the film forming at times more rapidly than at other times; most, *apparently*, at the 29th of April and the 1st of May.

Experiments made at Bermuda, per quantity.

Experiment 1. Sept. 25th. A glass receiver of 3920 cubic inches, = 15·5 gallons, was taken to the north side of the island beyond any building. Wind north; day fine; thermometer 79°. Into this, after well washing with rain water and collecting the air, were put 1500 grain measures of lime-water. The receiver was then well closed with a cork, and set aside.

Sept. 24, 4½ p.m.; therm. 82°. Some of the lime-water used was tested: 1500 lime-water taking 410 test sulphuric acid, the liquid would be 1·009 test. It took 330 to saturate the remaining lime-water; consequently left 80 for the carbonic acid in the air*.

Experiment 2. 25th of Sept. Receiver and acid as before, but the lime-waters but 210 for neutralization. Wind strong from S.W.; thermometer 80°.

Sept. 28th. The lime-waters from the receivers took 120 grain measures for neutralization, leaving 90 for carbonic acid gas; a very nearly similar result as before.

Experiment 3. Oct. 2nd, at 5 P.M.; wind S.W. at the cessation of a heavy gale, with much rain; therm. 78°, barom. 30·00.

Receiver and acid as before. Lime-water required 390 measures for neutralization. Tested that in receivers at 5 P.M. of the 8th inst.; therm. 75°. This required 210 measures of the acid for neutralization, leaving 180 for carbonic acid, being double that before, or about 1 in 3920.

Experiment 4. Oct. 11th, 4½ P.M. Collected air as before. There had been much rain during the day, but it was fine and calm after 3 P.M. Therm. 77°½. In this case the lime-water took 375 measures for neutralization.

Tested that in the receivers on the 18th; therm. 75°. The 1500 grains in the receiver required 280 grains for neutralization, leaving 95 for carbonic acid gas.

In the experiments 1, 2, and 4, the gas is consequently about 1 in 8000; and in the 3rd, 2 parts in 8000. In the 3rd, the receiver was out during the rain, but so placed as to prevent its entrance. The air in 2 and 4 had traversed the small island of St. Davids, distant perhaps 1¼ of a mile, thinly in habited, and thence the inlet of the sea, St. George's Harbour.

The acid was pure, brought out with me for particular experiments.

Looking to the general result, and in No. 3 the quantity being *double*, inaccuracy of observation of the measures might possibly have led to the differences.

[Probably the whole terraqueous globe is enveloped with the same atmosphere, as is the case in azotic and oxygenous gases. The reason why the carbonic acid is not so obvious, is its extreme minuteness. The whole quantity is not more than $\frac{1}{1000}$ th part of the mass, or $\frac{1}{1500}$ part of the volume of the atmosphere. I have examined the air in a hothouse in July, with the air pent up during the night, and open in the

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* The receiver was not long enough exposed: my bottle was two gallons; it was exposed three or four days, and agitated to exhaust the air. Consequently ten times as much would probably be required by ten times the size of the bottle.

usual manner during the day, and the whole quantity is the same as the carbonic acid in the atmosphere, neither more nor less.

August 9, 1837. J. D.]

XXVI. *On the Influence of the Rotation of the Earth on the Currents of its Atmosphere; being Outlines of a general Theory of the Winds. By Prof.* H. W. Dove *of Berlin.*[*]

[With a Plate.]

NOT one of the philosophers who have attempted to give a theory of the winds, has gone beyond the discussion of the regular phænomena between the tropics; for which, indeed, they cannot be blamed, as it is right in a very complicated investigation to examine the most simple case first. But, on the other hand, it must seem strange that since 1685, (in which year Halley published his theory of the trade-winds, consequently for 150 years), not a step has been made towards a general solution of the question. The purpose of this treatise is to show, that the phænomena of the trade-winds, those of the monsoons, and the complicated relations of the winds of the temperate and frigid zones are necessary and simple consequences of the same fundamental physical causes.

The velocity of rotation of the single points of the surface of the earth is in proportion to the semidiameter of the parallel circles under which they lie; it therefore increases from the poles, where it is zero, to the equator, where it is greatest. In the state of rest the air partakes of the velocity of rotation of the place over which it is. If, therefore, from the difference of temperature, or from any other cause, it receives an impulse to flow in a parallel circle, the rotation of the earth will not have any influence upon it, because the points of the surface at which the current of air arrives, have exactly the same velocity of rotation as the points whence it has proceeded. But if air is by any cause propelled from the pole towards the equator, then it comes from places whose celerity of rotation is small, to places at which it is greater. The air, consequently, then turns with less celerity to the east than the places with which it comes in contact; it therefore seems to flow in an opposite direction, viz. from east to west. The deviation of the wind from its original direction will be the greater, the more the velocity of rotation of the point of starting, by an equal progressive motion, differs from the velocity of rotation of the place at which the wind is observed, viz. the greater the difference of geographical latitude of both places. Hence follows:

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[*] From Poggendorff's *Annalen*, vol. xxxvi. p. 321.

1. In the northern hemisphere, winds which begin as north winds, in gradually advancing pass through NE., and become more and more easterly.

Supposing places,

$$\begin{array}{llll} A & A_{\prime} & A_{\prime\prime} & A_{\prime\prime\prime} \\ B & B_{\prime} & B_{\prime\prime} & B_{\prime\prime\prime} \\ C & C_{\prime} & C_{\prime\prime} & C_{\prime\prime\prime} \\ D & D_{\prime} & D_{\prime\prime} & D_{\prime\prime\prime} \end{array}$$

so situated, that of A, B, C, D, being under the same meridian, the place A is the most northern and D the most southern; of $A\ A_{\prime}\ A_{\prime\prime}\ A_{\prime\prime\prime}$ situated under the same parallel, A is the most western, $A_{\prime\prime\prime}$ the most eastern; and that the whole bulk of air contained between $A\,A_{\prime\prime\prime}$ and $D\,D_{\prime\prime\prime}$ from any cause is put in motion from north to south; then, if the air which had proceeded from $C\,C_{\prime\prime}$ arrives nearly as a north wind in the parallel $D\,D_{\prime\prime\prime}$, that coming from $B\,B_{\prime\prime\prime}$ will arrive quite as a north-east wind, while that arriving from $A\,A_{\prime\prime\prime}$ will appear still more as an easterly wind. *To an observer who is in* $D\,D_{\prime\prime\prime}$ *the vane will thus have gradually turned from north through north-east to east.*

2. In the southern hemisphere, winds that begin as south winds, in gradually advancing pass through south-east and become more and more easterly.

If, therefore,

$$\begin{array}{llll} d & d_{\prime} & d_{\prime\prime} & d_{\prime\prime\prime} \\ c & c_{\prime} & c_{\prime\prime} & c_{\prime\prime\prime} \\ b & b_{\prime} & b_{\prime\prime} & b_{\prime\prime\prime} \\ a & a_{\prime} & a_{\prime\prime} & a_{\prime\prime\prime} \end{array}$$

designate places of which those being under the parallel $a\,a_{\prime\prime\prime}$ are the most southerly, and those in the parallel $d\,d_{\prime\prime\prime}$ are the most northerly, *an observer being in* $d\,d_{\prime\prime\prime}$ *will see the vane turn gradually from* S. *through* SE. *to* E.

If in this manner an easterly wind shall have arisen in the northern or southern hemisphere, it will pass through the parallels $D\,D_{\prime\prime\prime}$ and $d\,d_{\prime\prime\prime}$ without being at all modified by the rotation of the earth.

If the cause which drove the air to the equator continues, the east wind, which is the consequence, will check the current. The current being thus checked, the air will soon acquire the velocity of rotation of the place beneath; it will join it in a state of relative rest. With a continual tendency to stream towards the equator, exactly the same phænomena will be repeated which we have just examined.

Let us now suppose that the equatorial currents appear after the polar currents have prevailed for a while. In the northern hemisphere a rising south wind will take the place of the polar current, grown more or less easterly by shifting from

E. through SE. to S.; in the southern, the equatorial current, appearing as a north wind, will change the polar current, grown more or less easterly from E. through NE. to N.

In the parallel $D\,D_{\prime\prime\prime}$ of the northern hemisphere, the variation hitherto observed will consequently be upon the whole:

N. NE. E. SE. S.;

in the parallel $d\,d_{\prime\prime\prime}$ of the southern hemisphere, on the contrary, exactly the opposite:

S. SE. E. NE. N.

Air which flows from the equator towards the poles comes from places having greater velocity of rotation to places which move more slowly towards east. Hence follows:

3. In the northern hemisphere a southerly wind in gradually advancing passes through SW. and becomes more and more westerly.

4. In the southern hemisphere a northerly wind in gradually advancing passes through NW. and becomes more and more westerly.

If

$$\begin{array}{cccc} D & D_{\prime} & D_{\prime\prime} & D_{\prime\prime\prime} \\ E & E_{\prime} & E_{\prime\prime} & E_{\prime\prime\prime} \\ F & F_{\prime} & F_{\prime\prime} & F_{\prime\prime\prime} \\ G & G_{\prime} & G_{\prime\prime} & G_{\prime\prime\prime} \end{array}$$

designate places of the northern hemisphere, of which those in the parallel $G\,G_{\prime\prime\prime}$ are the most southerly, and if the whole bulk of air contained between $D\,D_{\prime\prime\prime}$ and $G\,G_{\prime\prime\prime}$ is put in motion from south to north, *an observer being in* $D\,D_{\prime\prime\prime}$, *though he will receive the air coming from* $E\,E_{\prime\prime\prime}$ *nearly as south, will feel that which proceeds from* $F\,F_{\prime\prime\prime}$ *more as* SW., *and that from* $G\,G_{\prime\prime\prime}$ *more as west.*

If in the same way,

$$\begin{array}{cccc} g & g_{\prime} & g_{\prime\prime} & g_{\prime\prime\prime} \\ f & f_{\prime} & f_{\prime\prime} & f_{\prime\prime\prime} \\ e & e_{\prime} & e_{\prime\prime} & e_{\prime\prime\prime} \\ d & d_{\prime} & d_{\prime\prime} & d_{\prime\prime\prime} \end{array}$$

designate places of the southern hemisphere, $g\,g_{\prime\prime\prime}$ being the most northerly, and $d\,d_{\prime\prime\prime}$ the most southerly, and if the air between both parallels is put in motion towards the south pole, *an observer being in* $d\,d_{\prime\prime\prime}$, *though he will receive the air from* $e\,e_{\prime\prime\prime}$ *as north, will observe, that from* $f\,f_{\prime\prime\prime}$ *more as* NW., *that from* $g\,g_{\prime\prime\prime}$ *more as* W.

A westerly wind in both hemispheres will have a retarding influence upon new equatorial currents and fix them to relative calm. The tendency towards the pole continuing, the phænomena will always be repeated till new polar currents change the westerly wind in the northern hemisphere through NW. to N., in the southern through SW. to S. Hence the variation will be

for the northern hemisphere,	S. SW. W. NW. N.
for the southern hemisphere, on the contrary	N. NW. W. SW. S.

From the whole of the phænomena observed, the following, consequently, is the result:

A. In the northern hemisphere the wind turns, if polar currents and equatorial currents take place alternately, upon an average in a direction S. W. N. E. S. through the points of the compass, and it springs back between S. and W. and between N. and E. more frequently than between W. and N. and between E. and S.

B. In the southern hemisphere the wind turns, if polar currents and equatorial currents take place alternately, upon an average in a direction S. E. N. W. S. through the points of the compass, and springs back between N. and W. and between S. and E. more frequently than between W. and S. and between E. and N.

Hence follows:

a. Where in the tropical zone only polar currents prevail on the surface, there is no complete rotation, but an unchanged deviation proportional to the distance of the place of observation from the outward limits of the current, which is only modified a little by the variation of that limit in the seasons. *These are the trade-winds.*

b. Where in the tropical zone, by the peculiar distribution of the solid and fluid, a southerly current alternates once a year with a northerly one, there is only one rotation in the whole year. *These are the monsoons.*

c. In the temperate, and probably also in the frigid zones, where equatorial currents continually alternate with polar currents, the wind changes upon an equal average and more frequently in a fixed direction through the points of the compass, but in the northern hemisphere exactly in an opposite direction to that in the southern. *This is the phænomenon which I have denominated the law of rotation.*

It is evident then that the relations of the tropical winds are the most simple case of the law of rotation.

The preceding discussion does not depend on the origin of the motion of the mass of air contained between the observed parallels, whether we suppose it to be contemporaneous in all points of the same meridian, or successive, by suction or propulsion. It is also quite immaterial whether the currents arising in the north or the south are opposed to each other, or whether they are more or less beneath one another, and inclined towards the meridian. For that very reason I consider the names *northerly* and *southerly* currents to be the most conformable to nature, as making their names independent of the variations which the seasons and local causes may produce in their direction.

The trade-winds and monsoons are a phænomenon so manifest, that it is not necessary to prove their existence. It is otherwise with the law of rotation.

When in the year 1827 I endeavoured to prove the existence of this law by the calculation of 14,600 observations of the barometer, as many of the hygrometer, and 21,900 of the thermometer, which could not be added as mean quantities already determined, but were to be computed *one by one*, I did not presume that it would be objected to the result of so laborious an investigation, that of three seamen who had been questioned, one should know nothing of it. The possibility that this could happen, proves more distinctly than the silence of the physical manuals upon it, that philosophers did not acknowledge a law in the variations of the direction of the wind. If, however, we consider the remarkable regularity with which this law is manifested in the variations of the barometer, thermometer, and hygrometer in Paris and London, calculated by me not only upon a yearly average, but even in every single month, as well as its perfect independence of the daily period,—results which were confirmed by the interesting memoir of M. Galle relative to Danzig,—we might indeed expect, from the exactness of earlier observers, that at least the direct perception of that regularity did not escape them. In a review of older and later writings I have also found many proofs of this perception, but which always remained unnoticed for want of a strict proof. This proof, however, could only be given by passing from the computation of the *averages* to the computation of the *mean variations*. But the rule generally acknowledged as just, that we must *commence* from the average in the investigation of atmospheric phænomena, has unfortunately been understood to mean that we must not *go beyond* the average in these investigations.

While I have recourse to direct observations in order to prove the general validity of the law of rotation, I premise, that I myself consider this proof imperfect. The calculation of the observations of the barometer at a single place in North America, and in the interior of Russia, worked out as I have done for Paris and London, would be a more convincing proof than a number of the best authorities. But for years I have in vain endeavoured to procure journals of observations that might be applicable. The same is the case with regard to the southern hemisphere. But the accordance in the description of the phænomenon for nearly two centuries and a half, I believe, speaks for their correctness; nor is it likely that men of different nations and states, as Bacon, Mariotte, Sturm, Forster, Le Gentil, Don Ulloa, Toaldo, Poitevin, Romme, should have copied one another in noticing the same observation, particularly if we consider that in the works of Muschenbroek, Nollet, Sauri, and Saussure nothing is to be found upon it; nay, that Deluc and Cotte, who occasionally quote Mariotte's observation, omit it in the facts for which they vouch.

I. *Southern and Northern Hemisphere.*

Law of rotation in the northern : S. W. N. E. S.
——————————— southern : S. E. N. W. S.

1. I am indebted to the kindness of Captain Wendt, who sailed round the world several times as commander of the Prussian ship Princess Louise, in answer to an inquiry addressed to him, for the following notice:

"The wind in the southern hemisphere usually turns from north through west to south and south-east. Its direction consequently is contrary to that of the wind in the northern hemisphere. To the best of my knowledge the fact is nearly as follows: Near the Cape of Good Hope in summer, the wind is chiefly SE. but if the wind turns northerly, it is then more violent. When the best summer months are at an end, after a calm of short duration the wind usually blows very moderately from SE., with an unusually clear sky. The wind is continually increasing, whenever it turns easterly; and if it has turned to the north, clouds and lightning are sure to appear on the western horizon, and in less than half an hour a storm from WNW. will ensue, and will not cease until, after 24 or 48 hours, it has veered more to the south."

"Near Cape Horn, both to the E. and W., with a north wind there is generally good weather; when it veers to the NW. it soon blows hard; with a WNW. to SW. it usually blows a storm (which is also frequently the case from WNW. and NW.). The wind subsides as it becomes southerly. SSE. fine weather, frequently succeeded by a calm."

2. *Æthiopic Sea.*—(Le Gentil)*. "On the 25th and 26th we experienced a kind of gust (*coup de vent*) from north to south-west by west, and I remarked a fact which you have had oppor-

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* *Voyage dans les Mers de l'Inde,* ii. p. 701; *Lettre à M. de la Nux.*

tunity of observing more frequently than myself, that the winds do not follow the same rule in this hemisphere as in the northern hemisphere; in this they make the circuit of the compass from north to north-east, to east, to south-east, to south, &c.: in the southern hemisphere, on the contrary, they move in an opposite direction; the hurricanes, the tempests and the gusts appear to me to be subject to this same law in both hemispheres. Physicists have hitherto given no explanation of this phænomenon."

3. *Pacific Ocean.*—(Don Ulloa)*. "The wind in the South Pacific Ocean is never fixed in the NE., nor does it ever change from thence to the E.; its constant variation being to the W. or SW., contrary to what is seen in the northern hemisphere. In both the change of the wind usually corresponds with the course of the sun; hence, as with us, it changes from E. to S., and thence to W.: there it is from E. to N. and thence to W."

4. *South Sea.*—(Forster)†. "Between 40° and 60° south latitude, in the year 1773, we quite unexpectedly met with easterly winds, which were very contrary to our course at the time. It was also remarkable that every time the wind changed, which was the case four times between June 5th and July 5th, it gradually moved round half the compass in a direction contrary to the course of the sun." I believe I may understand Forster to have borrowed this expression in the way usual among navigators from the course of the sun in the northern hemisphere.

It would be very desirable to find remarks on this subject with regard to the southern hemisphere in the works of Basil Hall. I have looked for them in vain.

To these authorities I add, that all the descriptions I am acquainted with of storms in the southern hemisphere, give a rotation corresponding to the above.

II. *Northern Hemisphere.*

1. *England*, 1600. (Bacon's Draught for the Particular History of the Wind. Sect. xii. Philosophical Works, by Shaw. Lond. 1733. 4to. vol. iii. p. 476.)

"When the wind changes conformably to the motion of the sun; that is, from east to south; from south to west; from west to north; and from north to east; it seldom goes back; or if it does 'tis only for a short time: but if it moves in a contrary direction; viz. from east to north; from north to west; from west to south; and from south to east; it generally

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* Voyage to South America, vol. i. p. 8. ch. 3.

† *Bemerkungen*, S. 111.

returns to the former point, at least before it has gone through the whole circle.

"If the south wind begins to blow for two or three days, the north wind will sometimes blow suddenly after it: but if the north wind blows for the same number of days; the south wind will not rise till after the east has blown a while."

2. *France*, about 1700. (Mariotte on the Nature of Air, p. 160.)

"When the north and north-east winds cease, the east generally succeeds, and then follow the south and south-west winds. The south and south-west winds generally succeed the east in the temperate zones, and especially in France. In France the winds pass from east to south and to south-west, then to the west, to the north and north-east, and very seldom make an entire circuit in a contrary direction."

3. *Germany*, 1722. (Sturm, *Physica electiva sive hypothetica*, tom. ii. p. 1206.)

"Non vagatur tamen sine omni regula irregularis etiam haec flatuum aereorum variabilitas. Ex multis enim retro annis, et his ipsis, quibus haec scribimus, diebus, noviter observavimus, esse quandam illorum periodicam circulationem, ita ut occidentalem excipiat ut plurimum ac ordinarie septentrionalis, hunc sequatur gradatim orientalis, deinceps auster in occidentalem iterum paulatim determinetur; non neglectis equidem plagis intermediis, et raro admodum in contrarium verso hoc ordine, vix unquam saltem (si forte ab occidente in meridiem flectatur) ultra orientis terminos excurrente, tantum abest, ut plenum retrogradationis circulum facile absolvat; cum alterum illum directionis frequentissime, saepius uno mense pluries, decurrat: adeo ut haec una videatur inde reperta nobis via, qua citra multae artis subsidium, futuras aeris mutationes, in proximos saltem dies, praesciri, et absque frequenti errore praedici queant: id quod multiplici experimento compertum habemus."

4. *Italy*, 1774. (Toaldo on Meteorology applied to Agriculture, p. 62.)

"In fact, if there be no obstacle, the winds go the round of the horizon with the sun."

5. *Southern France*. (Poitevin on the Climate of Montpellier, p. 65.)

"When the winds have blown from the south and south-east with violence and brought rain with them, they run through the south-west and west points of the compass, and terminate with north-west, which brings back fine weather.

"The north and north-east winds often pass over the east and are succeeded by sea-winds (SSE.). It is very seldom that the north winds veer directly to north-west; however this sometimes takes place; in general they traverse the horizon passing by the east."

6. *Northern Temperate Zone of the Atlantic Ocean.* (Romme's Tables of Winds, Tides, and Currents, vol. i. p. 56.)

"According to an English captain of an East Indiaman, the dominant winds from the parallel of 30° N. to the frigid zone are in this sea from the west or WSW. He remarked that a great north or north-west wind which ends with a calm is followed by a south wind, which brings rain, and which acquiring much force ranges to the west and NW. or N. If these latter winds become violent they turn sometimes to NE. and blow for several days together, or end in a calm to be followed by a south wind. If this latter is rather westerly it is accompanied with rainy weather and squalls, and often comes back to the south with rain."

7. *Freiberg in Saxony,* 1806. (Lampadius's Systematical Manual of Atmospherology, page 189.)

"How exceedingly changeable are the winds in Germany! yet I have remarked a kind of periodical movement in them, which is as follows. I suppose the wind to blow from the south, with a clear sky. The barometer falls, the weather becomes thick, rain follows. In the mean time the wind becomes westerly. The rain still continues, and the barometer rises. The wind changes to the NW. Partial rain ensues. It grows colder. The barometer still rises, and the wind becomes N. and NE. The barometer is now at the highest point, the sky is serene, and the severest cold possible in the season prevails. The wind veers to the E. and the barometer falls a little, but the weather as yet remains clear. The wind veers to the SE., the barometer still falls, the warmth increases again. The wind then changes to the S., and the warmth reaches the highest degree agreeable to the season; the barometer falls, and we come back again to the first point. There are annually several such periods in every season. Sometimes it will take several weeks to complete this rotation of the wind through the whole compass, sometimes but a few days. The wind will very rarely change in a direction contrary to that above mentioned. In general all changes from the left to the right of the horizon are more frequent with us, and a southerly wind is the least frequent. There certainly exists a primary cause of this, which is, however, concealed by many casualties."

Lampadius has however gone beyond this excellent description of the phænomenon. As Sturm had done before him, he has founded meteoromantic rules upon the supposed correctness of this law, and in his "Contributions to Atmospherology" has examined to what degree they may be depended upon.

8. *East Prussia*, 1826. The following is the result of my own observations made at Koenigsberg. (Poggendorff's *Annal.*, vol. xi.)

I have observed rotations in the direction of S.W. N. E. S. at all seasons, but they appear most frequent during the winter. If a SW. wind blows with increasing violence, and finally prevails, it raises the temperature above the freezing point, therefore it cannot snow, but it rains, while the barometer arrives at its lowest state. The wind now turns towards the W., and the thick flakes of snow prove the beginning of a colder wind, as well as the quickly rising barometer, the vane, and the thermometer. If the wind is northerly the sky grows serene, and if NE. the maximum of cold and of the barometer takes place. This however gradually begins to fall, and fine cirri show by the direction of the fibres at their origin, the southern wind which had set in, and which the barometer already indicates, even while the vane feels nothing of it and still points steadily to the east. The southerly wind however with gradually increasing force drives the east wind in a downward direction; simultaneously with a decided falling of the quicksilver the vane indicates SE., the heavens become gradually more and more covered, and with increasing heat the snow is converted by SE. and S. by SW. again into rain. There is now a recommencement, and the driving downwards on the eastern side is in a highly characteristical manner separated from that on the west side by a clearing up for a short time. Once acquainted with the phænomenon, when it appeared in its most evident form it was easy for me again to recognise it in its more irregular changes; nay, to deduce these, and also a frequent springing backward, particularly on the western side. Hence, therefore, I ascertained that, in this country at least, all winds are great whirlwinds (I have seen rotations of from one to twenty two days); that the rotation within side this whirl moves on an average always in the same direction.

9. *Germany*, *Gunzenhausen*. Although the rotation has not been definitely described here, it will, however, easily be recognised in the following extracts. Luz[*] says the N. and NW. winds raise the barometer, and one might almost say, invariably. The E. and NE. also frequently do this; not, however, with so much certainty. At the same time there is a clear sky. With W. wind the barometer also rises, but the sky is then covered with lofty scattered clouds, which, however, seldom rain. With SE. wind the barometer falls, and the weather, notwithstanding, remains settled so long as

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[*] *Beschreibung von Barometern*, 1784, p. 351.

the wind does not veer towards the south. With regard to S. and SW. winds no such definite rules can be given. In general, the barometer falls when the wind comes from this quarter. If, however, it has continued for some time in this direction, and especially if it has rained for some time, the barometer again rises, although the wind continues to blow from S. and SW. I also found the barometer to fall with a N. and E. wind, if the wind came for some time from this quarter, and the clear weather would change into dull and rainy.

10. *Holland.* This subject has been examined by Van Swinden* more completely than by Luz. Horsley† was the first to demonstrate in a more definite manner the influence already pointed out by Halley and Mariotte, of the direction of the wind upon the state of the barometer, by calculations made from a table showing the heights of the barometer for every prevalent wind. The attention of Van Swinden being excited by this, he proposed to himself the question, How often does the barometer fall with a certain wind, how often does it rise with the same? The results of his calculation are a necessary consequence of the law of rotation. He finds in the year 1779, that the barometer

		Rose	Fell
With	SW.	74 times.	83·9 times.
—	W.	36 —	16·6 —
—	NW.	83 —	43·5 —
—	N.	12 —	9·3 —
—	NE.	24 —	28· —
—	E.	1 —	8·3 —
—	SE.	18 —	51·8 —
—	S.	10 —	15·5 —

In the three preceding years, he had obtained with regard to W., NW., N., and E., SE., S., results coinciding with the above; but, on the contrary, deviations with regard to NE. and SW. These solstitial points then appear here quite as definite as those of Luz. It does not appear from any expression of Van Swinden that he was acquainted with the law of rotation; and it is on that account that Saussure, in his *Hygrometrie*, asks, " Why do the east winds, although cold and dry, generally cause the barometer to fall in England and

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* *Mémoires sur les Observations Météorologiques faites à Francker en Frise pendant* 1779.

† An abridged state of the weather at London in the year 1774. Phil. Trans. for 1775.

in Holland, according to the observations of Horsley* and Van Swinden, while the west winds commonly cause it to rise? No hypothesis with which I am acquainted gives a satisfactory reason."

11. *Denmark.* During 1100 changes of the direction of the wind observed in Apenrade by Dr. Neuber†, 559 were in the direction of S., W., N., E., S.; 457 in the contrary.

12. *Sweden.* "In order to examine how far these changes (namely those motions of the barometer calculated by me in Paris, from the law of rotation for hydrometeors,) take place in other countries during rain. I have," says Dr. Kämtz‡, "brought together in a similar manner the calculations of Nicander in Stockholm. Of three observations out of nineteen, made between 2 and 9 o'clock, I have taken as a basis for the comparison the wind which blew about 2 o'clock. The following table contains the magnitudes found, in Parisian lines:

	Day before the Rain.	Rainy Day.
W.	+0·13	+0·22
NW.	+0·31	+1·06
N.	+0·42	+0·60
NE.	+0·6	+0·44
E.	−0·01	−0·41
SE.	−0·50	−0·65
S.	−0·41	−0·61
SW.	−0·71	−0·27

"On the day before and during the rain, the barometer sinks with easterly, rises with westerly winds, just as Dove has deduced from observations in Paris."

13. *North America.* In the State of Missouri, the wind in constant repetition traverses within from ten to twenty days every quarter of the horizon, and always in the following order, going from E. through S. to W., and through N. toward E. Duden§, who makes this remark, adds that he never had observed a completely opposite course.

14. *Germany.* Schübler‖ says: "The rotation of the winds

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* In regard to Horsley, Saussure is in error: he had only calculated the averages, but not examined the rise and fall.

† *Collectanea Meteorologica sub auspiciis Societatis Scientiarum Danicæ edita.* 1829.

‡ Meteor., vol. ii. p. 365.

§ Voyage to the Western States of America, p. 200.

‖ Fundamental Positions of Meteorology principally relating to Germany. 1831. p. 28.

takes place in Germany more frequently in the order of S. through SW., W., NW., N., NE., E., and SE., than in the opposite order of S. through SE., E., NE., &c."

[To be continued.]

XXVII. *A new Method of solving Equations of partial Differentials.* *By* S. S. GREATHEED, *Esq., B.A., of Trinity College, Cambridge.*[*]

SEPARATION of the symbols of operation from those of quantity, has, as far as I know, been hitherto applied only to the calculus of finite differences, and to the differential calculus where both are involved. It appears to me that if any much greater eminence than that to which analysis has already been brought, remains to be attained by it, that process is the most obvious and likely path. The following pages will show how, by applying it, a large class of partial differential equations, including nearly all that occur in applied mathematics, may be reduced to differential equations of two variables.

The following known theorem is one which will be constantly made use of.

The expression $\epsilon^{h\frac{d}{dx}} f(x)$ *is equivalent to Taylor's series for* $f(x+h)$.

I shall begin with the equation of the first order and degree with constant coefficients:

$$a\frac{dz}{dx} + b\frac{dz}{dy} = c.$$

If instead of $\frac{dz}{dy}$ we had nz, n being a constant, the equation would become

$$a\frac{dz}{dx} + nbz = c,$$

a linear equation between x and z, which may be solved by multiplying by the integrating factor $\epsilon^{\frac{nbx}{a}}$, whence

$$\frac{d}{dx}\left(\epsilon^{\frac{nbx}{a}} z\right) = \frac{c}{a}\epsilon^{\frac{nbx}{a}},$$

therefore

$$\epsilon^{\frac{nbx}{a}} z = \frac{c}{nb}\epsilon^{\frac{nbx}{a}} + C,$$

$$z = \frac{c}{nb} + \epsilon^{-\frac{nbx}{a}} C.$$

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[*] Communicated by the Author.

My method, then, for solving the proposed partial differential equation is this: Substitute $\frac{d}{dy}$ for n, or let n represent $\frac{d}{dy}$, and proceed exactly as in the above solution of the equation between two variables, substituting for the arbitrary constant an arbitrary function of y. The result then will be

$$z = \frac{c}{b}\left(\frac{d}{dy}\right)^{-1} + \varepsilon^{-bx\frac{d}{d.ay}} f(ay).$$

By the proposition which was premised, $\varepsilon^{-bx\frac{d}{d.ay}} f(ay)$ is equivalent to $f(ay-bx)$. Also, since integration is the reverse operation to differentiation, $\left(\frac{d}{dy}\right)^{-1}$ is equivalent to $\int dy = y$. Hence, finally,

$$z = \frac{cy}{b} + f(ay-bx),$$

which is the correct solution of the proposed equation. I shall proceed to explain the meaning, and prove the legitimacy, of the several steps in the solution.

In the first place, multiplying by the factor $\varepsilon^{\frac{bx}{a}\frac{d}{dy}}$* is equivalent to changing all functions of y to which it is prefixed into the same functions of $y + \frac{bx}{a}$. Now z is an unknown function of x and y, suppose it equal to $\phi(x, y)$, then

$$\varepsilon^{\frac{bx}{a}\frac{d}{dy}} \frac{dz}{dx} + \frac{b}{a}\varepsilon^{\frac{bx}{a}\frac{d}{dy}} \frac{dz}{dy}$$

is equivalent to

$$\frac{d}{dx}\phi\left\{x, y + \frac{bx}{a}\right\} + \frac{b}{a}\frac{d}{dy}\left\{x, y + \frac{bx}{a}\right\},$$

which is the *total* differential coefficient of $\phi\left\{x, y + \frac{bx}{a}\right\}$ with respect to x.

As for the other side of the equation, namely,

$$\varepsilon^{\frac{bx}{a}\frac{d}{dy}} . \frac{c}{a},$$

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* The more proper expression would be "prefixing the symbol $\varepsilon^{\frac{bx}{a}\frac{d}{dy}}$", but since it is convenient, and the operations are exactly analogous, I shall use the term "multiplying."

since $\frac{c}{a}$ does not contain y, the symbol prefixed to it has no effect, and

$$\varepsilon^{\frac{b\,x}{a}\,\frac{d}{d\,y}}\,\frac{c}{a}=\frac{c}{a}.$$

But the integrals of these two expressions with respect to x are not the same. This may be explained by writing instead of $\frac{c}{a}$, $\frac{d}{d\,y}\cdot\frac{c\,y}{a}$; which, by prefixing $\varepsilon^{\frac{b\,x}{a}\,\frac{d}{d\,y}}$, is changed to $\frac{d}{d\,y}\cdot\frac{c\left(y+\frac{b\,x}{a}\right)}{a}=\frac{d}{d\,x}\,\frac{c\left(y+\frac{b\,x}{a}\right)}{b}$.

Hence the equation

$$\frac{d}{d\,x}\left(\varepsilon^{\frac{b\,x}{a}\,\frac{d}{d\,y}}\,z\right)=\varepsilon^{\frac{b\,x}{a}\,\frac{d}{d\,y}}\,\frac{c}{a}$$

is the same as

$$\frac{d}{d\,x}\,\phi\left\{x,y+\frac{b\,x}{a}\right\}=\frac{d}{d\,x}\,\frac{c\left(y+\frac{b\,x}{a}\right)}{b}$$

(the first side representing the total differential coefficient with respect to x). By integration,

$$\phi\left\{x,y+\frac{b\,x}{a}\right\}=\frac{c\left(y+\frac{b\,x}{a}\right)}{b}+f\,(a\,y).$$

Finally, by multiplying both sides of the equation by $\varepsilon^{\frac{-b\,x}{a}\,\frac{d}{d\,y}}$, that is, changing functions of y into functions of $y-\frac{b\,x}{a}$,

$$\phi\,(x,y),\text{ or }z=\frac{c\,y}{b}+f\,(a\,y-b\,x).$$

Hence it may be seen that the essential part of the method consists in making each member of a partial differential equation a total differential coefficient with respect to one of the variables.

The most general class of equations of the first order, which can (as far as I am at present aware) be solved by this method, are those which fall under the form

$$\frac{d\,z}{d\,x}+\mathrm{X\,Y}\,\frac{d\,z}{d\,y}=\mathrm{P}\,z+\mathrm{Q},$$

where X is a function of x only, Y of y only, and P and Q are functions of both x and y. This may easily be reduced to the form

$$\frac{dz}{dx} + X\frac{dz}{dy} = Pz + Q,$$

for if $y' = \int\frac{dy}{Y}$, $Y\frac{dz}{dy} = \frac{dz}{dy'}$. I shall then consider the latter equation.

Let it be put under the form

$$\frac{dz}{dx} + \left(X\frac{d}{dy} - P\right)z = Q,$$

and treated as a linear equation between two variables z and x.

The integrating factor is $\varepsilon^{\int(X\frac{d}{dy} - P)\,dx} = \varepsilon^{X'\frac{d}{dy}} . \varepsilon^{-\int P\,dx}$, supposing $\int X\,dx = X'$. The effect of the first part of the factor, namely, $\varepsilon^{X'\frac{d}{dy}}$ is, as has before been stated, to change functions of y into functions of $y + X'$, and it affects, not only $\frac{dz}{dx}$ and z, but also the other part of the integrating factor, namely, $\varepsilon^{-\int P\,dx}$, provided P contain y; therefore y in P must be changed into $y + X'$ before the integration is performed, and afterwards, y is to be restored.

The equation will then stand:

$$\varepsilon^{X'\frac{d}{dy}} . \varepsilon^{-\int P\,dx}\left\{\frac{dz}{dx} + \left(X\frac{d}{dy} - P\right)z\right\}$$

$$= \varepsilon^{X'\frac{d}{dy}}\,\varepsilon^{-\int P\,dx}\,Q.$$

The first side is the total differential coefficient of $\varepsilon^{-\int P\,dx}z$ ($y + X'$ being substituted for y) with respect to x; and the second side is the similar differential coefficient of some unknown function of x and y. It would require the solution of the original equation to exhibit this function, but this difficulty is avoided by integrating the expression on the second side by the common rules. It remains to integrate both sides with respect to x, add an arbitrary function of y, divide both sides by $\varepsilon^{X'\frac{d}{dy}}$ (that is, change y into $y - X'$) and by $\varepsilon^{-\int P\,dx}$, and then the value of z is obtained.

I shall adjoin a few examples, but previously I shall prove the following curious theorem, which is of use in several.

$$\frac{1}{a \pm \dfrac{d}{d\log y}} \cdot y^m = \frac{y^m}{a \pm m},$$

where a is any constant.

Let $y = \varepsilon^t$, then $d \,.\, \log y = dt$, and

$$\frac{1}{a \pm \dfrac{d}{d\log y}} = \frac{1}{a \pm \dfrac{d}{dt}} \cdot \varepsilon^{mt}$$

$$= \frac{1}{a}\varepsilon^{mt} \mp \frac{1}{a^2}\frac{d\,\varepsilon^{mt}}{dt} + \frac{1}{a^3}\frac{d^2\varepsilon^{mt}}{dt^2} \mp \dots\dots\dots$$

$$= \left(\frac{1}{a} \mp \frac{m}{a^2} + \frac{m^2}{a^3} \mp \dots\dots\dots\right)\varepsilon^{mt}$$

$$= \frac{\varepsilon^{mt}}{a \pm m} = \frac{y^m}{a \pm m}.$$

From this it may be shown that if y, y', y'', &c. be any number of independent variables, and b, b', b'', &c. constants,

$$\frac{1}{a + b\dfrac{d}{d\log y} + b'\dfrac{d}{d\log y'} + b''\dfrac{d}{d\log y''} + \dots}\, y^m y'^{m'} y''^{m''} \dots\dots$$

$$= \frac{y^m y'^{m'} y''^{m''}}{a + mb + m'b' + m''b'' + \dots}.$$

Example 1. $x\dfrac{dz}{dx} + y\dfrac{dz}{dy} = nz.$

$$\frac{dz}{dx} + \left(\frac{1}{x}\frac{d}{d\log y} - \frac{n}{x}\right) z = 0.$$

The integrating factor is

$$\varepsilon^{\log x \frac{d}{d\log y} - n\log x} = x^{-n}\,\varepsilon^{\log x \frac{d}{d\log y}}.$$

Multiply by this factor, and integrate, therefore,

$$x^{-n}\,\varepsilon^{\log x \frac{d}{d\log y}}\, z = \phi(y) = \phi(\varepsilon^{\log y})$$

and
$$z = x^n\,\varepsilon^{-\log x \frac{d}{d\log y}}\,\phi(\varepsilon^{\log y})$$

$$= x^n\,\phi(\varepsilon^{\log y - \log x})$$

$$= x^n\,\phi\left(\frac{y}{x}\right).$$

Example 2. $\dfrac{dz}{dx} = \dfrac{y}{x}\,\dfrac{dz}{dy} + \dfrac{x}{y},$

By transposition $\dfrac{dz}{dx} - \dfrac{y}{x}\,\dfrac{dz}{dy} = \dfrac{x}{y},$

or $\dfrac{dz}{dx} - \dfrac{1}{x}\,\dfrac{d}{d\log y}\,z = \dfrac{x}{y}.$

The integrating factor is $\varepsilon^{-\log x \frac{d}{d\log y}}$. Multiplying by it, and integrating,

$$\varepsilon^{-\log x\,\frac{d}{d\log y}}\,z = \int \varepsilon^{-\log x \frac{d}{d\log y}}\,\frac{x}{y}\,dx$$

$$= \int \frac{x^{1-\frac{d}{d\log y}}}{y}\,dx$$

$$= \frac{x^{2-\frac{d}{d\log y}}}{2-\dfrac{d}{d\log y}}\cdot\frac{1}{y} + \phi(\varepsilon^{\log y}).$$

Divide by $\varepsilon^{-\log x\,\frac{d}{d\log y}}$ or $x^{-\frac{d}{d\log y}}$,

therefore $z = \dfrac{x^2}{2-\dfrac{d}{d\log y}}\cdot y^{-1} + \varepsilon^{\log x\,\frac{d}{d\log y}}\,\phi(\varepsilon^{\log y}).$

By the theorem above,

$$\frac{1}{2-\dfrac{d}{d\log y}}\,y^{-1} = \frac{y^{-1}}{3}.$$

Consequently $z = \dfrac{x^2}{3y} + \phi(xy).$

Example 3. $y\,\dfrac{dz}{dx} + x\,\dfrac{dz}{dy} = z,$

$$\frac{dz}{dx} + \left(\frac{x}{y}\,\frac{d}{dy} - \frac{1}{y}\right) z = 0,$$

or $\dfrac{dz}{dx} + \left(2x\,\dfrac{d}{d.y^2} - \dfrac{1}{y}\right) z = 0.$

The integrating factor is

$$\varepsilon^{\int\left(2x\,\frac{d}{d.y^2} - \frac{1}{y}\right)dx} = \varepsilon^{x^2\,\frac{d}{d.y^2}}.\,\varepsilon^{-\int\frac{dx}{y}}.$$

The effect of the first part of the factor on the second is to change it into

$$\varepsilon^{-\int\frac{dx}{\sqrt{y^2+x^2}}} = \varepsilon^{-\log(x+\sqrt{x^2+y^2})} = \frac{1}{x+\sqrt{x^2+y^2}}$$

$$= \varepsilon^{x^2\frac{d}{d\,.\,y^2}}\,.\,\frac{1}{x+y}.$$

Multiplying therefore by $\varepsilon^{x^2\frac{d}{d\,.\,y^2}}\frac{1}{x+y}$, and integrating,

$$\varepsilon^{x^2\frac{d}{d\,.\,y^2}}\frac{z}{x+y} = \phi\,(y^2),$$

$$z = (x+y)\,\varepsilon^{-x^2\frac{d}{d\,.\,y^2}}\phi\,(y^2)$$

$$= (x+y)\,\phi\,(y^2-x^2).$$

By the same principles the equation

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

may be integrated.

The method is equally applicable to equations of four or more variables, of which I shall take a single example.

Example 4.

$$u\frac{dz}{du} + x\frac{dz}{dx} + y\frac{dz}{dy} = a\,z + \frac{x\,y}{u}.$$

$$\frac{dz}{du} + \frac{1}{u}\left(\frac{d}{d\log x} + \frac{d}{d\log y} - a\right)z = \frac{x\,y}{u^2}.$$

Multiply by $\varepsilon^{\log u\left(\frac{d}{d\log x}+\frac{d}{d\log y}-a\right)} = u^{\frac{d}{d\log x}+\frac{d}{d\log y}\quad a}$,

and integrate, therefore

$$\varepsilon^{\log u\left(\frac{d}{d\log x}+\frac{d}{d\log y}-a\right)}z = \int u^{\frac{d}{d\log x}+\frac{d}{d\log y}-a\ -2}\,x\,y\,du$$

$$= u\,\frac{\frac{d}{d\log x}+\frac{d}{d\log y}-a-1}{\frac{d}{d\log x}+\frac{d}{d\log y}-a-1}\,x\,y + \phi\,(\varepsilon^{\log x}, \varepsilon^{\log y})$$

$$z = \frac{u^{-1}}{\frac{d}{d\log x}+\frac{d}{d\log y}-a-1}\,x\,y$$

$$+ u^a \phi\left(\varepsilon^{\log x - \log u},\ \varepsilon^{\log y - \log u}\right)$$

$$= - \frac{x\,y}{(a-1)\,u} + u^a \phi\left(\frac{x}{u},\ \frac{y}{u}\right).$$

There are several equations which are not of a form to which this method is immediately applicable, but which become so by a slight transformation. For instance, in the equation

$$\mathrm{X}\,\frac{d\,z}{d\,x} + \mathrm{Y}\,\frac{d\,z}{d\,y} = \mathrm{Z},$$

where X, Y, Z are functions of x, y, and z, respectively, assume

$$\int \frac{d\,z}{\mathrm{Z}} = z'$$

and it becomes

$$\mathrm{X}\,\frac{d\,z'}{d\,x} + \mathrm{Y}\,\frac{d\,z'}{d\,y} = 1.$$

In the equation

$$x\,\frac{d\,z}{d\,x} + z\,\frac{d\,z}{d\,y} + y = 0$$

make x the dependent variable, then

$$\frac{d\,z}{d\,x} = \frac{1}{\frac{d\,x}{d\,z}},\quad \frac{d\,z}{d\,y} = -\,\frac{\frac{d\,x}{d\,y}}{\frac{d\,x}{d\,z}}$$

and the equation becomes

$$y\,\frac{d\,x}{d\,z} - z\,\frac{d\,x}{d\,y} + x = 0$$

which may be solved in the same manner as Example 3.

The equation

$$(x+y)\,\frac{d\,z}{d\,x} + (y-x)\,\frac{d\,z}{d\,y} = z,$$

may be transformed so that this method shall be applicable, by taking $x+y = u$, and x and u for the independent variables. It then becomes

$$u\,\frac{d\,z}{d\,x} - 2\,x\,\frac{d\,z}{d\,u} = z.$$

As this article has already been extended to some length, I shall defer to a succeeding Number the application of my method to equations of the second and higher orders. It is in those chiefly that it claims a superiority to the method of Lagrange; and in cases to which his does not apply it gives, very readily, an abbreviated expression for the series, in which the solutions may be exhibited, but which would often be hard to determine by indirect methods.

Trin. Coll., Camb., July 20, 1837. S. S. GREATHEED.

XXVIII. *Memoranda on the Origin of the Botanical Alliances.*
By Sir EDW. FF. BROMHEAD, *Bart., MA., F.R.S. L. & E.*[*]

AGARDH mentions Batsch as the first writer who attempted to form alliances on a sound principle, though unsuccessfully; and Dr. Lindley justly notices "Agardhii opera æstumatissima Bartlingiique, qui viam ad meliores res aperuere." Agardh's work and a selection of others, which I in vain endeavoured to procure from the Continent, Dr. Lindley most kindly forwarded to me from his own library. By this I have been enabled to institute a comparison with the alliances proposed by botanists of every school, a test very useful in confirming sound assemblages, ascertaining their true limits, and developing the ground of their formation. Such a reference is moreover a necessary act of justice to the founders.

USNEALES.—These are the *Rhizophyta* or *Sporidiaceæ* of Rudolphi: "Sporangia spuria vel nulla, sporidia substantiæ vel superficiei immersa, cotyledonidia nulla." Linnæus doubted whether the Fungi should form part of the series in an arrangement of plants. They have been made sufficiently numerous to form one or more distinct alliances; some have represented them as passing into Lichens, others as passing into Algæ.

USNEOSÆ are nearly the *Sporidiferæ* of Agardh, and the *Fungales* of Dr. Lindley.

JUNGERMANNIALES.—These are the *Musci* (*hepatici* and *frondosi*) of Hedwig, who omits Riccia. They are the *Foliaceæ* of De Candolle, the *Musci* of Bartling, and the *Muscales* of Dr. Lindley.

JUNGERMANNIOSÆ are nearly Dr. Lindley's second section of Acrogens.

LYCOPODIALES.—The *Tetradidymæ* of Wahlenbergh, founded "quaternatâ dispositione sporarum," add to this alliance Ophioglossaceæ, and doubtfully Equisetaceæ. Agardh excluded the latter; Dr. Lindley excluded also the former from his *Lycopodales*. Lepidodendrum is now added: there cannot be any reason why extinct genera should be overlooked in botany more than in other branches of natural history. All

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[*] Communicated by the Author.

the great transitions in nature are through small families, in which the structure seems to want breadth, or what mechanicians would call stable equilibrium. A series of monographs of the families and tribes, which are limited to a few species, would be an important acquisition to science.

LYCOPODIOSÆ correspond with Dr. Lindley's first section of Acrogens.

CUPRESSALES.—These are the original *Coniferæ* of Linnæus, if we exclude Ephedra and Liquidambar.

CUPRESSOSÆ.—These are nearly the *Lepidanthæ acerosæ* of Shultz; or the *Gymnosperms* of Dr. Lindley's System. Horaninow, an acute Russian naturalist, includes the Lycopodial and Osmundal unions with Lichens under the name of *Sporophoræ*: "Synorrhizis sporæ conglutinantur ad embryonem producendum, ipsis innatum."

BETULALES.—These are nearly the *Amentaceæ* of Linnæus, who however placed Liquidambar among Coniferæ, and added to this alliance Myricaceæ, Platanaceæ, and Pistacia. Herman notices this form of inflorescence under the name of *Juliferæ*.

BETULOSÆ.—This formation perhaps more accurately represents the *Amentaceæ* of Linnæus. The *Lepidanthæ foliosæ* of Shultz nearly coincide. The *Micranthæ* of Agardh indicate a property important in this part of the system.

RHAMNALES.—The *Rhamnales* of Dr. Lindley consist of Rhamnaceæ, Chailletiaceæ, Tremandraceæ, Nitrariaceæ, Burseraceæ. Reichenbach includes several Rhamnales among his *Varifloræ Parvifloræ*.

EUPHORBIALES.—The *Tricoccæ* of Bartling are Stackhouseæ, Euphorbiaceæ, Empetreæ, Bruniaceæ, Rhamneæ, Aquifoliaceæ (Brexia), Pittosporeæ, Celastrinæ, Hippocrateaceæ, Staphyleaceæ. The *Euphorbiales* of Dr. Lindley are Euphorbiaceæ, Empetraceæ, Stackhousiaceæ, Fouquieraceæ, Celastraceæ (Hippocrateæ), Staphyleaceæ, Malpighiaceæ (Erythroxyleæ). The *Discigynæ Trihilatæ* of Agardh contain many unarranged Rhamnales, Euphorbiales, and Æsculales: calyx basi monophyllus, stamina definita discigena, carpellis subconnatis subternis.

ÆSCULALES.—The *Malpighinæ* of Bartling are Malpighiaceæ, Acerineæ, Coriarieæ, Erythroxyleæ, Sapindaceæ, Hippocastaneæ, Rhizoboleæ, Tropæoleæ. The *calycose* structure of the Nixus appears here.

HYPERICALES.—The *Guttales* of Dr. Lindley's *Calycosæ* are Clusiaceæ (Canelleæ), Rhizobolaceæ, Marcgraaviaceæ, Hypericaceæ, Ochranthaceæ.

LIMONIALES.—The *Meliales* of Lindley are Meliaceæ, Cedrelaceæ, Humiriaceæ, Aurantiaceæ, Spondiaceæ. The *Aurantiifloræ* of Reichenbach embrace several unarranged Hypericales and Limoniales, to which he adds Linaceæ and Leeaceæ. The "*Carpophyta, Thalamantheæ*" of Rudolphi embrace unarranged the families from Hippocrateæ to Limoniaceæ, both inclusive, to which he adds Ampelideæ, Dilleniaceæ, Magnoliaceæ, and Anonaceæ.

FABALES.—The *Legumina* of Cæsalpinus, or the *Leguminosæ* of Morison and Ray, must be considered the basis of this alliance; but they separated the herbaceous and woody plants. Royen united them.

FABOSÆ.—The *Calophyta* of Bartling include, among others, Rosales and Leguminosæ; Dr. Lindley unites these under his *Rosales* with Connaraceæ and Calycanthaceæ.

VIOLALES.—The *Violales* of Dr. Lindley, are Violaceæ (Sauvagesiæ), Samydaceæ, Moringaceæ, Droseraceæ, Frankeniaceæ.—Decandolle used the parietal structure as a means of classification. Dr. Lindley improved upon Agardh's *Valvisporæ*, and introduced the true distinctions.

PASSIFLORALES.—The *Passionales* of Dr. Lindley contain Passifloraceæ, Papayaceæ, Flacourtiaceæ, Pangiaceæ, Malesherbiaceæ, Turneraceæ.

PASSIFLOROSÆ.—The *Peponiferæ* of Bartling are Samydeæ, Homalineæ, Passifloreæ, Turneraceæ, Loaseæ, Cucurbitaceæ, Grossularieæ, Nopaleæ.

PORTULACALES.—The *Succulentæ* of Agardh are Melocactaceæ, Crassulaceæ, Mesembryanthaceæ, Portulacaceæ.

ELÆAGNALES.—The *Proteinæ* of Bartling are Laurineæ, Santalaceæ, Elæagneæ, Thymelææ, Proteaceæ. Dr. Lindley brought Penæaceæ into this neighbourhood. They are part of the miscellaneous *Rigidifoliæ* of Reichenbach.

ACANTHOSÆ.—The true *Monopetalæ* begin here; they seem to have been founded by Ray. Bartling's grouping of them is masterly, though the interior arrangements are not satisfactory.

LAMIALES.—These are the *Labiales* of Dr. Lindley. Dr. Brown formed *Verbenaceæ-Lamiaceæ* into a class. Arnott unites Selaginaceæ-Myoporaceæ-Verbenaceæ.

LAMIOSÆ.—These are the *DiscigynæMonopetalæ* of Agardh, who adds Pedaliaceæ: "fructu subtetraspermo; caryopses, vel capsula tetrasperma disco affixa." They are the *Nucamentosæ* of Dr. Lindley.

RHINANTHALES.—These are the *Scrophularineæ* of Dr. Brown, if we add Orobanchaceæ, and omit Digitaleæ, &c. The *Labiatifloræ* of Bartling embrace unarranged the whole Lamial union, if we add Oleaceæ and Jasminaceæ, and if we omit Verbasceæ-Digitaleæ. The *Personatæ* of Dr. Lindley's Nixus took the same range.

ERICALES.—These are the *Ericineæ* of Richard and Bartling; the *Ericales* of the Nixus. They are part of Agardh's miscellaneous *Aridifoliæ*: "folia coriacea, calyx sæpe coriaceus."

CAMPANULALES.—These are nearly the *Campanulaceæ* of Jussieu and Richard. They are the *Campanulaceæ* of Agardh.

CYNARALES.—The *Subaggregatæ* of Agardh include Campanulales and Cynarales, but he omits Plantaginaceæ, and adds Nyctaginaceæ with the tribe Staticineæ. The *Aggregosæ* of Dr. Lindley are Calyceraceæ, Compositæ, Dipsaceæ, Valerianaceæ, Brunoniaceæ, Plantaginaceæ, Globulariaceæ, Salvadoraceæ, Plumbaginaceæ.

MYRSINALES.—These are the *Primulales* of Dr. Lindley, if we add Brexiaceæ and Pittosporaceæ: he places Brexiaceæ near them, and mentions the affinity. The *Sapotaceæ* of Reichenbach contain Oleinæ, Styraceæ (Humiria), Trientaleæ, Myrsineæ, Olacinæ (? Millingtonia), Asterantheæ, Aquifoliaceæ (Brexia), Mimusopeæ.

RUTALES.—These are the *Rutaceæ* of Ad. de Jussieu, if we add Ochnaceæ. The *Discigynæ Polypetalæ Gynobaseæ* of Agardh are Rutales with Geraniaceæ, the other Geraniales being scattered in three or four classes: "Stamina disco inserta, stylus sæpe in Gynobasin dilatatus, carpella quina, sæpe libera vel apice lobata, subconnata." They are the *Rutales* of Dr. Lindley. They form the first six families of Bartling's *Terebinthinæ*; he relies much on the double pericarp here and elsewhere in the Usneaceous or woody race.

RUTOSÆ.—These are the *Gynobaseosæ* of Dr. Lindley, who adds Coriariaceæ.

MALVALES.—Dr. Brown must be considered the founder of this alliance; he first brought into notice the valvate æstivation. The *Columniferæ* of Agardh are Chlenaceæ, Tiliaceæ (Elæocarpeæ), Buttneriaceæ, Bombaceæ, Malvaceæ: "staminibus stylo adpressis, inæqualibus; styli plures, sæpe coaliti; folia lobata." The *Malvales* of Dr. Lindley are Sterculiaceæ, Malvaceæ, Elæocarpaceæ, Dipteraceæ, Tiliaceæ, Lythraceæ.

LAURALES.—These are nearly the *Lauri* of Jussieu. The *Laureales* of Dr. Lindley are Lauraceæ, Illigeraceæ, Cassythaceæ.

MAGNOLIALES.—The whole of the Magnoliales stood together in Dr. Lindley's first edition of his "Natural System." He has noticed the woody habit in this neighbourhood; his *Anonales* are Myristicaceæ, Magnoliaceæ, Anonaceæ, Schizandreæ, Dilleniaceæ.

Magnoliosæ.—This formation perhaps gives the correct limits of Dr. Lindley's important *Albuminosæ.*

Menispermosæ.—Linnæus was obviously acquainted with both the transitions to the Monocotyledons.

Asparagales.—The *Phylloacroblastæ Liliaceæ* of Reichenbach are Juncaceæ {Junceæ, Triglochinæ, Melantheæ, (Scheuchzeria, Butomus)}, Sarmentaceæ {Xeroteæ, Smilaceæ, Dioscoreæ}, Coronariæ {Methoniceæ (Alstromeria), Tulipaceæ, Gilliesieæ, Scilleæ, Hemerocallideæ, Alliaceæ, Dracæneæ}. Dr. Lindley has very happily disentangled the Liliaceæ.

Asparagosæ.—The *Endogenous* alliances begin here; Cæsalpinus vaguely marked them "Triplici principio fibrosæ et bulbosæ;" Ray must, it seems, be deemed the founder of the *Monocotyledons* as a class, though Lobel noticed the structure.

Bromeliales.—Richard unites the Hypocarpous Endogens (nearly Orchidales and Bromeliales) under the name of *Monosymphysogonie,* loosely arranged; he mentions as an objection, that Hydrocharaceæ would so be separated from Alismaceæ. Hess, a botanist of admirable tact, adopts Richard's arrangement, which is also more or less recognised by Dr. Lindley.—Agardh makes some Rhizanths stand next Hymenomycetes; if they all lie together, they may well form a distinct alliance.

Ulvales.—These are nearly the *Algæ.*

Charales.—The *Goniopterides* of Bartling contain Characeæ and Equisetaceæ. It may be doubted whether the shell of Sigillaria can be considered as bark.

Osmundales.—These seem to be nearly the *Capillares* of Morison and Ray. Dr. Lindley has judiciously broken them down into natural families.

Piperales.—The *Piperinæ* of Bartling include Chloranthaceæ, Piperaceæ, Saururaceæ. In his first edition of the "Natural System," Dr. Lindley threw together after the Chenopodiales the following families: Saurureæ, Chlorantheæ, Lacistemeæ, Piperaceæ, Podostemeæ, Callitrichineæ, Ceratophylleæ. This led directly to the Haloragales, and suggested the transition from the Urticales, on which the arrangement of the Ulvaceous race is founded.

Haloragales.—The *Halorageæ* of Bartling contain as sections Hippuridea, Callitrichea, Halorageæ (Trapa).

Œnotherales.—The *Calycifloræ* of Bartling contain his Halorageæ, Lythrarieæ (Elatinaceæ), Onagrariæ (Philadelpheæ), Rhizophoreæ, Vochysieæ, Combretaceæ, embracing nearly both alliances.

Myrtales.—The *Myrtinæ* of Bartling are Memecyleæ, Melastomaceæ, Myrtaceæ (Lecythidaceæ, excl. Punica).

Rosales.—These are the *Rosaceæ* of Jussieu, with the addition of some later discoveries.

Saxifragales.—The *Saxifragaceæ* of De Candolle include Saxifrageæ, Hydrangeaceæ, Cunoniaceæ, Baueraceæ, Escalloniaceæ. The *Corniculatæ* of Reichenbach contain Crassulaceæ (? Cephalotus, ? Francoa), Saxifrageæ {Genuinæ (Adoxa), Cunoniaceæ (Bauera, Hydrangea), Philadelpheæ}, Bruniaceæ. His Ribesiaceæ contain Cacteæ, Grossularieæ, Escallonieæ (Aristotelia).

Cucurbitales.—These are the last four families of Bartling's *Peponiferæ*, with the addition of Begoniaceæ, the affinity of which he hints at. The *Cucurbitales* of Dr. Lindley are Cucurbitaceæ, Loasaceæ, Cactaceæ, Homaliaceæ; he places Begoniaceæ near them.

Chenopodiales.—The *Oleraceæ* of Agardh contain Chenopodeæ (excl. Galenia, Salvadora, Theligonum), Rivineæ, Phytolacceæ (Miltus), Amaranthaceæ, § Illecebreæ, Petivereæ, Polygoneæ. The *Curvembryosæ* of Dr. Lindley are Amarantaceæ, Chenopodiaceæ, Tetragoniaceæ, Phytolaccaceæ, Polygonaceæ, Petiveriaceæ, Scleranthaceæ, Nyctaginaceæ, Menispermaceæ. The families of the Portulacales and Chenopodiales require to be recast; Bartling has effected much in his *Caryophyllinæ*.

Boraginales.—These are nearly the *Asperifoliæ* of Ray. Linnæus noticed the circinate inflorescence. The *Tubifloræ* of Bartling constitute unarranged the Boraginal union, if we add Plumbaginaceæ and Verbasceæ-Digitaleæ, and if we omit Retziaceæ.

Gentianales.—These are the *Gentianæ* of Jussieu, if we add Retziaceæ.

Apocynales.—These are nearly the *Contorti* of Linnæus. The *Contortæ* of Bartling embrace the Gentianales and Apocynales, if we add Retziaceæ, and omit Lygodysodeaceæ.

Galiales.—The *Rubiacinæ* of Bartling are Lygodysodeaceæ, Rubiaceæ (Stellatæ), Caprifoliaceæ, Viburneæ.

Cornales.—The *Cornales* of Dr. Lindley are Hamamelaceæ (Fothergilleæ), Cornaceæ, Loranthaceæ.

Geraniales.—The *Geraniaceæ* of St. Hilaire and Richard are Oxalideæ, Tropæoleæ, Balsamineæ, Linaceæ, Geranieæ. The *Geraniales* of Dr. Lindley are Geraniaceæ, Balsaminaceæ (Tropæoleæ), Oxalidaceæ; he mentions the affinity of Surianaceæ and Limnanthaceæ, and places them also among *Gynobaseosæ.*

CISTALES.—The *Cistales* of Dr. Lindley are Elatinaceæ, Linaceæ, Hugoniaceæ, Chlenaceæ, Cistaceæ, Reaumuriaceæ.

BRASSICALES.—This alliance was established by Dr. Brown, Resedaceæ being here removed, and Tremandraceæ being added. Bartling's *Rhœadæ* are Tremandreæ, Polygaleæ, Resedaceæ, Fumariaceæ, Papaveraceæ, Cruciferæ, Capparideæ; stress being laid upon the intervalvular placentation. The miscellaneous *Brevistylæ* of Agardh include Brassicales, Polygalaceæ, &c., but without Tremandraceæ: "fructibus solitariis brevistylis, stigmate suborbiculato; stylus o vel brevis et quasi acumen germinis; stigma orbiculatum, et peltatum vel capitatum."

NYMPHÆALES.—The *Ranales* of Dr. Lindley are Ranunculaceæ (Podophylleæ), Papaveraceæ (Fumarieæ), Nymphæaceæ (Hydropeltideæ), Nelumbiaceæ, Cephalotaceæ (Dionæa).

AVENALES.—The *Glumaceæ* of Reichenbach are Gramineæ, Cyperoideæ, Commelinaceæ {Restioneæ (Lilæa), Xyrideæ (Aphyllanthes, some Liliaceæ), Philydrinæ, Xyphidieæ (Flagellaria), Pontedereæ, Commelineæ}. The *Glumosæ* of Dr. Lindley take nearly the same range.

TYPHALES.—The *Spadicosæ* of Dr. Lindley are Pandanaceæ, Cyclanthaceæ, Araceæ, Acoraceæ, Typhaceæ, Naiadaceæ, Juncaginaceæ, Pistiaceæ.

July 13, 1837.

In a note upon Sir E. Ffrench Bromhead's former paper, inserted in our last volume, pp. 48, 137, the agreement of some of the principles of arrangement advocated by the author with those enunciated by Mr. Wm. S. Macleay was briefly pointed out. It may tend to preserve the interest now taken in the subject, if some further points of coincidence between the views of classification which have arisen in the mind of one naturalist from the investigation of the animal kingdom, and those which have been presented to that of another from the investigation of the vegetable kingdom, be also noticed. It is observed in the preceding paper, under the head 'LYCOPODIALES,' p. 247—8, that "All the great transitions in nature are through small families, in which the structure seems to want breadth, or what mechanicians would call stable equilibrium." Taking "families" to mean, merely, in this place, comparatively smaller groups than those which they are the means of uniting, the passage here cited is an accurate statement of the nature and characters of what have been termed by Mr. Macleay, *Osculant* groups. The want of "breadth," or "stable equilibrium" in their structure, is the consequence of their being the means of transition from one type to another, and uniting, therefore, in themselves, the structures of the groups which they thus connect. In the animal series, as Mr, Macleay has remarked, the osculant groups which unite the primary divisions are, comparatively, very imperfect beings; the result, doubtless, of the blending in them of the structures belonging to two distinct types.—E. W. B.

XXIX. *Account of some Experiments made in different Parts of reference to the Effect of Height.* By JAMES D. FORBES,

TABLE VI.

FLAT NEEDLE.

Place.	Date.	Mean Time.	No. of Vibrations observed.	Observed Time.	Rate Chronometer.	Arc.*		Temp. Reaum.	Corrected Time 100 Vibrations.	Intensity. Paris = 1.
						a.	*m.*			
	1832.	h m		s	s			°	s	
Edinburgh	June 2.	11 39	300	1075·97	+13	20	90	15·95	356·54	·839
Brussels	July 9.	12 55	300	1008·39	+17	20	100	23·1	333·38	·965
Königstuhl†	July 28.	11 17	100	327·84	+23	10	110	15·1	326·06	1·012
Heidelberg	July 28.	11 47	100	327·30	+23	10	110	15·1	325·52	1·015
Brühl	Aug. 1.	11 34	100	332·80	+27	10	140	17·0	330·72	·984
Brühl	Aug. 1.	11 47	100	333·51	+27	10	140	17·0	331·43	·980
Salève Summit	Aug. 17.	2 14	100	319·10	+20	10	120	17·4	317·14	1·073
Geneva	Aug. 20.	11 55	100	319·23	+27	10	120	22·2	316·80	1·076
Mont Breven	Aug. 22.	1 52	100	315·89	+27	10	130	16·2	314·01	1·095
Chamouni	Aug. 23.	12 56	100	318·83	+27	10	130	18·6	316·71	1·077
Jardin	Aug. 25.	12 45	100	317·47	+27	10	130	11·3	316·04	1·082
Chamouni	Aug. 26.	1 48	100	318·11	+27	10	140	15·3	317·01	1·075
Aoste	Aug. 29.	4 29	100	317·00	+27	10	140	16·1	315·12	1·089
St. Bernard[1]	Aug. 31.	8 46	100	318·93	+27	10	140 ?	9·1	317·69	1·072
St. Bernard	Aug. 31.	9 0	100	319·34	+27	10	140	9·6	317·84	1·071
St. Bernard	Aug. 31.	10 13	100	318·77	+27	10	140	11·3	317·88	1·070
Martigny[2]	Sept. 1.	8 54	100	318·81	+27	10	100	14·5	317·13	1·075
Martigny	Sept. 1.	9 7	100	318·84	+27	10	100	14·6	317·14	1·075
Interlaken	Sept. 10.	5 4	100	321·20	+14	10	140	14·05	319·52	1·061
Interlaken	Sept. 10.	5 16	100	320·93	+14	10	150	14·05	319·18	1·063
Schmadribach	Sept. 12.	12 59	100	319·69	+14	10	130?	11·05	318·32	1·069
Grindelwald	Sept. 14.	10 29	100	319·80	+14	10	130	14·45	318·12	1·071
Grimsel	Sept. 18.	8 6	100	320·53	+14	10	150	6·55	319·54	1·062
Grimsel	Sept. 18.	8 18	100	319·99	+14	10	150	7·0	318·98	1·066
Grimsel	Sept. 18.	8 29	100	320·27	+14	10	150?	7·4	319·25	1·064
Münster	Sept. 18.	5 58	100	319·84	+14	10	140	9·5	318·60	1·068
Gemmi Summit	Sept. 21.	9 12	100	319·79	+14	10	140	9·65	318·54	1·069
Grindelwald	Sept. 23.	7 13	100	320·14	+14	10	120	6·7	319·21	1·065
Grindelwald	Sept. 23.	9 30	100	320·46	+14	10	140	8·2	319·33	1·064

Europe, on Terrestrial Magnetic Intensity, particularly with Esq., F.R.SS. L. & E., &c., Prof. Nat. Phil., Edinb.

(Continued from p. 174.)

Faulhorn	Sept. 24.	8 43	100	320·69	+14	10	150	7·5	319·61	1·062
Faulhorn	Sept. 24.	8 54	100	321·03	+14	10	140	7·0	320·02	1·060
Faulhorn	Sept. 24.	9 6	100	321·09	+14	10	140 ?	6·8	320·11	1·059
Surennes	Sept. 28.	11 20	100	321·39	+14	10	140	12·65	319·85	1·062
Klus, near Altorf . . .	Sept. 28.	5 49	100	321·13	+14	10	130	9·85	319·86	1·061
St. Gothard	Sept. 30.	9 17	100	320·73	+14	10	130	6·3	319·81	1·062
Locarno	Oct. 2.	2 52	100	319·50	+14	10	150	18·55	317·39	1·079
Bellaggio	Oct. 8.	8 38	100	317·51	+14	10	130	13·4	315·93	1·090
Wallenstadt	Oct. 12.	10 7	100	321·64	+14	10	130	13·05	320·03	1·063
Lucerne	Oct. 15.	10 26	100	321·94	+14	10	140	9·0	320·74	1·058
Geneva	Nov. 10.	11 58	100	321·10	+21	10	130	7·0	320·08	1·067
Geneva	Nov. 10.	12 10	100	321·16	+21	10	130	7·0	320·14	1·066
	1833.									
Edinburgh	May 7.	5 14	300	1094·00	+ 3	20	120	17·65	362·20	·840
Edinburgh	May 7.	5 36	100	364·34	+ 3	10	120 ?	17·0	362·2	·840
Paris	June 11.	2 0	300	1005·17	...	20	150	20·8	332·34	1·000
	1835.									
Edinburgh	May 4.	1 53	100	371·84	...	10	80	9·0	370·65	·840
Edinburgh	May 4.	2 7	100	371·39	...	10	120	8·7	370·15	·842
Paris	June 13.	4 8	100	342·19	−40·5	10	130 ?	19·9	340·04	1·000
Paris	June 13.	4 22	100	342·00	−40·5	10	130 ?	19·8	340·10	1·000
Pic de Bergons[3]	July 18.	4 23	100	324·63	+20	10	150	14·95	322·81	1·112
Pic de Bergons[4]	July 21.	11 17	100	324·81	+20	10	120	15·0	323·04	1·111
Pic de Bergons[4]	July 21.	11 31	100	324·84	+20	10	130	15·1	323·04	1·111
Luz	July 28.	11 44	100	323·40	+20	10	130	19·0	321·23	1·124
Luz	July 28.	11 58	100	323·48	+20	10	130 ?	19·0	321·31	1·123
Luz	July 28.	12 18	100	323·40	+20	10	80	19·0	321·34	1·123
Gavarnie.	July 29.	12 32	100	323·43	+20	10	140	19·0	321·25	1·124
Ste Marie[5]	Aug. 7.	10 48	100	323·98	+20	10	140	19·55	321·68	1·121
Ste Marie[6]	Aug. 7.	11 7	100	324·09	+20	10	140	19·85	321·82	1·120
Pic de Midi	Aug. 8.	11 5	100	323·71	+20	10	130 ?	11·25	322·28	1·117
Pic de Midi	Aug. 8.	11 22	100	324·03	+20	10	150	10·4	322·64	1·115
Brèche de Roland[7] . . .	Aug. 11.	11 17	100	324·37	+20	10	140	12·6	322·78	1·114

* *α.* indicates the initial semi-arc of vibration; *m.* the number of vibrations required to reduce it to half its amount.

† Near Heidelberg, [1] The dipping-needle at first near the instrument. [2] Local disturbance suspected. [3] Small compass in pocket. [4] Unexceptionable. [5] Unsteady. [6] Good. [7] Extremely tremulous. Gusts of wind.

TABLE VII.

Place.	Particular Situation.	Latitude, N.	Long. from Paris.	Height, Eng. feet.	Observed Intensity: Paris = 1·000.					
					Needle, No. 1.		Mean.	Flat Needle.		Mean.
Edinburgh	Greenhill, field, June 1832	55° 57′	5° 33′ W.	300	·840		·840	·839		·840
Edinburgh	——— garden, May 1833	55 57	5 33	300	·841	·842		·840	·840	
Edinburgh	——— field, S. of garden, May 1835	55 57	5 33	300	·839	·839		·840	·842	
Brussels	Enclosure of Observatory, 20 yards from NW. corner of the building	50 51	2 2 E.	300	·959	·960	·960	·965		·965
Spa	South face of clay-slate hill, close to the town, on the NW. side	...	...	...	·981	·977	·979			
Königstuhl, near Heidelberg	Summit of the hill	49 25	6 23	1700	1·018	1·018	1·018	1·012		1·012
Heidelberg	Prof. Leonhard's garden on a stone table	49 25	6 12	300	1·017		1·017	1·015		1·015
Brühl	In a quarry near the bank of the Rhine	...	...	...	·987 ·985	·985	·986	·984	·980	·982
Laach	NW. side of the lake	50 25	4 56	1000	·982	·985	·984			
Mont Salève, near Geneva	Summit of the Grand Salève	46 6	3 51	4500	1·078	1·077	1·077	1·073		1·073
Geneva	Botanic Garden, August 1832	46 12	3 49	1300	1·078	1·078	1·076	1·076		1·071
Geneva	——— November 1832	46 12	3 49	1300	1·075 1·074	1·073		1·067	1·066	1·095
Mont Breven	Summit.	45 56	4 30	8400	1·089		1·089	1·095		1·076
Chamouni	On the further side of the Arve from the village	45 55	4 32	3400	1·085	1·085	1·085	1·077	1·075	1·082
Jardin	At the "Pierre d'Herschel"	45 55	4 39	9000	1·084		1·084	1·082		
Col des Fours	Close to the snow, in a cleft of rock	45 45	4 25	8900	1·088		1·088			
Aoste	In a summer-house (built entirely of wood, and without nails) in the garden of the inn	45 44	5 00	1900	1·096		1·096	1·089		1·089
St. Bernard	Between the Hospice and the lake	45 52	4 50	8100	1·082		1·082	1·072 1·070	1·071	1·071
Martigny	Garden of the inn	46 6	4 45	1600	1·083		1·083	1·075	1·075	1·075
Interlaken	On the bank of the Aar	46 42	5 32	1900	1·068		1·068	1·061	1·063	1·062
Schmadribach	Near the upper cascade (at the head of the valley of Lauterbrunnen)	46 31	5 33	5200	1·077	1·081	1·079	1·069		1·069
Grindelwald	Behind the inn; 14th Sept. 1832	46 38	5 42	3700	1·074	1·076	1·075	1·071		1·071
Grindelwald	At the lower glacier; 23rd Sept. 1832.	46 38	5 42	3400	1·072		1·072	1·065	1·064	1·064

Meyringen	Near the church	46 44	5 52	2100	1·073 1·076	1·075		
Grimsel	Near the Hospice	46 34	5 59	6200	1·074	1·074	1·062 1·066 1·064	1·064
Münster (Vallais)	On the bank of the stream, just above the town.	46 30	5 57	4200	1·080 1·077	1·078	1·068	1·068
Gemmi	Within shelter-house near the summit	46 25	5 17	7500	1·079 1·078	1·078	1·069	1·069
Frütigen (Kanderthal)	A little below the town	46 36	5 18	2300	1·074 1·073	1·073		
Faulhorn	Summit, to the W. of the house	46 40	5 40	8900	1·070 1·071	1·071	1·062 1·060 1·059	1·060
Engelberg	At the side of the road before reaching the town	46 49	6 7	3400	1·068 1·073	1·071		
Surennes	Summit of the Pass	46 49	6 13	7700	1·072 1·071	1·071	1·062	1·062
Klus (near Altorf)	On a muir.	46 49	6 19	1600	1·071 1·072	1·072	1·061	1·061
St. Gothard	At the summit-level N. of the Hospice	46 34	6 14	7100	1·070 1·072	1·071	1·062	1·062
Locarno	In the bed of the torrent below the Convent	46 10	6 28	700	1·084	1·084	1·079	1·079
Orta (on Lake Orta)	At the side of the road, above the town	45 47	6 4	1000?	1·093 1·092	1·092		
Bellaggio (L. Como)	On the shore N. of the town	46 00	6 56	700	1·096 1·094	1·095	1·090	1·090
Reichenau	On the bank of the Upper Rhine, a little above the junction of the streams	46 49	7 4	2000	1·075 1·077	1·076		
Wallenstadt	Near the lake; S. end	47 7	7 00	1400	1·070 1·068	1·069	1·063	1·063
Lucerne	In a wood-yard on the E. side of the lake, near the bridge	47 03	5 59	1500	1·068	1·068	1·058	1·058
Rigi Culm	To the N. of the inn	47 03	6 09	5900	1·062	1·062		
Paris Observatory	M. Arago's Magnetic Cabinet, June 1833	48 50	0 00	200	1·000 1·000	1·000	1·000	1·000
Paris Observatory	——— ——— ——— June 1835	48 50	0 00	200	1·001 1·000		1·000	
Pic de Bergons, near Luz, Hautes Pyrénées	Summit, 18th July 1835	42 50	2 18 W.	6900	1·116 1·115 1·113	1·115	1·112	1·111
Pic de Bergons	——— 21st July 1835	42 50	2 18	6900	1·117 1·113 1·117		1·111 1·111	
Luz, Hautes Pyrénées	Field E. of the village, 20th July	42 51	2 20	2400	1·123 1·125 1·122 1·131	1·125		
Luz, Hautes Pyrénées	——— 28th ———	42 51	2 20	2400	1·125 1·124		1·124 1·123 1·123	1·123
Gavarnie	Near the road, to the N. of the inn	42 43	2 21	4500	1·127 1·126	1·126	1·124	1·124
Ste Marie, Vallée de Campan	In a field above the village	42 59	2 07	2800	1·121 1·120	1·120	1·121 1·120	1·120
Pic du Midi de Bigorre	Summit	42 55	2 14	9600	1·122 1·121 1·121	1·121	1·117 1·115	1·116
Brèche de Roland	W. side; N. aspect of the Breche	42 41	2 20	9300	1·122 1·123	1·123	1·114?	

§ 4. *On the Direction of the Isodynamic Lines (for horizontal Intensity) in the Central Alps, and in the Pyrenees; and on the Influence of Height.*

25. The next question comes to be how to deduce the general results contained in the preceding tables. Where it is merely required to deduce the position of isodynamic lines (which may be considered as sensibly straight for a district of moderate extent), projection of the results upon paper would afford quite a sufficient approximation, where the stations are sufficiently multiplied. Thus the variations in latitude and longitude would be determined, and lines of intensity 1·00, 1·01, 1·02, &c. might be drawn with great accuracy upon a geographical map.

26. But the same process will not suffice, if we have a third variable, such as height, and require to extract its influence. The problem, then, is not to draw lines, but planes of equal intensity. For its solution I resolved to use the method of least squares*, which is peculiarly applicable to a question of the kind just stated, and may be made to give, as will immediately be seen, the most probable value of the four following quantities, viz. the variation of intensity for 1′ of latitude; its variation for 1′ of longitude; its variation for 100 feet of elevation; its most probable absolute value at the origin of the coordinates, or the station to which the others are referred.

27. I assumed that the intensity of any point whose coordinates of latitude, longitude, and height, might be denoted with sufficient accuracy by an expression of the form

$$a x + b y + c z = 1 \quad \ldots\ldots\ldots\ldots\ldots \quad (1.)$$

a, b, and c indicating the position of the point by reference to the three coordinates, whilst x, y, and z denote the coefficients of variation of intensity according to each of these, and which are to be discovered. The above expression being the equation to a plane, denotes that the isodynamic lines are not considered as curved, but as straight, which though not absolutely accurate, may be admitted in a country of small extent.

28. Eq. (1) gives the intensity I in terms of a, b, and c, the

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* It would be absurd to claim any merit for the application of a method so universally known. But lest I should be supposed to have borrowed without acknowledgement the method of reduction employed by Professor Lloyd and Captain Sabine in their excellent Magnetic Survey of Ireland (Fifth Report of the British Association), I desire to state, that I had some years ago proposed to myself the present method of reduction as the only one adapted finally to solve (within the present limits of error) the question of the influence of height, which so greatly complicates the problem.

coordinates of the place; a being reckoned in *minutes* of latitude, b in *minutes* of longitude, c in *hundreds of feet* of elevation. It is convenient to assume some station as a point of reference, and write for a, b, and c, the *differences* of the coordinates merely, and for I the *difference* of intensities. Let a', b', c', and I' represent these quantities for the fundamental station, and then for any other the expression will be

$$(a-a')\,x + (b-b')\,y + (c-c')\,z = \mathrm{I}-\mathrm{I}'$$

and by a combination of all the equations of similar form which the observations furnish, we are to deduce the most probable values of x, y, and z, the coefficients of variation in each direction. If, further, we wish to have the most probable *absolute* value of the horizontal intensity at the fundamental station before mentioned, it must clearly be deduced from the whole mass of the observations, and not from the observation made there alone. Let us suppose, then, that the intensity at the fundamental station requires a small correction, $\delta\,\mathrm{I}'$, we shall write $\mathrm{I}'+\delta\,\mathrm{I}'$ instead of I' in the preceding expression, considering $\delta\,\mathrm{I}'$ as another unknown quantity, which will give us a series of equations (for the different points of observation) of the form

$$(a-a')\,x + (b-b')\,y + (c-c')\,z = \mathrm{I}-\mathrm{I}'-\delta\,\mathrm{I}' \;\ldots\ldots\; (2.)$$

or using the letters with subscript numerals instead of $a-a'$, &c., and putting all the unknowns on the left hand, we shall have a series of equations of condition of the form

$$\left.\begin{array}{l} a_1\,x + b_1 y + c_1\,z + \delta\,\mathrm{I}' = \mathrm{I}_1 \\ a\ \,x + b_2 y + c_2\,z + \delta\,\mathrm{I}' = \mathrm{I}_2 \\ \qquad\qquad \&\mathrm{c.} \end{array}\right\}(3.)$$

from which the most probable values of x, y, z, and $\delta\,\mathrm{I}'$ are to be deduced by the method of least squares.

29. The observations contained in Table VII. include two groups of observations, to which we mean to apply the method in question. One of these includes the alpine observations made in August, September, and October 1832; the other a short series in the Pyrenees, made almost entirely with reference to the effect of height in 1835. The remaining observations must be considered for the present as isolated. They are important, however, as fixing the relative horizontal intensities at Paris, Edinburgh, Brussels, Heidelberg, and some points of less note. The admirable coincidence of the Edinburgh observations made in different years gives great confidence in the accuracy of the determination of ·8402 for the horizontal intensity, that at Paris being = 1; both needles

giving the same mean result to four decimal places. Professor Hansteen has given ·8428, which must be considered as a close coincidence.*

For Brussels I find by "No. I."	0·960
by "Flat,"	0·965
Captain Sabine	0·951
M. Quetelet (4 series)	0·964
M. Rudberg	0·971

I subjoin a few comparisons of stations common to M. Quetelet's series† and mine.

	Quetelet.	Forbes, No. 1.
Castle of Heidelberg	1·020	1·017
Town of do.	1·024	
Königstuhl (summit)	1·027	1·018
Geneva	1·080	1·076
Chamouni	1·093	1·085
St. Bernard	1·097	1·082
Martigny	1·092	1·083

[To be continued.]

XXX. *On a New Rain Gauge. By the Rev.* THOMAS KNOX, *M.R.I.A.*‡

[With a Figure: Plate II.]

ON the 26th of June last, a new rain-gauge was exhibited to the Royal Irish Academy, contrived by the Rev. Thomas Knox.

The object of this instrument is to register the amount of rain that falls when the wind is in different points. Its construction is very simple. The water,—instead of descending from the reservoir directly into the tube of registry,—passes through a lateral tube into an annular-shaped vessel, divided into eight compartments, each of which terminates below in a graduated glass tube. It is obvious, then, that if the eight tubes be set to correspond with the cardinal and intermediate points, and that the reservoir be made to revolve on a vertical axis by means of a vane, the direction of which corresponds with that of the lateral tube, the object proposed will be attained. Mr. Knox has preferred to make the reservoir fixed, and the system of tubes moveable; but the result is obviously the same.

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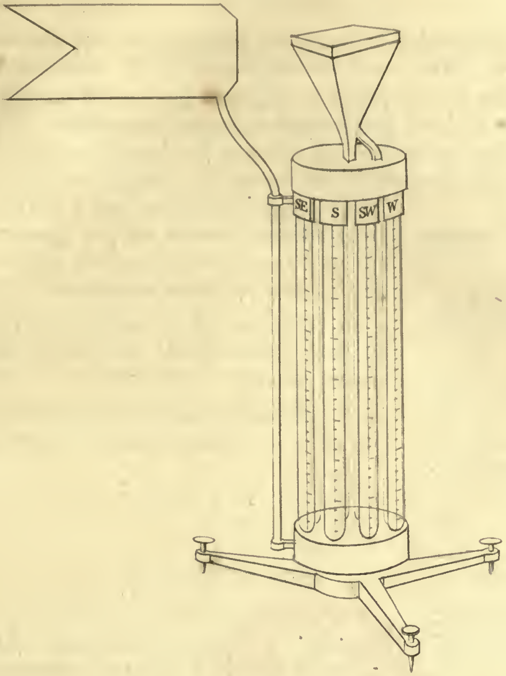
[*] Since this paper was read, this result has been still more nearly confirmed by the observations of Professor Bache of Philadelphia, who, by connecting Edinburgh and Dublin, and taking Professor Lloyd and Captain Sabine's observations for the comparative intensities at Dublin and Paris, has obtained the number ·8400.

[†] See his two papers in the *Mémoires de l'Académie de Bruxelles,* tome iv.; and an abstract in the *Annuaire de l'Observatoire de Bruxelles,* 1834.

[‡] From the Proceedings of the Royal Irish Academy, No. 5.



The Rev. Mr. Knox's Rain Guage

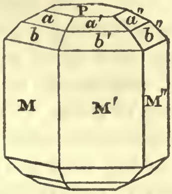
Scales of Recent & Fossil Salmon

J. Basire sc.

XXXI. *On the Crystalline Form of Pyrosmalite: hitherto undescribed.* *By* H. J. Brooke, *Esq., F.R.S., &c.*[*]

HAVING in my possession a specimen of this mineral with well-formed crystals resembling the annexed figure, and having measured them with the reflective goniometer, I am enabled to supply a description of their form, which has not yet, that I am aware of, been given.

Assuming, for the sake of simplicity, the regular hexagonal prism as the primary form, and that the planes *b* result from a decrement by one row corresponding to $\overset{1}{B}$, the ratio of a terminal edge to a lateral edge will be found as 15 to 16 nearly, and the planes *a* will correspond to the symbol $\overset{2}{B}$.



The angular measurements are as follows:

P on *a* = 148° 30′
b = 129 13
M = 90
M on M′ = 120

H. J. B.

XXXII. *Papers on the alleged Periodical Meteors of the* 13*th of November, and on Shooting Stars in general.* *By* M.M. Wartmann *and* Quetelet.

I. *Notice respecting the Periodic Meteors of the* 13*th of November.* *By M.* L. F. Wartmann[†].

A COSMOLOGICAL phænomenon of the most interesting kind, although new as yet in the records of science, is at present attracting the attention of astronomers, meteorologists, and physicists. The magnificent assemblage of luminous points and globes which has been seen for several years presents a highly important subject of inquiry, by which we may be enabled to add to the stock of our knowledge respecting the constitution of our planetary system.

The appeal made upon this occasion by M. Arago spread far and wide, and this very year [1836] numerous observations, made in different places, have been sent to the illustrious philosopher who had asked for them. They concur in showing that, towards a point of the heavens at a small distance from the stars β and γ in the constellation Leo, a considerable quantity of shooting stars seem to be produced, and to succeed one

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[*] Communicated by the Author.

[†] From the *Bibliotheque Universelle,* N. S. 2 de Ann. No. 18, June 1837: having been read before the Society of Physics and Natural History of Geneva, Dec. 15, 1836.

another at short intervals, precisely in the place where a prodigious number of them had already been seen at Geneva in 1832, and especially in the United States in 1833. What is the nature of these fugitive stars? Whence do they come? Whither do they go when they disappear from our sight? Do they sometimes fall upon the earth? Such are the principal questions which every one asks himself, and which are of the highest interest.

The much-wished-for fall of one of these meteors would without doubt furnish the chemist and physicist with the means of explaining certain points quite unknown. Those observers also, who were aware of the importance of this inquiry, have not neglected to bestow their attention in this direction, and some of them, in fact, state that they have seen several of these meteors which were projected against the sides of the mountains by which they were surrounded. This fact is undoubtedly of a positive nature, but is it such as to prove the authenticity of the fall of the meteor down to the surface of the earth? Have not the illusions which exercise so great an influence here, and under which observers are more or less placed, contributed to a belief in a projection towards the ground which was apparent only? In support of this suspicion I may be allowed to mention a fact which I had an opportunity of stating more than six years ago in the former series of the Journal of Geneva, in the numbers for March and April, 1830, as follows: A meteor appeared on the 19th of March of the before-mentioned year, at half-past seven in the evening; according to the report of eye-witnesses, it had a round disc, with a well-defined edge, which was almost equal to that of the full moon, and which shed a strong light of a bluish colour; it circulated with great velocity from east to west, and appeared to be at a very great height. Those who observed it at Geneva, and who followed it with their eyes in its horizontal course, thought they saw it burst in the air, and fall in pieces at some paces before them. Other persons, living at the village of Chêne, half a league from Geneva, and who were by chance in the street, being convinced that they had seen it fall on a neighbouring house, ran directly to ascertain whether the building had not been set on fire. This same meteor was also remarked at Saint-Legier, near Vevey, in the canton of Vaud, and on the heights of Fraubrunnen in the canton of Berne. Those who saw it from this last place, and who followed it for about thirty seconds, agree in saying that it travelled slowly in the direction of the Jura, and that it appeared to them to fall not far from the neighbourhood of Orbe, a small town of the canton of Vaud, thirteen leagues north-east of Geneva. Thus in the three situations, the illusion of the observers was so complete, that in spite of the distance which separated them, namely, in the one case a half league and in the other more than thirteen leagues, they each thought they saw the meteor fall down near them. Such a fact evidently shows that this fall is by no means real, and that if the meteor seemed to descend towards the horizon, this circumstance without doubt is owing to the quick decrease of the angle of sight which measured its apparent height, as the meteor was in rapid motion away from the observers as it pursued its horizontal course.

The appearance of this isolated meteor showed a sufficiently remarkable resemblance to those which for some years past have been seen periodically towards the middle of November, to make it desirable that an opportunity should occur of verifying whether there are any amongst these last which really fall upon our globe.

The night of the 12th to the 13th of November, this year, appeared to me proper for this interesting inquiry, from the meteorological circumstances with which it was attended at Geneva, and which I hastened to avail myself of. Rather thick clouds completely veiled the heavens in a uniform manner; they occupied a very elevated region, where they remained stationary all the night. The temperature was mild, the air calm, and the darkness great, although no fog thickened the transparency of the atmosphere.

The barometer, the thermometer, the hygrometer, the magnetic needle, the ethrioscope and the etectroscope were attentively watched at the observatory from seven in the evening to seven in the morning, and their progress marked with care every quarter of an hour*. At the beginning of the observations, at seven o'clock in the evening of the 12th, the barometer reduced to the zero degree marked $725^{\mathrm{mm}}{\cdot}08$, the centigrade thermometer in the open air $+ 7^{\circ}{\cdot}8$, and Saussure's hygrometer 87°. At midnight the first of these instruments was at $726^{\mathrm{mm}}{\cdot}95$, the second at $+ 6^{\circ}{\cdot}9$, and the third at 93°. On the 13th, at seven in the morning, the barometer marked $729^{\mathrm{mm}}{\cdot}30$, the thermometer $+ 5^{\circ}{\cdot}2$, and the hygrometer 98°.

To sum up, I shall say that the barometer, whose progress was gradually ascending, rose in the space of twelve hours of observation $4^{\mathrm{mm}}{\cdot}22$; that the thermometer, whose minimum had been $+ 5^{\circ}{\cdot}2$, varied in the same space of time only $2^{\circ}{\cdot}6$; and that the hygrometer proceeded 11° towards humidity. As to the ethrioscope, it did not give (as might be anticipated,

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* Two of the instruments, the compass and one of the electroscopes, belong to the Cabinet de Physique of our Academic Museum; these were kindly placed at my disposal, for which my best thanks are due to the directors.

with a tranquil and regularly clouded sky) any sign of radiation of heat across the atmosphere. The pith-ball electrometers, placed in the open air, remained motionless; lastly, the magnetic needle presented, at eleven minutes past nine, a slight deviation of 0°·5 to the east in declination; a deviation which remained the same till a quarter of an hour after midnight, after which it varied, always in the same direction, and till the morning, between 0°·1 and 0°·7.

Assisted by three amateurs who wished to join me, a continued look-out was kept, not only towards the region of the east, but in every quarter of the heavens; the terrace of the Observatory commanding the entire horizon.

From seven to ten o'clock in the evening a light breeze prevailed, hardly perceptible, which blew from the north-east; and from ten in the evening till seven in the morning the air remained perfectly calm, excepting at three periods, namely, at two, at forty-five minutes past three, and at fifteen minutes past four, when a light breeze was again perceived, and lasted each time ten minutes.

At forty-five minutes past eight in the evening, and from the south-south-east, a feeble white light illuminated the upper part of the clouds for from three to four seconds. At fifty-one minutes past nine a reddish light, resembling lightning, streaked the upper part of the clouds for nearly three seconds, in the east. At forty minutes after eleven there were white glimmerings, very feeble, which streaked the clouds between the north-east and the south-east; they had a kind of intermitting, and they lasted about six seconds. At thirty-five minutes past one, and directly in the east, there were, in the upper region of the clouds, some lights, in general very feeble, which continued during ten seconds. Lastly, at three minutes past four a white light, less pale, shone for two or three seconds in the elevated stratum of the clouds, in the south-east. But during the whole night, not a single luminous meteor, no shooting star, no aërolite or visible asteroid pierced the clouds to fall in the circle of our horizon. Nevertheless, it is probable that if the sky had not been clouded we should here have very well seen the shooting stars which were observed at the same date in our neighbourhood, in France and elsewhere; and perhaps the magnificent spectacle which the sky presented at Geneva in the night of the 12th to the 13th of November, 1832, of which Professor Gautier gave an account in the fifty-first volume of the *Bibliothèque Universelle*, might again have been exhibited before our eyes.

The result, then, of our observations is, that the shooting stars circulate in a much more elevated region of the sky than that attained by the clouds, and that meteors of this kind rarely fall to the surface of the earth, if they ever do. This opinion acquires so much more probability, as no one till now, at least so far as I know, has been able to obtain an authentic specimen of this mysterious substance.

A learned astronomer communicated to the Academy of Sciences of Paris, in the session of the 5th of this month (December 1836) a curious and very interesting memoir, which appears to suggest that the luminous nebulosity by which the sun appears to be surrounded in the direction of its equator, a nebulosity which is projected far into space, assuming the form of a cone, and which has been known for two centuries by the name of the zodiacal light, might probably be the source of the myriads of shooting stars of the 13th of November, the earth at this epoch passing in the neighbourhood of the summit of this cone. Nevertheless, the author of the memoir, M. Biot, after having considered the subject under different views and discussed it scientifically, ends by declaring that he neither asserts nor rejects this identity*.

Some writers think that the origin of the shooting stars which compose the periodical phænomenon of the 13th of November, might also be ascribed to a great planet which may formerly have been broken into a multitude of fragments, which would continue to circulate one after the other in an orbit whose position is such that the earth approaches annually very near to it on the 13th of November. These fragments, endowed with a great velocity of projection, would enter our atmosphere at this period, cross it rapidly, and, by the friction caused by the resistance of the air, would grow so hot there as to become incandescent and to send forth a bright light until the moment of their quitting it.

This hypothesis, very ingenious as it may appear, is not free from objection. Already a celebrated philosopher, whose opinion is of great weight, has not hesitated to say that it would be premature to attempt ascending to the physical cause of these curious appearances until certain matters of fact had been cleared up †; and assuredly M. Arago is right.

It is certain that in one and the same night an innumerable multitude of these meteors have been seen in places whose geographical situation differs 90° in longitude and six hours in time, a circumstance which gives to their appearance a duration of at least 18 hours; our night being at this period

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* *Comptes Rendus de l'Acad. de Paris,* No. 23, December 1836, vol. iii. p. 663.

† *Ibid,* p. 633.

more than 12 hours, and beginning 6 hours sooner than in the United States.

Now as in the month of November the earth advances in its orbit 445,500 leagues in 18 hours, a change of place during which the appearances are incessantly succeeding one another, it would be necessary that these meteors, if they really constitute asteroids, should exist by millions in the zone where they appear. But then these heavenly bodies, which should approach so very near to the earth, must frequently fall down upon it, from the attractive power which the mass of our globe would inevitably exercise on them; and this is what has not yet been observed.

When astronomers at the beginning of this century had successively discovered Ceres, Pallas, Juno and Vesta, which revolve around the sun between Mars and Jupiter in orbits which have not a very great eccentricity, the idea of making asteroids originate from the fragments of a planet which might have been destroyed by means of an internal explosion was already put forth; but M. Biot remarked that, with regard to these four telescopic stars, this hypothesis is inadmissible, because, according to the theory of attraction, such an explosion would have necessarily given to these fragments unequal velocities of projection in starting from the same point, whence great unequal axes would have resulted, which is contrary to observation*.

It is known that Professor Brandés proved long ago, by corresponding observations made in different places and often repeated, that there are shooting stars which circulate with a velocity of 13 leagues, of 25 to a degree, in a second, at a height of 180 leagues above the surface of the earth †. It is also manifest, from the observations made in the United States compared by Professor Olmsted, that the centre from whence the meteoric shower of the 13th of November of 1833 set out, was elevated at a mean height of more than 800 leagues, and consequently that it was in a region which affords no aliment for combustion. The vivid lustre then which these meteors exhibit, and which they could not borrow from the sun, is their own inherent property. But as in our planetary system we know of no celestial circulating body which shines with its own light, this essential fact, which must necessarily be kept

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* *Traité Elémentaire d'Astronomie Physique*, 2nd edit., vol. iii. p. 42.

† These quantities, on the exactness of which we can rely, are the results of comparative observations begun in 1798 by MM. Benzenberg and Brandés, and continued on a greater scale, in 1823, by M. Brandés and his pupils, at Breslaw, Dresden, Leipe, Brieg, Gleiwitz, &c.—*Bibl. Univ.*, vol. li. p. 203; *Annuaire du Bureau des Longitudes de Paris* for 1836, p. 292.

in view, sufficiently shows the propriety, I would almost say the necessity, of considering shooting stars as a distinct class of phænomena.

In bringing together the different data furnished by observation, and in considering the particular circumstances connected with them, we may be led in some measure to conjecture that the source of this singular phænomenon is, perhaps, an electric focus, of which the determining cause is not yet known. But we must bear in mind that, in the region of hypothesis, and especially when we treat of a new subject as yet very little studied, analogy alone, whatever verisimilitude it may appear to possess, is not a basis sufficiently sure to found an opinion upon. I give this idea, therefore, only as a simple inference. It would, besides, be difficult to rank the shooting stars, which are seen unaccompanied with noise, in the catalogue of aerolites, whose fall, which often happens by day, is generally attended by a hissing in the air, by decrepitation, repeated detonations, and a smell more or less intense.

According to a communication made last year to the Academy of Sciences of Paris*, M. Millet Daubenton observed on the 13th of November, 1835, at about nine in the evening, the sky being serene, a luminous meteor, having the appearance of an incandescent globe, which exploded in the air and set on fire a barn covered with wood and thatch, near the chateau of Lauzières, in the department of Ain. M. Millet, according to his own account, is the only observer who saw the immense shower of fire that the meteor formed after bursting. This mere chance, which gave value to his observation, induced him to try if he could not find some stone of an unknown nature near the house and in the surrounding fields, and, indeed, he asserts that he picked up two of the size of a small egg. It is much to be regretted that the Academy after having begged M. Millet to send one of these specimens that they might ascertain its nature and make an analysis of it, has as yet kept silence respecting the examination of this meteoric product so interesting by its date.

Although thirty-seven years have passed since the 12th of November, 1799, the time when MM. Humboldt and Bonpland saw, at Cumana, a very unusual appearance of shooting stars which greatly excited their attention, our stock of knowledge respecting the cause and nature of this majestic phænomenon has remained very incomplete.

Without doubt we have yet to bring together many facts, to gather many observations, to arrange them, to discuss them,

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* See the *Comptes Rendus,* vol. i. p. 414.

in order to obtain a definitive solution of the problem. Recently, MM. Olmsted, Arago, Biot and other illustrious philosophers have been occupied with this interesting subject *. They have furnished ingenious ideas and fresh views, by which science will profit. It is true that some diversities are observable in the hypotheses which they have advanced, but these documents are not themselves the less to be prized and preserved. Who knows, but that the light, to the investigation of which every one earnestly applies, may not one day spring from the clashing of opinions?

II. *On the question whether Shooting Stars are more numerous at certain times than at others. By* M. QUETELET.†

M. Quetelet informed the Academy that during the night of the 12th of November he employed himself at the observatory of the city in noticing the shooting stars, for the purpose of ascertaining, whether in fact, their appearance were more frequent than at another season. His observations presented nothing remarkable as to the number of these meteors.

We remember that M. Arago, in giving to the Academy of Sciences at Paris an account of the results of the numerous observations which he had produced in support of this fact, quoted amongst other numbers as being extraordinary, that of 170 shooting stars which the students of astronomy in the observatory of Paris entrusted by him with making observations, had counted during the night of the 13th of November. For appreciating this number, however, and establishing a comparison, facts were wanting, that is to say, the knowledge of the mean number of those meteors which may be observed in a night at any other season of the year. For the purpose of determining this number, M. Quetelet entered upon some investigations relative both to his former observations on shooting stars, and to those of other persons, and he arrived at this result, that the number of shooting stars which are observed, on an average, in an hour, looking constantly towards the same quarter of the heavens, is about eight, and that several observers, placed so as to observe the different regions of the heavens, may count double the number. Accordingly, the number of 170 shooting stars observed at Paris by several persons on the night of the 12th of November would not be at all astonishing; on the contrary, it would come very near to

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* See the article on shooting stars by Professor Olmsted in the American Journal of Science, or the French translation in vol. iii. of the *Compilateur*, October 1836, page 52; the notices of MM. Arago and Biot in the *Comptes Rendus des Séances de l'Académie des Sciences de Paris*, vol. iii. pp. 560, 629, 663; a notice by Professor Gautier, in the *Bibl. Univ.*, vol. li. p. 189, &c.

† From *L'Institut*, and originally derived from the *Bulletin de l'Acad. Royale de Bruxelles*.

the average number of these meteors which may be observed on a winter's night.

This result of the inquiries of M. Quetelet is important enough for us to give it, supported by all the documents which establish it.

M. Quetelet, before making known the observations which he recorded in 1824, with several other persons, remarks, that in making these observations his object was not to record the number of shooting stars which may be counted in a given time, but merely to bring together the elements necessary for calculating the height, the velocity, and all that has relation to the path of these meteors; it follows, therefore, that the results which they furnish ought to be considered as an under estimate, since many stars were not recorded, because the elements which should have served for their calculation were not sufficiently exact. The same remark must also be applied to the observations made by Benzenberg and Brandés in 1798, the results of which will be given, as well as those made by this last philosopher in 1823, the results of which will also be given.

The observations by Benzenberg and Brandés in 1798 were made in the environs of Gœttingen. These two philosophers were at first alone, and placed at a distance of 27,050 French feet apart. But after three series of observations they felt the necessity of being further apart, and they placed themselves at the extremities of a base of 46,200 feet, and this time each of them took an assistant to note down under his dictation the observations, the results of which are brought together in the following table :

1798.	Shooting Stars observed by		Number of Hours.	
	Benzenberg.	Brandés.	Benzenberg.	Brandés.
Sept. 11.	9	11	2^h 0^m	2^h 19^m
—— 13.	6	8	1 7	1 36
Oct. 6.	11	13	2 8	2 24
—— 9.	14	63	2 46	8 12
—— 14.	33	123	7 46	7 47
Nov. 4.	62	49	6 34	5 35
Totals ..	135	267	22 21	27 53

Mean number for Benzenberg, about 6 stars an hour.
Brandés, about 10 stars an hour.
Mean number, 8 stars per hour.

The observations by Brandés in 1823 lasted two successive hours; they were made near the time of the new moons, and during the months of April, May, August, September, and October. The results are given in the following table :

Places of Observation.	Shooting Stars observed.	No. of Hours.	Mean No. per Hour.	Number of the Observers.
Breslaw …………	650	50	13·0	Brandés and his assistants.
Neisse ……………	307	30	10·2	Several observers.
Mirkan …………	65	8	8·1	One observer.
Gleiwitz …………	356	44	8·1	Two ———
Brieg ……………	144	20	7·2	One ———
Trebnitz …………	36	6	6·0	One ———
Cracow …………	43	8	5·4	One ———
Leipe ……………	36	8	4·5	One ———
Berlin …………	7	4	1·8	One ———
Brechelshof………	26	16	1·6	One ———
Dresden …………	40	26	1·6	Two ———

The following are the results of the observations which M. Quetelet made at Brussels in 1824 during ten evenings, together with those made at Liége by MM. Van Rees, and Plateau; and at Ghent by MM. Morren and Manderlier:

Places.	Shooting Stars.	Time.	Mean No. per Hour.
Brussels …………	155	10h. 26m.	15·0
Liége ……………	42	5 0	8·4
Ghent ……………	51	5 30	9·3

After this communication of M. Quetelet, M. Sauveur stated, that being on the road from Brussels to Liége in the night of the 8th of last August, he observed a considerable number of shooting stars, of which several were remarkable for their size and brilliancy.

M. Quetelet suggests that this epoch presents a singular agreement with that of the 10th of August, which the results of observations of shooting stars point out as one of those which are to be remarked for the abundance of meteors of this kind. (See on this subject Brandés's *Untersuchen über die Entfernung und Bahnen der Sternschuppen*; Leipzig, 1825; and Chladni's *Feuer-meteore*, p..89.)

Wishing to aid in throwing more light on this interesting and yet little known branch of meteorology the Academy has resolved to propose for 1837 a series of observations on shooting stars.

III. *On the Height, Motion, and Nature of Shooting Stars.*
By M. QUETELET.*

Shooting stars, those meteors so long neglected by philosophers, are beginning at last to engage their attention. We ask ourselves how it happens, that whilst measuring even to the minutest circumstances the motion of those heavenly bodies which are at the extremity of our solar system, and which, by their very distance, escape the attention of the many, greater thought should not have been bestowed on a more careful examination of the nature and cause of

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* From the *Annuaire de l'Observatoire de Bruxelles* for 1837.

the numerous appearances of these meteors, which, infinitely nearer to us, streak every night the surface of heaven, and are sometimes seen in such numbers that the heavenly vault would appear to resolve itself into a shower of stars.

Let us not, however, be in haste to suppose that nothing has been done upon this subject. We might almost be tempted to admit that the sciences also experience the influence and caprice of fashion, and that a certain class of researches can only interest at a certain epoch and under certain circumstances. Shooting stars, which had never been the object of an investigation expressly undertaken, were examined for the first time in a serious manner in 1798, by Benzenberg and Brandés, who examined them during many nights and at many intervals, with a view to determine their mean height, their velocity, and what belonged to the nature of their trajectory. In 1823, Brandés, seconded by a tolerable number of observers placed in different stations, resumed the same work. At nearly the same time (1824), I undertook, with the aid of from twelve to fifteen persons, similar observations, which were made at Brussels, Ghent, and Liége. I know not whether other regular observations of the same nature have been made since. Only, at my request, seconded by Sir John Herschel, the English scientific men assembled at Cambridge in 1834, thought proper to propose this subject of inquiry in the list of objects worthy to engage the attention of observers. The Royal Academy of Brussels has just come to a similar resolution.

Now, combining the results of the observations made in Germany and in Belgium, the following are the principal conclusions which may be deduced from them.

1. The *height* at which shooting stars appear varies within very wide limits; nevertheless the mean height may be considered as being from 15 to 20 leagues of about 20 to a degree, that is to say, near about the limits of our atmosphere. The two series of observations made in Germany gave:

SHOOTING STARS.

HEIGHT.	In 1798.	In 1823.	Total.
1 to 3 German Miles	1	4	5
3 to 6	2	15	17
6 to 10	3	22	25
10 to 15	6	35	41
15 to 20	2	13	15
20 to 30	2	11	13
30 to 40	1	3	4
45·7	...	1	1
60	...	1	1
100 and more.	...	1	1

2. Shooting stars have in general a direction *inclined* towards the surface of the earth. Of 36 computed trajectories, Brandés found 26 descending ones, 9 ascending, and 1 horizontal; 13 formed an

angle of less than 45° with the vertical; 14 were between 45° and the horizon; 8 between the horizon and 135°; and only 1 was still more elevated. With regard to the azimuths, of 34 trajectories, 23 had a direction southwards and 11 to the north, 21 to the west, and 13 to the east. Separating the shooting stars into two groups, we find 25 of them whose course inclines more to a southwest direction, or whose azimuth is less than 135° to the west and 45° to the east, and only 9 are in the other half portion of the heavens. This difference seems to be connected with the direction of the motion of the earth in its orbit, admitting that the meteors in question may be considered as small asteroids.

3. The *brilliancy* of shooting stars is very various; these meteors sometimes surpass Jupiter and Venus in light, and sometimes they are only perceived by the help of finders. Some leave after them luminous tracks visible for some seconds after their passage, which are not to be confounded with those luminous and rapid traces which depend upon the length of the sensation on the retina. The trajectories appear generally as straight lines. Some of them, however, are very sensibly curved; they are far from exhibiting a continued brilliancy in their whole extent.

4. The *velocity* of shooting stars has not been capable of determination with any precision, except for a very small number of these meteors*: it is from 3 to 10 leagues a second.

5. As to the *mean number* of shooting stars which can be observed at any given epoch of the year, after having particularly examined this question,† (*Bulletin de l'Acad. Royale de Bruxelles*, vol. iii. p. 404, *et seq.*) I have come to this result, that a single observer or several observers directed towards one and the same region of the heavens can see, on an average, eight shooting stars an hour, and that several observers, placed so as to see the different regions of the heavens, may reckon twice that number of them.

6. It would seem that a cause exists which produces, from about the 8th to the 15th of November, more frequent appearances of shooting stars. I have also thought that I remarked a greater frequency of these meteors in the month of August (from the 8th to the 15th).

7. As to the *nature* of shooting stars many doubts still remain on this subject: are they to be considered as asteroids, according to an hypothesis of some standing; or as stones shot from the volcanos of the moon, according to the opinion of Benzenberg, Chladni, and other physicists‡? I should be inclined to think that a distinction must be made between the shooting stars which leave luminous trains after them, persistent and often characterized by sparks, and those whose course is marked by a trace of light as momentary as the appearance of the star, and which is only owing to the duration of the impressions on the retina. The first appear to me to be really

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* For six of these meteors whose velocity I was able to calculate, I found 5 leagues, 7·6, 4·5, 3·0, and 3·4, mean 4·7 leagues.

† See the preceding paper. Edit.

‡ *Die Sternschuppen sind Steine aus den Mondvulkanen, Benzenberg, Bonn,* 1834.

bodies foreign to our earth. The 31st July 1824, I observed an aerolite, or luminous mass, which presented very remarkable circumstances, left sparks on its passage, and must have fallen in the neighbourhood of Antwerp.

[The principal authorities and sources of information on the interesting subjects of shooting stars and the alleged periodicity of certain appearances of them, have been cited in the preceding papers of MM. Wartmann and Quetelet; but we may usefully add a few references in detail to the observations and researches of the physicists in the United States, who have been the first to call attention to the apparent periodical previous appearance and return of the brilliant shower of meteors witnessed in November, 1833.

At present, the recurrence of the *shower of meteors*, as it has been termed, is asserted to have taken place about the end of the second week in November, in the years 1799, 1831, 1832, 1833, 1834, 1835, and 1836. On the other hand, many observers deny that any remarkable or unusual phænomenon of the kind was seen, except in the years 1799 and 1833, affirming that a greater number of shooting stars was observed in the other years (especially in those subsequent to 1833), merely because the attention of observers was specially directed to them at a certain time. The following papers and notices, among others, have appeared in late volumes of Professor Silliman's American Journal of Science and Arts. *On the Meteors of Nov.* 13, 1833; by Prof. E. Hitchcock: vol. xxv. p. 354. On the same subject, and on the Meteors of Nov. 13, 1834, and of Nov. 1835; by Prof. D. Olmsted: vol. xxv. p. 363; vol. xxvi. p. 132; vol. xxix. p. 168, 377; vol. xxx. p. 370. *Investigations respecting the Meteors of Nov.* 13*th*, 1833, &c.; by A. C. Twining: vol. xxvi. p. 320. *Papers*, by Prof. A. D. Bache, *denying that any recurrence of the phænomenon took place in November*, 1834: vol. xxviii; and vol. xxix. p. 383. *An Observation* in Nov. 1833; vol. xxvi. p. 397. *An Observation* in Nov., 1835; vol. xxix, p. 390. *Letter from the* Rev. W. B. Clarke, *denying the alleged periodicity*; vol. xxx. p. 369. A more recent paper by Prof. Olmsted, has been reprinted by Prof. Jameson, in his New Edinburgh Phil. Journal for July last. Prof. Olmsted's first paper contains an extensive collection of observations of the Meteors of Nov. 13, 1833, made in different parts of the United States, and gives various inductions from them. In it also is proposed a theory to explain the phænomena, which is thus finally expressed:—" *That the Meteors of Nov.* 13*th consisted of portions of the extreme parts of a nebulous body, which revolves around the sun in an orbit interior to that of the earth, but little inclined to the plane of the ecliptic, having its aphelion near to the earth's path, and having a periodic time of* 182 *days nearly.*"—Silliman's Journal, vol. xxvi. p. 172.

Some observations on shooting stars, made at Devonport in November, 1836, have been noticed in our last volume, p. 234. Observations of the same period, made at Berlin, Breslau, Frankfort-on-the-Maine, and Gummersbach, will be found in Poggendorf's *Annalen*, vol. xxxix. p. 353—356.

The frequent appearance of shooting stars in August had been noticed in England by Dr. T. Forster, (see Phil. Mag., First Series, vol. lxiv. p. 294), and at Pavia, we believe, by M. Bellani.—E. W. B.]

XXXIII. *Researches into the Cause of Voltaic Electricity.* *By* Mons. AUGUSTE. DE LA RIVE, *Rector and Professor of Natural Philosophy of the Academy of Geneva, Corresponding Member of the Academy of Sciences of Paris.*[*]

Abstract.

AT the commencement of 1828[†], in a memoir entitled, *Analysis of the circumstances which determine the direction and intensity of the electric currents in a voltaic pair*, I showed by several experiments the impossibility of explaining the production of voltaic electricity by the theory of contact, and the necessity for having recourse to chemical action. At the end of the same year I published an abstract of the researches which form the subject of this article, in the *Annales de Chimie et de Physique*[‡], which appeared in three parts in the memoirs of the *Société de Physique et d'Histoire Naturelle* of Geneva, in 1829, 1832, and 1835. These three parts, especially the last, were enriched with new facts not contained in the abstract which I had given in 1828; but the principles were then stated, and the new details that I added have only had the effect of proving their exactitude. Thus in 1828, that is long before the labours of Messrs. Ohm, Fechner, and Faraday, I had already given the theory of the pile, founding it upon principles which have been confirmed by the important researches of the physicists whom I have just mentioned. But I ought to acknowledge that these researches, particularly those of Mr. Faraday, have led not only to the discovery of new laws of great value, but in addition, have given, by the discovery of those laws, a more solid basis to the chemical theory of the pile than was afforded by my own labours. As my memoir forms a volume of 170 pages, and consequently cannot be inserted entire in a scientific journal, I have endeavoured to give an abstract of it, as complete as is possible, for the *Philosophical Magazine*, and I have taken advantage of this circumstance to add to it a few facts which I have not hitherto had an opportunity of publishing.

EXPLANATION OF THE PRINCIPLES UPON WHICH THE CHEMICAL THEORY OF VOLTAIC ELECTRICITY IS, IN PART, FOUNDED.

PRINCIPLE I.—*No electricity is developed by two bodies in contact when they do not undergo any chemical action, provided that there is likewise an absence of calorific and mechanical action.*

This principle, in support of which I produced a great num-

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[*] Communicated by the Author.

[†] *Annales de Chimie et de Physique*, vol. xxxvii. p. 225.

[‡] *Ibid.*, vol. xxxix. p. 297.

ber of facts in 1828, has since that period been the subject of a warm controversy. It has been attacked by many physicists upon two grounds: 1. That electricity is produced by the simple contact of bodies; 2. That a good theory of the pile cannot be given without admitting Volta's electromotive force. A reply to the second objection is unsuited to a portion of the memoir devoted to the chemical theory of the pile; I shall here therefore notice only the first, and endeavour to show the exactitude of the principle which I have stated above.

The reason that it has been, and is still often supposed that the simple contact of two heterogeneous bodies develops electricity, is the difficulty of securing them from all calorific and mechanical, and especially from all chemical action. Are the bodies placed in a liquid? When the liquid does not attack them the air dissolved in the liquid oxidates them if they are metals, or the water of the solution forms hydrates with them if they are oxides. Are they left in the air, or placed in some other gas? It is soon evident from the alteration of their surfaces that they are attacked by the air or the gas; the finger or the humid substance with which they are touched is also often the cause of chemical alteration at the points of contact. Indeed nothing is more difficult than to secure the absence of chemical action in these experiments. And when we consider on the one hand how feeble is the electricity produced, since to perceive it the assistance of the most delicate apparatus is required; and on the other what an immense quantity of electricity may be developed by the most feeble chemical action, as has been proved by the experiments of Mr. Faraday; we ought no longer to be surprised that excessively feeble chemical action, the traces of which cannot be detected but at the end of a certain time, may give sensible signs of electricity.

But it is not impossible to exclude all chemical action, and then no disengagement of electricity is perceptible even with the most delicate apparatus. Thus pairs of platina and gold, or of platina and rhodium, in extremely pure nitric acid, do not give any current to the most sensible galvanometers; the same thing occurs with platina and palladium in diluted sulphuric acid. In order to the success of these experiments it is necessary to employ only highly purified substances, for one drop of hydrochloric acid in the nitric, or of nitric acid in the sulphuric, immediately renders the pairs active. From experiments of this kind I shall produce one which appears to be possessed of some interest. Two plates of perfectly polished steel were immersed in a flask filled with a solution of potash; one was insulated, and the other metallically fixed by its extremity to a plate of platina immersed in the same liquid; the two steel plates were inserted in the cork stopper belonging to the flask in such a manner that the upper extremity of each passed through the cork into the air. The portion of the two plates immersed in the liquid remained perfectly intact; at the end of three years their surfaces had not lost any degree of polish; and in this respect there was no difference between them, notwithstanding that one of them, in consequence of its contact with platina, ought, according to the theory of Volta, to have become oxidated, and that so much the more readily as the solution of potash is a conductor of electricity; but the part of the plate which was inserted in the cork, and passed through it into the air, was covered at the end of three years with a very thick coat of oxide; the insulated plate was also oxidated at the same points, but in a much less degree, and it is worthy of notice that the exterior of this plate presented a globule of oxide of iron of the size of a pea, exactly similar in miniature to those globules which are formed in iron tubes used for the conveyance of water. From the preceding it follows that oxidation must have commenced in order to the existence of an electric current; the current produced by this oxidation decomposes water, and in consequence determines a stronger oxidation upon the steel plate in communication with the platina, and this oxidation, in its turn, increases the energy of the current; in this experiment, the water, which, produced by evaporation, moistened the cord, being mixed with a great deal of air, performed at the same time the office of the exciting body and the conductor.

I have indicated the causes which may determine a chemical action when two heterogeneous bodies are placed in contact without making use of a liquid, and I have ascertained that if their occurrence be prevented no electric effect is obtained. To avoid these sources of error Messrs. Pfaff and Becquerel employed a condenser of which one of the plates was of copper and the other of zinc, between which a metallic communication was established by means of an insulated arc of copper. The first of these physicists placed the two plates of his condenser in a vacuum in hydrogen, or in azote carefully dried; the second gilt the copper plate, and covered the plate of zinc with a thin coating of lac-varnish; but notwithstanding these precautions they obtained a development of electricity by the contact of the two metals. Can it be said that there was no chemical action in their experiments? It may easily be proved that in the medium in which M. Pfaff placed his plates sufficient atmospheric air always enters to produce a slight oxidation of the surface of the zinc; and that in M. Becquerel's experiments the coating of varnish was too thin to prevent this oxidation, which took place through the pores that the alcohol produced by evaporating. In short, if a plate of zinc well brightened [*decapé*] be employed, it will be seen that whether M. Pfaff's method, or that of M. Becquerel be pursued, it will become tarnished at the end of a certain time; proving that there has been an oxidating action of the air, and the formation of a suboxide. The only means by which I have succeeded in preventing all electrical signs from the contact of the two plates is, by covering the plate of zinc with a layer of lac-varnish, so thick as not to be transparent; in which case the air no longer gains access to the zinc, and consequently cannot oxidate it.

Sufficient attention has not in general been paid to the formation of these coatings of suboxide upon the surface of most of the metals, the existence of which may be easily proved by comparing the parts of a metallic surface recently brightened [*decapé*] with one which has been for some time exposed to the air. This formation is often very rapid, which is the reason that much more distinct electric signs are obtained by touching an electroscope with metallic surfaces at the very moment after they have been brightened [*decapé*]; this is more especially true when, insulating these surfaces by employing a glass handle instead of the fingers, the oxidating action of the air is the only chemical action to which they are exposed. I have also proved the rapidity with which these coatings of suboxide are formed, in studying the nature of the electricity which is developed by the well-brightened [*decapé*] surfaces of most of the metals when they are rubbed with wood, cork, or a dry finger, or any conducting body whatever, not metallic. If these surfaces be rubbed the very moment after they are brightened [*decapé*] the electricity acquired by the metal is always negative; but if the rubbing be deferred for a longer or shorter time, according to the nature of the metal, the electricity, even in dry air, is positive, excepting with the metals which are not oxidable, or which are only so in a very small degree. The cause of this alteration is, that in the second case a coating of suboxide has had time to form; it is removed by the substance with which it is rubbed, to which it adheres, and the friction then occurs between the metal and its oxide; on which account the first is positive. Lastly, I shall cite the following from the facts which I have collected to prove this rapid oxidation of the surface of most metals: If a metallic plate (not of platina), which after having been brightened [*decapé*] has remained for some time exposed to the air, be fixed to the negative pole of a pile, and a plate of platina to the positive pole, and the two plates be immersed in a solution of sulphuric acid, the oxygen always appears upon the positive plate a few seconds before the hydrogen is seen upon the negative; a fact which can only be explained by admitting that the first quantities of hydrogen disengaged have been employed in deoxidating the negative metal: it is not necessary that this metal be very oxidable; either silver or copper will answer; all that is necessary is that the pile be feeble, and the solution a good conductor.

The experiment which is considered as the most favourable to the theory of contact is that of putting in contact platina and peroxide of manganese, when the former of those substances is found to be positive and the latter negative. Other peroxides, such as that of lead, will give the same results. By putting a thin plate of wood, instead of one of platina, upon the plate of the condenser, and by placing on the wood the peroxide which is held between the fingers, the condenser of positive electricity is charged. If a piece of paper steeped in pure water, or what is still better, in acidulated water, be substituted for the wooden plate, and the peroxide be touched with a piece of wood or with a dry finger, the condenser of negative electricity is charged. Thus the development of electricity is not due to the contact, but to the slight deoxidation that the peroxides, such as those of manganese and lead, undergo by contact with humid bodies, or to the foundation of hydrates due to the same circumstance. In the same manner may be explained the slight instantaneous current which M. Becquerel obtained by immersing peroxide of manganese and platina in pure water. This current only occurs when the circuit has been interrupted for fifteen or thirty minutes, in order to allow of the accumulation of electricity due to the slight chemical action; which accumulation, as we shall presently see, happens in this instance alone, in consequence of the imperfect conductibility of the elements of the pair. The same phænomenon occurs with gold and platina, and arises from the slight oxidation of the gold effected by the operation of the air dissolved in the water. It is true it is very feeble, but if allowed to accumulate for about half an hour it will be capable of producing an instantaneous current perceptible with a very sensible galvanometer.

Principle II.—*All chemical action disengages electricity; but the electricity disengaged is not, in every case, nor under every form proportional to the vivacity of the chemical action. Two principal circumstances may explain this anomaly; viz. the immediate recomposition in a larger or smaller proportion of the two electricities at the points at which they are separated* *by chemical action; and the particular nature of this action, which, according to the bodies between which it is exerted, gives rise to electric effects more or less intense.*

When there is chemical action between two bodies, one takes possession of the positive electricity, (these are in general *oxygen*, *oxides*, &c.) the other of the negative electricity, (these are in general the *bases*, *metals*, *oxides*, &c.). Each of these bodies is in a state of electric tension, and it may be rendered sensible by putting one of them in communication with an electroscope, and the other with the earth. There is an electric current when the two bodies are united exteriorly at the points at which the chemical action takes place, and it may be rendered sensible by employing the metallic wire of a galvanometer to effect this union. Thus when a communication is established between the condenser of an electroscope and an oxidable metal, and it is touched with the finger or any other humid conductor, the metal takes the positive electricity, which it transmits to the condenser, and the humid conductor the negative, which passes into the earth. In the particular case in which the condenser is formed of one plate of zinc and one of copper, the negative electricity which the zinc acquires by the chemical action exerted upon its surface by the air or some other gas, passes in part into the copper with which the zinc communicates metallically; the positive electricity with which the layer of air adhering to the surface of the zinc is charged passes into that metal, or, after having neutralized what remains of the negative, charges it positively; by means of condensation a sufficient quantity of positive electricity is accumulated upon the surface of the zinc and of negative upon the copper to admit of their presence being easily perceived by the electroscope; it may also be seen, as experiment has constantly proved, why the electric tension of each of the plates is the moiety of what it would be if one of them were put in communication with the earth and the other with the electroscope. In the same manner when two metals communicating with each other metallically are immersed in a liquid which exerts a chemical action upon one of them, and not upon the other, the positive electricity with which the liquid becomes charged, and the negative which is taken by the attacked metal, unite by the intervention of the metal not attacked, and the conductor which unites the two metals, constituting the electric current which it is said leaves the metal attacked in a state which is called positive in relation to the other. It is not necessary that the second metal should not be attacked; it is sufficient that it be attacked in a less degree than the other in order to the establishment of the current, in consequence of the difference in the quantities of electricity developed by the two unequal chemical actions.

But, it will be said, if such be the fact, how is it that a stronger tension is not obtained by putting an oxidable metal in communication with the electroscope and then immersing it by its extremity in an acid solution instead of holding it in the fingers? How is it that the strongest electric currents are not always produced by the most lively chemical actions, and that a metal immersed in a liquid which attacks it but feebly, forming a pair with another metal, or a piece of the same metal immersed in a liquid which acts upon it strongly, can be sometimes positive, that is to say, that whence the current commences? To explain these anomalies and reconcile them with the chemical theory, recourse must be had to a principle which indeed flows from the nature of things, viz. *the immediate recomposition in a larger or smaller proportion of the two electricities developed by chemical action.* It is necessary then carefully to distinguish the electricity *perceived* from the electricity *produced*; the latter must evidently be proportional to the extent of the chemical action, that is, that in a given time it depends upon the number of chemical atoms which are combined, and consequently upon all the other circumstances which may have exerted an influence upon the number of these combinations (the extent of the surface exposed to chemical action, the vivacity of that action, &c.). The electricity perceived is a proportion of the electricity produced, a proportion which depends upon the relative conductibility of the bodies entering into the system in which the electricity is propagated, upon the disposition of the different parts of the system and upon the nature of the apparatus to be employed in showing the presence of the electricity, &c.; circumstances which, as we shall see, all have an influence upon the degree of facility with which the two electric principles follow some certain course, or become again immediately united to the same surface from which by chemical action they are separated.

When a capsule of platina filled with sulphuric or diluted nitric acid is placed upon the plate of a condenser, and a plate of zinc, held in the fingers, is immersed in it, a very feeble charge is given to the plate of the condenser, although the chemical action may have been very lively; the reason is, not that there has not been an enormous disengagement of electricity, a fact which may be proved by employing this electricity in producing a current; but in this experiment the negative electricity developed in the zinc unites with the positive with much greater facility than it can pass through the fingers and the body of the experimenter in order to lose itself in the earth;

there will therefore only be a very feeble positive tension, often scarcely any. But if the diluted acid be replaced by concentrated sulphuric acid, though the chemical action will be less lively, the electric tension will be much stronger, this acid being a very bad conductor; and the passage of the electricity from the liquid to the metal immersed in it being extremely difficult, the two electricities uniting immediately upon the surface attacked in a much smaller proportion: if, instead of a piece of metal, a piece of wood rather moist be immersed in the concentrated sulphuric acid, the positive tension acquired by the acid is still stronger. If a capsule made of an oxidable metal be employed, and, after heating it, a few drops of a liquid capable of attacking it at that high temperature in ever so small a degree (pure water is sufficient), be poured into it, a quantity of negative electricity is developed, which is sufficiently strong to be sensible without the assistance of the condenser, and even to give sparks. In this case the drop of liquid injected into the heated capsule is converted into vapour while it is attacking the metal, and carries off with it the positive electricity, which cannot then combine immediately with the negative electricity left in the metal; but if even the smallest quantity of the liquid remains in the capsule unvaporized the immediate recomposition takes place, and only very feeble traces of negative electricity can be obtained. If the electricity developed by the action of a gas, or by that exerted by a humid body, such as the hand or a piece of wood, upon the metal with which it is in contact, is often much stronger than the electricity resulting from the much livelier action of a liquid, the reason is, that in the former case the immediate recomposition of the two electrical principles is almost null, in consequence of the imperfect conductibility of the exciting bodies, and that the electricity produced is almost entirely perceived. There is, however, a slight recomposition, for the negative tension of an insulated metal is sensibly augmented by giving a translatory motion to the gas which attacks its surface; the consequence of which is, that the positive electricity accumulated in the gas being removed with it, cannot unite with the negative left in the metal*.

The principle of immediate recomposition of the two elec-

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* The most successful method of performing experiments of this kind is, to take tubes of zinc, copper, or any other metal susceptible of attack, to suspend them to an insulating handle, and cause a current of very dry air mixed with a little chlorine to pass interiorly. If a communication be established between the metallic tube and an electroscope, it will be found charged with strong positive electricity. The positive electricity carried off by the current of gas may also be collected by causing the current to pass through a tube of platina after it has passed through the metallic tube which it has attacked.

tricities, applies also to the production of electric currents in a pair. In very lively chemical actions the larger proportion of the two electricities developed often undergoes this recomposition; a small part only runs through the whole circuit, especially if it be not a very good conductor; which is the reason that the strongest currents are not always those produced by the most lively chemical actions, and that in a pair the metal the most attacked is not always the positive one, that is, the one whence the current commences. However, the latter case occurs only when each of the two metals of the pair are immersed in different liquids; a single example shall be given. A plate of zinc is placed in concentrated sulphuric acid, and a plate of copper is immersed in nitric acid; the two acids are immediately in contact, and the two metallic plates communicate by means of the wire of a galvanometer. In this pair the zinc is positive though it be much less attacked than the copper; because the two electricities developed by the action of the sulphuric acid upon the zinc can be more easily reunited by making the tour of the circuit, than by passing from the sulphuric acid to the zinc, and reciprocally; while, on the contrary, the two electricities developed by the action of the nitric acid upon the copper reunite immediately with the greatest facility, in consequence of the conductibility of the nitric acid, and the ready passage of the electric current from that acid to the copper; while to make the circuit they would be obliged to traverse the concentrated sulphuric acid, which is a very imperfect conductor, and pass from the zinc to the acid,—a very difficult passage. Two circumstances prove the exactitude of this explanation: 1. The same result is obtained in the preceding experiment by substituting a plate of zinc, similar to that which is immersed in the sulphuric acid, for the plate of copper immersed in the nitric acid. 2. If a capsule of platina be put upon the plate of the condenser, and filled in succession with nitric acid, and concentrated sulphuric acid, and a plate of copper or of zinc, held between the fingers, be immersed in the first-mentioned liquid, and a plate of zinc in the second, a much stronger positive electricity is obtained in the second case than in the first.

The foreign substances which are often mixed with metals greatly facilitate this immediate recomposition, by giving rise to local actions. Thus, as I have already shown,* the lively action of diluted sulphuric acid upon common zinc is due to a small proportion of iron contained in the zinc, and to the electrical currents which, taking place upon its surface, pass

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* *Bibl. Univer.*, April 1830. [See also Phil. Mag. and Annals, N. S. vol. viii, p. 298. Edit.]

from the particles of zinc to the particles of iron. These currents decompose water; the hydrogen is disengaged, and the oxygen, by combining with the zinc, increases the energy of the current, which thus successively becomes cause and effect; and the action may be observed to go on constantly increasing in intensity, as if it were the effect of an accelerating force, and neither to be constant or diminished but when the surface of the metal becomes covered with a little suboxide black arising from the decomposition of the sulphate of zinc by the local currents. If, instead of common zinc, distilled, and consequently very pure zinc be employed, a very feeble chemical action is obtained by immersing it in diluted sulphuric acid; but if a pair be formed by uniting it metallically with platina immersed in the same liquid, the intensity of the chemical action which takes place upon its surface is augmented; the disengagement of the hydrogen and the oxidation of the zinc become more and more energetic in consequence of the current which is established from the zinc to the platina, and which here also is successively effect and cause. The limit to the augmentation of intensity is only the effect of the constantly increasing resistance experienced by the current in the circuit, in proportion to its increasing strength; the resistance arises from the imperfect conductibility of the parts of which the circuit is composed, and the deposit of suboxide of zinc which takes place, not upon the metal itself, as in the preceding case, but upon the platina. The only difference which exists between the effects resulting from the employment of distilled zinc when united to platina, and of the zinc of commerce is, that in the first case the currents traversing a longer circuit may be perceived, while in the second, being almost molecular, they cannot be apprehended; but they exist equally in both cases. It will now be easily understood why, in the same circumstances, a plate of common zinc with another metal forms a much less powerful pair than a plate of distilled zinc, though it produces a much livelier chemical action; it may also be understood why the latter zinc is negative when it forms a pair with distilled zinc which is less attacked than itself. Very great advantage will therefore be found to result from the employment of distilled zinc in the construction of voltaic piles instead of common zinc; for with the former nearly all the chemical action can be utilized, since it is in its totality cause and effect; whilst with common zinc the greater part of it is lost by immediate recomposition; the proportion *utilized* is therefore but a small fraction of the *total* quantity of zinc dissolved.

Since I made known the properties of distilled zinc, Mr.

Faraday has remarked that they are also possessed by amalgamated zinc: it appears from the observations of Professor Daniel that the amalgamated zinc is considerably attacked the moment it is immersed in acidulated water, but that the hydrogen which results from this action prevents the continuation of it, by remaining strongly attached to the mercury of the amalgam; but that if a communication be established between the amalgamated zinc and a plate of platina or copper immersed in the same liquid, the current which results from the action upon the zinc removes the hydrogen to the platina instead of leaving it upon the mercury, and the surface of the zinc being no longer defended can be oxidated in the same manner as if it were not amalgamated.

The principle of the immediate recomposition of the two electricities which has just been explained so far as it is connected with the production of electricity by chemical action, applies to every other mode of developing that agent, such as *heat*, *friction*, or *pressure*. In every case the *total* quantity, not of electricity produced, but the quantity *perceived*, or what may be called the *tension* of the source, essentially depends upon the greater or less degree of facility with which the two electricities can be immediately recomposed at the same points at which they are separated; this facility varying according to the nature of the substances employed.

Thus, according to what precedes, the *quantity* of electricity produced in a chemical action will depend solely upon the number of the atomic combinations which take place during the continuance of that action, as has been so well shown by Mr. Faraday; but the *intensity of the current* will depend upon the proportion of the two electricities produced which passes, in order to its neutralization, through the conductor where this intensity is measured; so also the *energy of the tension* will depend upon the degree of difficulty experienced by the two electricities, separated by chemical action, in reuniting immediately. However, it is not probable that this is the only circumstance having an influence upon the intensity of the current and the tension. Some experiments incline me to think that the very nature of chemical action exerts an influence in this respect: we will suppose, for example, that all the electricity produced by the oxidation of a certain number of atoms of zinc, and all that resulting from the oxidation of the same number of atoms of copper, pass, in the same time, through the same conductor; the first would produce much more intense dynamic effects than the second. It would appear, indeed, according to some researches which I published in the *Annales de Chimie et de Physique*, in January

1836, that the currents produced by the combinations of the same number of atoms effected in the same period of time, would be so much the more intense in proportion to the increase of strength of the chemical affinity which unites the atoms of each combination.

I shall not here enlarge upon this point, which, considering its importance, deserves to be separately discussed, and to which I hope to return immediately, adding further developments upon the subject to the brief statement which I published in the *Annales de Chimie.* The theory of the pile is, however, independent of this question; all its importance in relation to that theory being that it proves: 1. That there is no development of electricity by the simple contact of two heterogeneous bodies. 2. That chemical action produces electricity according to the laws that have been indicated. 3. That the quantity of electricity developed in a given time depends upon the number of combinations which take place in that time. 4. That the *tension* of an electric source and the *intensity* of a current depend upon the proportion of the electricity produced, which, under a statical or a dynamical form, reaches the instrument by which the tension or the intensity is measured.

Part II.—*Theory of the Voltaic Pile.*

If all the bodies through which the electric current is passed were sufficiently good conductors to admit of the two electricities, which have been separated by the chemical action exerted upon one of the metals of a pair, following, in totality, in order to reunite, the direction presented to them by those bodies, instead of being neutralized in a larger or smaller proportion upon the surface where they have been developed, voltaic piles would be unnecessary; a single pair would produce every effect, with an intensity corresponding to the extent of the surface. But this is far from being the fact; excepting a few electro-magnetic phænomena, the greater part of the effects of voltaic electricity can only be produced by making the electric current pass through conductors more or less imperfect; wherefore, if a single pair be employed the result is, that, as with common zinc, there is a local action which gives no perceptible current; or, as with distilled zinc, there is scarcely any effect or none at all, because no current can be established through the circuit. The use of the pile is therefore to facilitate the passage of the current through imperfect conductors, and not to increase the quantity of electricity; for the utmost that can be effected by a pile composed of a certain number of similar pairs is to compel all the electricity produced by only one of its pairs to pass through the conducting body which connects its poles. The only means of attaining this object is to separate the two metals of a pair by other pairs as similar to the first as possible; these intermediate pairs, the number of which should correspond to the more or less imperfect conductibility of the bodies interposed, will each produce as much electricity as the extreme pair, but these electricities do not pass through the conductor; they only compel the electricities of the extreme pair to pass through it almost in totality. We shall now endeavour to show how this effect is produced. We shall take a pile in activity; and suppose that all the pairs of which it is composed are so exactly similar to each other, in every respect, that the free electricity upon each of them has the same intensity. Let *b* be a pair in the pile, taken at hazard, and disposed in such a manner that its *zinc* is immersed in the same liquid as the *copper* of the pair *a* which precedes it, and its *copper* in the same liquid as the *zinc* of the pair *c* which follows it. The chemical action of the liquid upon the zinc of the pair *b* develops in it a certain quantity of electricity; the portion of this electricity which does not undergo immediate recomposition remains free, and the same for all the pairs, they being similar and symmetrically disposed with relation to each other. According to this the positive electricity of *b*, developed by chemical action, in the liquid in which the copper of *a* is immersed, neutralizes the negative electricity of this latter pair, which is equal to it. In the same manner the negative electricity of *b*, which by chemical action is carried to the zinc, and thence to the copper in contact with the zinc, neutralizes the positive electricity of *c*, which also is perfectly equal to it. There remains then an excess of free positive electricity in the liquid in which the zinc of *a* is immersed, and an excess of free negative electricity perfectly equal upon the copper of *c*. But these free excesses are neutralized by the equal and opposite electricities of the following pairs, with regard to which we may reason in the same manner as for the pairs *a*, *b*, and *c*. Thence there results an excess of free positive electricity at the extremity of the pile at the side of *a*, and an exactly equal excess of negative electricity at the extremity situated at the side of *b*. Such is found to be the fact if a communication be established between each of the extremities and an electroscope; and if they be united by a conductor, the two excesses of free electricity are collected together and form the current. The intensity of this current, as experiment has proved, ought to be perfectly equal to that of the current which is established in the pile itself, between all the pairs.

It most frequently happens that the quantity of free electricity disengaged upon each pair of a pile is not mathematically the same. The difference may arise from one of these three causes: either that the pairs have not exactly equal surfaces, that the metals of which they are formed are not perfectly similar, or that the liquids in which they are immersed are not completely identical; circumstances which all have an influence upon the total quantity of electricity produced in a given time, and upon the portion of that electricity which remains free. However, experience teaches, that when the two poles of a pile are united by a conductor, the current which traverses this conductor is still mathematically equal to that which traverses each of the pairs of the pile. To establish this important result, instead of soldering the zinc and copper of the same pair to each other, an independent conductor must be fixed to each. By means of these two conductors a metallic communication is established between the two metals of the pair, by the intervention of one of the wires of a double galvanometer, the second wire of which serves as conductor to the current of a second pair of the same pile, or to effect a communication between the two poles. If these two currents be carefully made to pass in contrary directions in each of the wires of the galvanometer, their action upon the needle will always be found absolutely null, provided they are mathematically equal. This equality is easily explained. Take the most feeble pair in the pile; let *b* be this pair; the positive electricity disengaged by *b* cannot neutralize all the negative of *a*; there will remain then in the copper of *a* an excess of negative electricity which will retain, by neutralizing it, an equal quantity of positive; the result will be that *a*, though much stronger than *b*, can only set at liberty a quantity of positive electricity equal to that of *b*. So also the negative electricity of *b* can neutralize only a part of the positive of *c*; the rest of this positive electricity will neutralize an equal part of the negative of the same pair; and consequently *c* also can only liberate a quantity of negative electricity equal to that of *b*. It appears from this analysis, that the current of each pair, and consequently the current of the whole pile, should be equal to the current produced by the weakest pair. Now, experiment fully proves, that if a feeble pair be introduced into a pile composed of energetic pairs, the immediate result is a considerable diminution in the force of the current of the pile, and consequently of the current of each of the other pairs; but this reduction is never sufficient to render this current equal to that which would be developed by the pair introduced in an isolated state. Indeed, any pair whatever necessarily produces a greater quantity of electricity when it is in the circuit than when it is isolated; in the latter case the two electricities have a tendency to reunite immediately in a larger proportion than when the pair is placed between two others, one of which takes possession of its positive electricity and the other of its negative: in addition, the current which is established in the liquid in which the oxidable element of the pair is immersed, facilitates the oxidation of that element by decomposing the liquid, and consequently augments the quantity of electricity developed. This is particularly the case when piles are employed consisting of pairs of copper and distilled or amalgamated zinc; the current itself is at once cause and effect. Sometimes the pair introduced is so feeble that it can only be considered as an imperfect conductor interposed in the liquid which unites the opposite elements of the two pairs between which it is placed; its influence then consists solely in diminishing the quantity of the opposite electricities of these pairs which would neutralize each other, and consequently the current of all the other pairs and that of the whole pile. If the diminution of effect be due to the cause which has just been stated, it ought to be the same whatever be the direction in which the elements of the pair introduced are disposed; and I have had opportunity of verifying that such is the fact.

We shall now inquire whether the theory which has just been enunciated will explain all the phænomena presented by voltaic piles; and to this end we shall investigate in succession their tensile and their dynamic effects.

Tensile effects.—When a pile is insulated and its two poles are not united by a conductor, an equal quantity of positive and negative electricity should, in agreement with what we have just seen, be found at each of them. These two electricities, constantly carried towards a pole, would in the end acquire an enormous tension if the pile itself which separates them were not a more or less perfect conductor, which allows them in part to reunite in neutralizing each other; it acts in this respect as the band of moist paper placed by Volta between the two poles of the pile, or as a secondary pile placed in the same position. Commencing at each of the poles, an electricity should therefore be found in the pile of the same nature as that of the pole, but constantly diminishing towards the middle where it is null, because that is the point of meeting and consequently the place of neutralization of the two electricities; and this is precisely what experiment demonstrates. The recomposition of the electricities of the two poles, which takes place through the pile itself, is proved by the following fact, that when one of the poles is put in communication with the earth, the other requires a much stronger tension; but the difference between the tension in the two cases increases in proportion as the liquid with which the pile is charged is a better conductor; such should be the fact, for when the liquid is a good conductor the two electricities can reunite with greater facility through the pile, if one of them be not furnished with means of flowing into the earth. Numerous experiments have been made in relation to this circumstance by charging the same pile in succession with river water (Rhone water), a solution of sulphate of soda, and a diluted solution of nitric acid. I have observed, in particular, that to obtain the maximum tension the pole where the tension is perceived must be left in contact with the condenser for a considerable time, if the liquid with which the pile is charged be simply water; rather a shorter, though still an appreciable time, if it be sulphate of soda; and, lastly, an almost insensible period if it be the solution of nitric acid. The following are the results of an experiment performed with a pile of ten elements, *zinc and copper*, the surfaces four inches square, charged with river water:

Duration of contact of the pole with the condenser,

15″
30″
60″.

Divergence of the gold leaves of the electroscope,

2°
6°

(The gold leaves touch the envelope of the electroscope).

If we wish to charge the condenser twice in succession with the same pile, a period of time must be allowed to elapse between the two charges, varying according to the nature of the liquid with which the pile is charged, exactly in the same manner as the duration of the contact of the condenser and the pole varies. These results may easily be explained by considering, that as river water has much less action upon zinc, a much longer time is required for the development of a certain quantity of electricity, while with saline and especially with acid solutions, this electricity is developed instantaneously. That in the first case, notwithstanding its slow production, there may still be a considerable quantity of electricity accumulated at the poles, is a fact which arises from the imperfect conductibility of the pile. I have seen a pile of 120 elements, *zinc and copper*, charged only with pure water, give sparks at both its poles, in very dry weather in winter, like an electrical machine, while the same pile, charged with an acid solution, gave scarcely any signs of tension to an electroscope. It is the same with dry piles, in which the humidity with which the paper is always more or less impregnated performs the office of conductor, and they present in a much more obvious manner all the phænomena produced by piles of zinc and copper, charged with pure water. I must not omit to say that, in operating with the last-mentioned piles, care must be taken completely to insulate all the parts of which they are composed, by uniting their pairs by glass rods covered with lac-varnish instead of wooden rods, and by employing as troughs glasses insulated by means of supports of resin.

Dynamic effects of the pile.—For investigating the dynamic effects of the pile, I have employed either a galvanic multiplier, Bréguet's metallic helix, which I placed in the circuit to appreciate the heat developed by the current, or a very sensible apparatus intended to measure the gases resulting from the decomposition of the water by the current in passing through an acid solution. In my memoir I have given a detailed description of this apparatus, and of the manner in which I employed it.

In the first place, it is evident that whatever be the dynamic effect to be produced, when the number of pairs is constant, the quantity of electricity disengaged in a given time, and consequently the intensity of the effect produced, increase in proportion to the increased extent of the surface of each pair. This increase, however, is far from being the same for the different classes of effects, as shall be shown elsewhere. But the true question which is here to engage our attention is this: A surface attacked by a certain solution, and a corresponding surface of a metal less attacked, or not at all, being given, what is the number of pairs to be formed of them to produce the most considerable dynamic effect? At the first glance the reply does not appear doubtful. A single pair must be made of them, for, according to our theory, the quantity of electricity which circulates through the conductor is always equal to that developed in a single pair; the electricities of the interior pairs reciprocally neutralizing each other. But this theoretical reply is only true so far as the conductor which unites the two poles can be considered as perfect; thus it is true with relation to all the dynamic effects that can be produced by employing, as a conductor, a copper wire of sufficient thickness not to be heated by the current. It is true also in a less degree, that is, two or three pairs must be formed of the given surfaces to produce the maximum of effect, when the connecting wire is, either from its nature or its dimensions, a worse conductor, in which case the wire becomes heated even to redness in consequence of its resistance to the passage of the current. But the reply is completely false when the conductor is imperfect, such as a liquid or two points of charcoal; a rather considerable number of pairs must be made of the surfaces in this case in order that the current may pass, decompose, and heat the liquid, and produce heat and light between the two points of charcoal. It is therefore exceedingly inaccurate to distinguish the nature of effects by the number of pairs most favourable for their production; and to say, for example, that to produce considerable calorific effects, piles of large surfaces and a small number of pairs must be employed,—and inversely, for considerable chemical and physiological effects. Experiment has proved that the degree of conductibility of the body interposed between the poles, alone determines the number of pairs most favourable, whatever be the nature of the effect produced, and that this number *is exactly in an inverse ratio to the conductibility of the body.*

This law flows immediately from the theory of the pile which we have enunciated. In fact, when the two electricities are accumulated at the two poles of the pile, they have, as we have seen, two different ways of neutralizing each other, that of the pile itself and that of the conductor, which unites the two poles. The larger or smaller proportion of the two electricities which follow either of these paths, depends upon the relative facility afforded by them for their reunion; if the pile be in ever so small a degree a better conductor than the body interposed between its poles, no portion of the current, or only a very feeble one, will traverse this body. In every case, therefore, the number of pairs formed from the given metallic surfaces must be large enough to render the pile a worse conductor than the body placed between its poles. Care must also be taken to make all these pairs equal, for should one of them be smaller than the others, its influence, as we have already seen, immediately diminishes the quantity of free electricity which circulates in a given time through the conductor. This result is in exact agreement with my own observations, and with some experiments very recently performed by Mr. Binks, (Phil. Mag., July 1837,) which confirm with the most remarkable precision the theory which we have given.

Examination of circumstances which affect the power of the pile.

We have supposed in the preceding paragraph that the quantity of metal employed to make a pile is constant; but it may happen that in a given pile either the surface or the number of its pairs may be increased. What will be the result of these species of augmentation in theory and in practice? We proceed to inquire.

Experiment teaches us that if the number of the pairs remain constant, the tension of the pile is not increased in a sensible manner by increasing their surfaces, and that the dynamic effects are increased in proportion as those effects are produced by better conductors placed between the poles of the pile. I shall not relate, from fear of prolixity, the numerous experiments which have conducted me to this law; besides, many physicists had already made analogous observations. The theoretical reason for this result may be easily imagined. It is true, that by increasing the surface of the pairs, the quantity of electricity disengaged in a given time is increased, but the interior conductibility of the pile is also increased, as may be readily understood. Now, for effects of tension, these two circumstances nearly counterbalance each other; I am, however, disposed to believe, from experiment, that when the pile is charged with a liquid which is a very good conductor, and which exerts a lively chemical action upon one of the metals of the pairs, an increase of the surface of the pairs diminishes the tension. As to the dynamic effects, it may be clearly seen that when they are produced in much better conductors than the pile, as much is gained by increasing the surface of the pile, and in consequence the quantity of electricity which circulates in a given time through these conductors. But if the bodies placed between the poles be imperfect conductors, and only in a *small* degree better conductors than the pile, by increasing the surface of the pairs, the quantity of electricity which circulates through those bodies is increased; but the conductibility of the pile is also augmented, and as that of the bodies interposed between the poles does not alter, a larger proportion of the total amount of the electricities necessarily reunites through the pile itself. This is the reason that in the latter case the dynamic effects do not increase with the surface of the pairs in so large a proportion as in the former case; this is also the reason that in certain dynamic effects, such particularly as physiological effects, the augmentation is insensible.

We will now inquire what will be the result, if, without altering their surface, the number of pairs be increased, taking care that the additional pairs are exactly similar to those of which the pile is already composed. The following are the results:

I. That the tension of the pile increases constantly with the number of the pairs, but that for dynamic (magnetic, calorific, and chemical,) effects, there is a limit in the number of pairs which produces them with the greatest degree of intensity.

II. That the number of pairs which in each case produces the maximum of effect, diminishes in proportion as the body placed between the poles is a better conductor, and the liquid interposed between the poles a worse one, and less adapted to exert a lively chemical action.

III. That it often happens, especially when the pile is not very energetic, that when the number of pairs the most favourable to the production of a certain effect, in each case, has been exceeded, the diminution in the intensity of the effect which results from the addition of other pairs, ceases when this addition amounts to a certain number; that the effect then again becomes as intense as it was previously, to diminish a second time in the same manner by again increasing the number of pairs.

IV. That whatever be the absolute intensity of the effects produced by a pile, this intensity diminishes with a rapidity proportional to the amount of the number of the pairs composing the pile, such at least is the fact when the conductor placed between its poles is very good, and the liquid with which it is charged exerts a feeble chemical action.

As the preceding results are entirely new I shall describe some of the experiments by which they were arrived at.

Exp. I.—*Pairs of zinc and copper, with surfaces four inches square charged with slightly acidulated water.*

Number of Pairs.	Degrees of Bréguet's helix.
20	65°
15	50
10	40
5	40
3	43
2	35
1	25

Exp. II.—*Pairs of zinc and copper, with surfaces sixteen inches square charged with acidulated water which has been several times employed, and which consequently is more saline than acid.*

10	17°
20	17
40	10
60	25
120	20

Exp. III.—*Pairs the same as the preceding but charged with a solution still less acid.*

10	12°
20	14
30	15
40	6
50	7
60	9
80	10
90	11
100	9
120	7

Exp. IV.—*Pairs the same as the preceding.*

Number of Pairs.	Number of seconds necessary to obtain the same volume of gas.
60	75″
120	52
180	43

Exp. V.—*Another experiment with the same pairs.*

10	66″
20	25
30	22
40	17
60	14
80	13
100	12
120	15

By employing pairs similar to the preceding, with the double magnetic galvanometer, I found that two, twenty, and a hundred and twenty pairs developed perfectly equal currents. The strongest currents were developed by fourteen and seventy pairs.

To exhibit the influence of the duration of the effect upon its intensity, I shall produce some further experiments performed with pairs with surfaces only four inches square, but charged with a strong solution of nitric acid. Two pairs gave in the first instant 215° of Bréguet's helix, at the end of five minutes 100°, at the end of ten minutes 80°. In another experiment four pairs gave in the first instant 300°, at the end of five minutes 160°, at the end of fifteen minutes 100°. Lastly, six pairs gave at the first instant 500°, but the effect diminished with great rapidity. The preceding pairs had never been employed; but after using them a certain number of times at considerable intervals, I found a great difference in the effect resulting from the larger or smaller number of pairs. Thus in the first experiments were obtained, with two pairs, 53° of Bréguet's helix; with four pairs, 75°; with six pairs, 97°; and in the latter experiments were obtained, with two pairs, from 11° to 12°; and with six pairs, from 5° to 6°.

The following experiments were made with a slightly acid solution, and show the rapidity with which the intensity of the current diminishes according to the number of pairs. At the instant of immersion two pairs gave 50° of Bréguet's helix, but at the expiration of a minute they only gave 30°; fourteen pairs perfectly similar gave at the first instant 35°, but at the end of a minute their effect was reduced to 10°. When the solution had lost nearly all its acidity, two, four, eight, and sixteen pairs, all gave 20° at the moment of immersion, but their effect underwent a diminution, the quantity and rapidity of which corresponded to the increase in the number of pairs.

We proceed to inquire how the preceding results are to be reconciled with our theory of the pile.

In all the experiments which have been described the surface of the pairs remains constant, their number alone varies; it is therefore evident, from what has been said, that the quantity of electricity produced at each of the poles is always the same, but that the two electricities accumulated at the two poles reunite in a larger or smaller proportion through the pile. So soon as, by sufficiently increasing the number of the pairs, we have succeeded in causing nearly the whole of the two electricities to pass, in order to reunite, by way of the conductor, in preference to that of the pile, the limit is attained beyond which nothing is gained by adding to the number of pairs. It may therefore easily be seen why this limit will be so much the sooner attained, in proportion as the bodies interposed between the poles are better conductors, and on the contrary the liquid of the pile a worse conductor. But why, by increasing the number of pairs beyond the limit, is the intensity of the current diminished? It would appear that neither diminution nor augmentation should be effected. To explain this result it must be observed that when the chemical action of the liquid upon the oxidable metal of the pair is lively and prompt, it develops quantities of electricity upon each pair, sufficiently considerable to be regarded as sensibly equal in the same time, allowing that the small differences which exist among them disappear when they are compared to the absolute quantities themselves. The result therefore is, that all the pairs being nearly of the same strength, an increase in the number of pairs would not have any influence upon the quantity of electricity circulating between the poles; in fact, as we have seen, the intensity of the current is not diminished by increasing the number of pairs. But if the chemical action, instead of being lively, is weak, it cannot be considered as developing, in the same instant, physically equal quantities of electricity; and the differences are more sensible in proportion as the absolute quantity of electricity developed is less. On the other hand, as the quantity of electricity in circulation in each pair, and between the poles of the pile, is determined by that disengaged by the most feeble pair, when once the necessary limit has been exceeded, the more pairs there are the more the number of cases is increased, in which a feeble disengagement of electricity may take place in a given time, and consequently the more diminished is the total quantity of free electricity in circulation. To prevent this diminution there must always be an absolute simultaneity and equality in the quantities of electricity disengaged, in the same instant, by each pile, which is physically impossible, and that impossibility increases as the number of the pairs becomes greater, and the chemical action more feeble: all which is in perfect agreement with experiment. It now remains to be shown why, when the addition of a certain number of pairs has diminished the intensity of the current, a further addition, instead of continuing to reduce it, can on the contrary terminate the diminution, and cause the intensity again to increase. When it was said, that the limit in the number of pairs most favourable for obtaining a certain effect, was attained, the moment the current preferred the way of the conductor to that of the pile, it was not to be understood by that, that no portion of the current passed through the pile; on the contrary, the result of the manner in which the electricity is distributed between the conductors is, that a certain proportion of the electricities continues to reunite through the pile. But as this proportion is very small, and as besides, when the number of pairs is already great, the addition of others has no sensible influence, the physical limit is attained, even when a proportion of the two electricities still reunites through the pile. However, if the number of pairs be considerably increased, that part of the electricity which circulates through the pile is in the end diminished, and consequently that which passes through the conductor is in the same degree increased. A great addition to the number of pairs is then a cause of augmentation to the current; it is also, as has just been shown, a cause of diminution. This augmentation and this diminution do not follow the same law in relation to the number of pairs.

It is easy to conceive that as with a certain number of pairs the augmentation preponderates over the diminution, so, on the contrary, with a different number, the diminution preponderates over the augmentation. The same cause produces contrary effects with energies which acknowledge no regular law; hence the alternations in the intensity of the current in each particular case may depend upon a multitude of variable circumstances; such for example as the nature of the pairs, their dimensions, the degree of acidity and conductibility of the liquid employed, &c. The results of experiment are in exact agreement with this.

We shall conclude our researches by inquiring into the result, if, instead of active pairs, inactive pairs be added to the pile, that is, homogeneous metallic arcs of zinc, copper, or platina. After I had enunciated the theory of the pile for the first time, in 1828, M. Marianini thought a strong objection might be raised against it from the influence of inactive pairs, which he had studied carefully and in detail. Interposing between the active pairs of a pile, diaphragms of copper, in a greater or smaller number, he found that no augmentation in the tension of the two poles resulted from it, and that this interposition even diminished the chemical power of the pile in the decomposition of water. But, said he, by means of these diaphragms the interior conductibility of the pile is diminished, and consequently so much the more electricity should pass by the conductor which unites its two poles. It is perfectly true that whatever diminishes the conductibility of the pile should augment the tension of its poles, and the quantity of electricity which circulates in the conductor which unites them; *provided at the same time that the quantity of electricity developed by each pair is in no degree altered.* Now the manner in which M. Marianini diminishes the conductibility of the pile is not included in these limits. The zinc and copper of the pairs between which he places his diaphragms are no longer in the same condition as those of the other pairs; in fact the liquid which separates them loses a great part of its conducting power by the interposition of these diaphragms; hence the positive electricity accumulated on the side of the zinc, and the negative carried off by the copper reunite in much less proportion through this liquid, which is become a worse conductor; it is exactly as if, instead of interposing diaphragms, a liquid of much less conducting power had been substituted for the liquid which separates the pairs. What is the result? That the free electricity of all the other pairs diminishes in the same ratio as that of the pair which we have been considering, so that if, on the one hand, the two electric principles accumulated at the two poles reunite with less facility, on the other hand they are developed in smaller quantity. With regard to tension, which is unconnected with the element of time, since the condenser remains in contact with the pole an indeterminate period, it may be conceived that the two effects just noticed will nearly counterbalance each other, but it is different with regard to the decompositions effected by the current, and in general, with regard to all its dynamic effects, for there is not sufficient time for the accumulation of the two electric principles; and whatever diminishes the quantity of free electricity disengaged by each pair in a given time, and consequently at the two poles, diminishes the intensity of the effects produced by this circulation. The only means by which the conductibility of a pile may be diminished, without altering the quantity of electricity which it produces, is, as has been already said, by the addition of similar pairs.

In support of the explanation which has just been given, I shall produce some experiments which I made with diaphragms of different kinds. I had previously shown (*Annales de Chim. et de Phys.*, Feb. 1825) and again in 1828 (*Annales de Chim. et de Phys.*, vol. xxxvii. p. 225.) that the diminution of conductibility in a liquid which results from the interposition of a metallic diaphragm, is reduced in the proportion that the diaphragm is attackable by the liquid. The following are the results which I obtained.

A pile of seven elements, zinc and copper, the surfaces each four inches square, charged with pure water, mixed with $\frac{1}{10}$ of nitric acid, gave 125° of Bréguet's metallic helix. A diaphragm of zinc, interposed between any two pairs, reduced the effect of the current to 100°; and a diaphragm of copper reduced it to 70°. A pile of twenty pairs, similar to the preceding, but less strongly charged, gave 110° of the metallic helix; a diaphragm of zinc produced no sensible diminution in the intensity of the current; a diaphragm of copper reduced it to 100°. Lastly, employing diaphragms of platina, by means of which I divided a certain quantity of concentrated nitric acid into two or more parts, through which passed the current of a strong pile composed of eight pairs, each two feet square, and charged with a mixture of forty parts water, two sulphuric acid, and one nitric acid, I always obtained, with Bréguet's metallic helix, the following results:

Exp. I.		*Exp. II.*	
Number of diaphragms.	Cent. Deg. of the helix.	Number of diaphragms.	Cent. Deg. of the helix.
1	312°	1	220°
2	170	2	100
3	75	3	27
4	12	4	5
5	0	5	0

Exp. III.

Number of diaphragms.	Cent. Deg. of the helix.	Time for the evolution of the same volume of gas.
0	38°	5″
1	3	25″
2	0	no effect.

From the first two experiments it may be seen in what an enormous proportion the calorific intensity diminishes, according to the increase in the number of diaphragms. In the third experiment the current was made to pass at the same time through the metallic helix, and an acid solution placed in succession in its path. From this experiment it may be seen that the interposition of a diaphragm which reduced to $\frac{1}{13}$ the calorific intensity of the current, only reduced its chemical power to $\frac{1}{5}$; a difference which shows the considerable influence which the rapidity of the current exerts over calorific power; as I have shown in a special memoir. (*Bibl. Univer. Jan.* 1829.)

Summary.—We shall briefly recapitulate the results of these researches.

1. The contact of two heterogeneous bodies without *calorific*, *mechanical*, or *chemical* action, is not of itself a source of electricity.

2. Chemical action, even the most feeble, develops a sensible quantity of electricity; but if this electricity be not always perceptible, if especially its intensity be not always in the ratio of the vivacity of the chemical action which produces it, it is essentially due to the immediate recomposition of the two electricities which takes place upon the surface attacked.

3. Entering upon the theory of the pile, the production, and distribution of electricity in this apparatus, may be completely explained by the development of electricity resulting from the chemical actions, and the neutralizations of the free electricities which take place from pair to pair.

4. Attention being paid to the conductibility of the pile itself, and comparing it with that of the bodies placed between its poles, all the dynamic effects of voltaic electricity, the circumstances which may modify the intensity of these effects, and the apparent anomalies which they present may be easily explained; provided at the same time the principles above stated are not lost sight of.

Ryde, Isle of Wight, 10th Aug., 1837.

[We shall notice in our next Number M. De la Rive's paper *On the influence of heat on the passage of an electric current from a liquid into a metal*, and also his *Researches on the Properties of Magneto-electric Currents*.—EDIT.]

XXXIV. *On the Affinity of some Fossil Scales of Fish from the Lancashire Coal Measures with those of the recent* Salmonidæ. *By* W. C. Williamson, *Esq., Curator to the Manchester Natural History Society**.

[With Figures: Plate II.]

IN examining the roofstone of the black and white coal at Peel near Worsley, I was fortunate enough to meet with remains of fish, of a character sufficiently interesting to merit this brief notice. This coal occupies a position about a thousand yards from the top of the carboniferous series, and at Peel presents in its immediate vicinity the following section:

	Ft.	In.
Metal, 15 yards.		
Coal	7	0
Metal, 4 yards.		
White sandstone, 35 yards.		
Metal, 44 yards.		
"White" coal	2	5
Clay, one to seven yards.		
"Black" coal	2	5

The shale in which the fish occur rests immediately upon the "white coal." It is very compact, bituminous, and well calculated for the preservation of minute characters. In this shale I have already detected at least four species, belonging to as many distinct genera, but at present would only call the attention of ichthyologists to one, scales of which magnified to thrice their natural size, are represented by the sketches (Pl. II.) figs. 4, 5, and 6. I have met with several specimens of the fish to which they belong, but all are more or less in a crushed condition. The scales vary in form from being nearly circular to an oblong rhomboid, and on examining the scales of recent Salmonidæ, I found that they had the same general forms. A more minute examination exhibited a strong resemblance in the arrangement of the cycloid striæ, which are nearly as distinct in the fossil as in the recent species, especially on the inferior surface of the scale, part of which is represented at fig. 5.

Fig. 1, 2, and 3 exhibit scales of the common salmon (after the pearly enamel is removed) magnified to the same degree as the fossil ones.

The only specimen I have yet found showing all the fins is about 4 inches long. The head is crushed, but the body and tail are nearly perfect. The anterior dorsal and the ven-

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* Communicated by the Author.

tral fins are placed opposite each other, as also are the anal and what appears to be the posterior dorsal. Here a discrepancy exists. In recent Salmonidæ, the latter fin is merely a fleshy appendage, and not supported by rays. In the fossil specimen the fin, though imperfect, exhibits traces of true rays. Though this will prevent its being considered one of the Salmonidæ, I know none other of the abdominal Malacopterygii that have the same arrangement of the fins: this fact, combined with the resemblance between the scales, which differ only in the existence of radiating striæ at one extremity of the fossil, which do not exist on the recent ones, seems to indicate a close affinity. The discovery of more complete specimens may throw new light upon their nature.

XXXV. *Analyses of the Hydrates of Baryta and Strontia.* *By* H. M. NOAD, *Esq., Lecturer on Chemistry.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ON looking over some of the back Numbers of your Magazine, which have only lately come into my hands, I met with a paper by Mr. J. D. Smith on the hydrates of barytes and strontia, in which he seems to think that the previous determination of the quantity of water combined with these metallic oxides, by Mr. Phillips, is not correct. On referring to my journal, I found the results of an analysis of these hydrates by myself performed about two years ago, and having some crystals of each at the bottom of two bottles in which they had remained since that time, I determined to analyse them again, with the greatest care, as my results did not exactly correspond with those of Mr. Smith. As my recent experiments coincide exactly with my former ones, perhaps you will allow them to be inserted in your Journal, for the inspection of Mr. Smith and others. I must observe that the greatest care was taken to get the crystals perfectly dry by pressure between folds of blotting-paper.

	Bar.		Sulph. of Bar.		Bar.		Water.
Exp. 1.	37·5	gave	26·75	=	17·56	+	19·94
Exp. 2.	30·	gave	21·5	=	14·11	+	15·89
Exp. 3.	26·	gave	18·62	=	12·22	+	13·78
					43·89		49·61

or 76·7 one equivalent of baryta combined with 86·69 nearly $9\frac{3}{4}$ equivalents of water.

Thirty grains of crystals of strontia dissolved in hot water, and precipitated by bicarbonate of potash, gave 16·5 of carbonate of strontia = 11·56 strontia; a second thirty grains gave precisely the same result: so

Stron. Stron. Water.
30 11·56 + 18·44, or, 51·8, one equivalent of strontia combined with 82·62, rather more than 9 equivalents of water.

Although these experiments do not agree with those either of Mr. Phillips or Mr. Smith, their coincidence with each other appears to show that these two metallic oxides do not behave precisely the same with respect to water.

I am, Gentlemen, yours, &c.,
Shawford near Bath, Aug. 18, 1837. HENRY M. NOAD.

XXXVI. *Analytical Investigation of Professor* Wallace's *Property of the Parabola. By* J. R. YOUNG, *Esq., Professor of Mathematics in Belfast College.*[*]

IN the Philosophical Magazine for August 1836, a remarkable property of the parabola, first established by Professor Wallace, is made the subject of a communication from Mr. Lubbock; in which communication that distinguished mathematician has given a proof of the theorem upon the principles of analytical geometry.

In a subsequent Number of the Magazine, that for January 1837, two other analytical investigations were given; one by Mr. Greatheed of Trinity, and the other by Mr. Holditch of Caius College, Cambridge; and a neat geometrical proof was at the same time offered, differing but little from that of Professor Wallace himself.

Ingenious and interesting as these several investigations are, they can, I think, scarcely be regarded as more than mere *verifications*, by analysis, of a previously known geometrical truth; in as much as they do not exhibit the steps by which an analyst would be likely to be led to such a property, unless it were anticipitated at the outset. It is certain also that no analytical investigation hitherto given is comparable, on the score of simplicity, with the geometrical proof; although it would be premature to assert that analysis is incompetent to furnish a proof equally simple and elegant.

The investigation which I here offer is remarkably easy; it differs essentially from those hitherto given, and will I think bear a favourable comparison with the geometrical method.

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[*] Communicated by the Author.

Referring to the diagram at page 33, in the Magazine for January last, (vol. x.) let p be the origin of conjugate axes, p being the point of contact of the tangent A P; and let a line be drawn from A, any point in this tangent, to the focus F. The angle v contained between this line and a second tangent A Q, will be obtained from the equations of the lines themselves by help of the known relation,

$$\tan v = \frac{(a-a') \sin \beta}{1+a a'+(a+a') \cos \beta},$$

a and a' representing the coefficients of x in those equations, and β the inclination of the axes of reference. Let the coordinates of the point of contact Q be $x_{\prime}, y_{\prime}$; then those of the point A will be $0, \frac{y_{\prime}}{2}$; also, the point F is $(m, -2\, m \cos \beta)$. Hence

$$\text{Equation of A Q is } y = \frac{2\, m}{y_{\prime}} (x+x_{\prime})$$

$$\text{Equation of A F is } y - \frac{y_{\prime}}{2} = \frac{\frac{y_{\prime}}{2} + 2\, m \cos \beta}{-m}\, x$$

the tangent of the angle between them is by the preceding formula

$$\tan v = \frac{-\left\{\frac{2\, m}{y_{\prime}} + \frac{y_{\prime}}{2\, m} + 2 \cos \beta\right\} \sin \beta,}{1 - 1 - \frac{4\, m}{y_{\prime}} \cos \beta + \frac{2\, m}{y_{\prime}} \cos \beta - \frac{y_{\prime}}{2\, m} \cos \beta - 2 \cos^2 \beta}$$

$$= \frac{\left\{\frac{2\, m}{y_{\prime}} + \frac{y_{\prime}}{2\, m} + 2 \cos \beta\right\} \sin \beta}{\left\{\frac{2\, m}{y_{\prime}} + \frac{y_{\prime}}{2\, m} + 2 \cos \beta\right\} \cos \beta} = \tan \beta.$$

Hence, since the angle β is equal to the angle F P A, it follows that from whatever point in a tangent to a parabola two lines be drawn, one to touch the curve, and the other to the focus, the angle between them will be constant, and equal to that between the fixed tangent and radius vector of its point of contact. Thus F C subtends equal angles at A and B, and therefore the points A, B, F, C are in the same circumference.

It follows moreover that, B being upon the tangent R B, F B P = F R B; and since F P B = F B R ∴ P F B = R F B, a property which must be previously established, in the geometrical method, and of which I have never seen any analytical proof.

In instituting a comparison between the geometrical and analytical methods, in any given instance, account should of course be taken of the amount of extraneous aid required for the furtherance of each process. In analysis the truth is in general evolved from the fundamental conditions, independently of subsidiary propositions beyond those which constitute the first principles of the subject. In geometry, on the contrary, we cannot arrive at the same conclusion without the assistance of some neighbouring truth or chain of truths nearly as remote from first principles as that to be established. It may be proper to remark, that the expression $-2m\cos\beta$, for the ordinate of the focus, is deduced from the simplest and most obvious considerations. The abscissa of the focus is m, which is also the radius vector of the origin; the ordinate is thus the base of an isosceles triangle whose side is m, and angle at the base β or the supplement of β, accordingly as this ordinate is negative or positive; its true value is therefore that employed above.

Aug. 7, 1837.

XXXVII. *On Thermo-electricity.* *By* Mr. Francis Watkins.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THAT species of electricity developed by the influence of temperature first observed and made known to us by M. Seebeck, in communications to the Academy of Berlin, in the years 1821 and 1822, having latterly attracted increased attention in this country, through the very interesting letter of Professor Wheatstone, published in your last May number, page 415, and also from a valuable communication of Professor Andrews, printed in the following June number, page 433, I venture to trouble you with a few remarks, which I flatter myself if admitted into your miscellany will not prove unacceptable to many of your readers.

Professor Wheatstone's communication informs us that Cav. Antinori obtained the spark from a thermo-electric pile of Nobili's construction, consisting of twenty-five elements, by employing an electro-dynamic helix and a temporary magnet, while Professor Wheatstone employed a thermo-electric pile of thirty-three elements of bismuth and antimony, formed into a cylindrical bundle $\frac{3}{4}$ of an inch in diameter and $1\frac{1}{3}$ in length: the poles of this pile were connected by means of two thick wires, with a spiral of copper ribbon fifty feet in length and 1½ inch broad, the coils being well insulated by brown paper and silk.

We gather from these observations that Cav. Antinori employed an elongated coil as his electro-dynamic helix with temporary magnets for eliciting the spark, while Professor Wheatstone very judiciously resorted to the flat copper ribbon coil, contrived and recommended for developing electricity of feeble intensity by Professor Joseph Henry, of New Jersey College, Princeton.

Cav. Antinori gives different lengths of his coils, and I presume in all cases that he had the advantage of the influence of temporary magnets, for I have experimented with short and slender coils surrounding different metals, wood, &c., and failed entirely in obtaining sparks under these conditions. When the same coils enveloped soft iron, then I got a spark, even a feeble one, from a coil the wire of which was only seven feet long and $\frac{1}{40}$ of an inch in diameter.

With Professor Henry's flat coil I always show larger sparks than with an elongated wire coil and large temporary magnet, and the snapping noise of the spark is certainly more discernible. Hence I feel warranted in advising those who desire to show the thermo-electric spark in its fullest effect to use the flat ribbon coil of Professor Henry, in preference to the elongated wire coil and temporary magnet, at the same time permit me to add that those who possess a fair-sized electro-magnet can exhibit the spark of a thermo-pile with tolerable efficiency.

My first attempt to repeat Professor Wheatstone's experiments was with a very small and slender thermo-pile of thirty pairs of elements three inches long, and a Henry's coil, and I fully succeeded in eliciting the spark. A few days afterwards in an interview I had with the Professor he was so kind as to inform me that a thermo-electric pile of one inch square plates in his possession had afforded him sparks of an increased size to those obtained from the small pile described in his communication to you. Hence I adopted the suggestion of employing large metallic elements.

I have recently made many experiments with thermo-piles of various-sized and different-formed metallic elements, and different numbers of alternations, and find that the powers exerted by the metallic elements of a pile are referrible to the same law which governs the development of electricity derived from other sources of excitation, namely, that quantity is increased by mass.

The spark has hitherto been obtained from the surface of mercury, a metal at all times to be avoided in an apparatus in which metals like antimony and bismuth joined together by soft solder form a part. I arrange one of my extremities of the pile of strong sheet copper, cut like a comb, and covered with soft solder (the latter is a plan, I believe first suggested by Dr. Hare, to obviate the trouble of amalgamating at every experiment), and when the moveable extremity of my Henry's coil is passed over the comb, and the thermo-electric pile in action, splendid sparks are seen every time the moving part of the coil breaks the circuit by leaving a tooth of the comb. I have used stellar-formed wheels, vibrating pendulums, &c., &c., to break contact, and all give beautiful sparks and shocks when desired. When a small steel file forms the moving part, splendid scintillations are noticed, and to do away with amalgamation, soft solder, &c., I frequently employ an old plan of silver against silver for making and breaking contact; the spark thus developed is vivid, and, as we might expect, of a beautiful green hue; but it must be confessed that no sparks are so brilliant as those from the surface of mercury, for we can seldom obtain other metallic surfaces equally clean.

I desire here to record what I believe to be novel, that on the 27th of last June, with a thermo-electric pile, consisting of thirty pairs of bismuth and antimony, 1½ in. square and ⅛ thick, with the radiation of red hot iron at one extremity and ice at the other extremity, a soft iron electro-magnet, under the inductive influence of the electricity thus generated, supported ninety-eight pounds weight, the most powerful thermo-electric magnet I have heard of; but it must be observed that this is no maximum, for whoever employs a larger elementary battery will no doubt obtain greater effects, not only as regards inductive influence on soft iron, but all others in which the influence of temperature may be exerted.

There is an ample field for investigation open for those who have leisure on this subject. Who knows but hereafter electro-magnetism may be employed as a prime mover, and that a thermo-pile may be the exciting cause?

By adopting Professor Henry's method of giving the shock with his flat ribbon coil, from a single pair of voltaic plates, I have succeeded in obtaining in a marked and decided manner the physiological effects on the tongue, with the thermo-pile of thirty pairs of elements.

The spaces in your excellent journal are far too valuable to be occupied by lengthened descriptions of the various-formed metallic elements that I have used and that may be employed, and even of those that I have found best suited for the experimental inquirer and public demonstrator. Nor dare I venture to occupy your pages with long details of the results arrived at by different modes of applying increasing or decreasing temperature to the pile. All that now need be said on that head is that I have thermo-electric piles varying from fifteen to thirty pairs of metallic elements, which give brilliant sparks by simply pouring hot water on one end, while the other end is at the temperature of the atmosphere. Again, sparks are exhibited by the same piles when the temperature is reduced at one end by the aid of ice, and the other end at the temperature of the surrounding air.

Of course, as has been noticed before, the effects will be greatly enhanced by still greater difference of temperature being produced at the opposite end of the pile.

I remain, Gentlemen, yours, &c.,

5, Charing Cross. FRANCIS WATKINS.

XXXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

May 31, 1837.—"ON certain areas of elevation and subsidence in the Pacific and Indian oceans, as deduced from the study of Coral Formations;" by Charles Darwin, Esq., F.G.S.

The author commenced by observing on some of the most remarkable points in the structure of Lagoon islands. He then proceeded to show that the lamelliform corals, the only efficient agents in forming a reef, do not grow at any great depths; and that beyond twelve fathoms the bottom generally consists of calcareous sand, or of masses of dead coral rock. As long as Lagoon islands were considered the only difficulty to be solved, the belief that corals constructed their habitations (or speaking more correctly, their skeletons), on the crests of submarine craters, was both plausible and very ingenious; although the immense size, sinuous outline, and great number, must have startled any one who adopted this theory. Mr. Darwin remarked that a class of reefs which he calls "encircling" are quite, if not more, extraordinary. These form a ring round mountainous islands, at the distance of two and three miles from the shore; rising on the outside from a profoundly deep ocean, and separated from the land by a channel, frequently about 200 and sometimes 300 feet deep. This structure as observed by Balbi resembles a lagoon, or an atoll, surrounding another island. In this case it is impossible, on account of the nature of the central mass, to consider the reef as based on an external crater, or on any accumulation of sediment; for such reefs encircle the submarine prolongation of islands, as well as the islands themselves. Of this case New Caledonia presents an extraordinary instance, the double line of reef extending 140 miles beyond the island. Again the Barrier reef, running for nearly 1000 miles parallel to the North-East coast of Australia, and including a wide and deep arm of the sea, forms a third class, and is the grandest and most extraordinary coral formation in the world.

The reef itself in the three classes, encircling, barrier and lagoon, is most closely similar; the difference entirely lying in the absence or presence of neighbouring land, and the relative position which the reefs bear to it. The author particularly points out one difficulty in understanding the structure in the barrier and encircling classes, namely, that the reef extends so far from the shore, that a line drawn perpendicularly from its outer edge down to the solid rock on which the reef must be based, very far exceeds that small limit at which corals can grow. A distinct class of reefs however exists, which the author calls "fringing reefs," which extend only so far from the shore, that there is no difficulty in understanding their growth. The theory which Mr. Darwin then offered, so as to include every kind of structure, is simply that as the land with the attached reefs subsides very gradually from the action of subterranean causes, the coral building polypi soon again raise their solid masses to the level of the water; but not so with the land: each inch lost is irreclaimably gone:—as the whole gradually sinks, the water gains foot by foot on the shore, till the last and highest peak is finally submerged. Before explaining this view in detail, the author offered some considerations on the probability of general subsidences,—such as the small portion of land in the Pacific, where many causes tend to its production, an argument first suggested by Mr. Lyell, and the extreme difficulty (with the knowledge that corals grow at but limited depths) in explaining the existence of a vast number of reefs on one level, without we grant subsidence, so that one mountain top should be submerged after another; the zoophytes always bringing up their stony masses to the surface of the water. Subsidence being thus rendered almost necessary, it was shown by the aid of sections, that a simple fringing reef would thus necessarily be converted by the upward growth of the coral into one of the encircling order, and this finally, by the disappearance through the agency of the same movement of the central land, into a lagoon island. In the same manner a reef skirting a shore would be changed into a barrier extending parallel to, but at some distance from, the mainland.

Mr. Darwin then showed, that there existed every intermediate form between a simple well characterized encircling reef, and a lagoon island; that New Caledonia supplied a link between encircling and barrier reefs; that the different reefs produced by the same order of movement were always in juxtaposition, of which the Australian barrier associated with encircled islets and true lagoons, affords a good example. He then proceeded to show that within the lagoon of Keeling Island, proofs of subsidence might be deduced from many falling trees and a ruined storehouse; these movements appearing to take place at the period of bad earthquakes, which likewise affect Sumatra, 600 miles distant. It was thence inferred as probable, that as Sumatra rises, (of which proofs are well known to exist,) the other end of the lever sinks down; Keeling Island thus acting as an index of the movement of the bottom of the Indian Ocean. Again at Vanikoro, where the structure indicates according to the theory recent subsidence, violent earthquakes are known lately to have occurred.

The author then removed an apparent objection to the theory, namely, that subsidence would form a disc of coral but not a cup-shaped mass or lagoon, by showing that the corals which grow in tranquil water are very different from those on the outside, and less effective; and that as the basin becomes shallower they are subject to various causes of injury. The lagoon nevertheless is constantly filling up to the height of lowest water spring tides, (the utmost possible limit of living coral,) and in that state it long remains, for no means exist to complete the work. Mr. Darwin then proceeded to the main object of the paper, in showing that as continental elevations act over wide areas, so might we suppose continental subsidences would do, and in conformity to these views, that the Pacific and Indian seas could be divided into symmetrical areas of the two kinds; the one sinking, as deduced from the presence of encircling and barrier reefs, and lagoon islands, and the other rising, as known from uplifted shells and corals, and skirting reefs. The absence of lagoon islands in certain wide tracts, such as in both the West and East Indies, Red Sea, &c., was thus easily explained, for proofs of recent elevation are there abundant. In a like manner, in very many cases where islands are only fringed with reefs, which according to the theory had not been subsiding, actual proofs of elevation were adduced. Mr. Darwin remarked that, excepting on the theory of the configuration of reefs being determined by the order of movement, the circumstance that certain classes which are characteristic and universal in some parts of the sea, being never found in others, is quite anomalous, and has never been attempted to be explained.

Mr. Darwin then pointed out the above areas both in the Pacific and Indian Oceans, and deduced the following as the principal results. 1st. That linear spaces of great extent are undergoing movements of an astonishing uniformity, and that the bands of elevation and subsidence alternate. 2. From an extended examination, that the points of eruption all fall on the areas of elevation. The author insisted on the importance of this law, as thus affording some means of speculating, wherever volcanic rocks occur, on the changes of level even during ancient geological periods. 3. That certain coral formations acting as monuments over subsided land, the geographical distribution of organic beings (as consequent on geological changes as laid down by Mr. Lyell) is elucidated, by the discovery of former centres whence the germs could be disseminated. 4. That some degree of light might thus be thrown on the question, whether certain groups of living beings peculiar to small spots are the remnants of a former large population, or a new one springing into existence. Lastly, when beholding more than a hemisphere, divided into symmetrical areas, which within a limited period of time have undergone certain known movements, we obtain some insight into the system by which the crust of the globe is modified during the endless cycle of changes.

A letter to Charles Lyell, Esq. " On some changes of level which have taken place during the historical period in Denmark"; by G.

Forchammer, Phil. Doct. Copenhagen, Foreign Member of the Society.

The author referring to the observations of Mr. Lyell and of Mr. Nilsson, on the unequal elevations of Sweden and subsidences of Scania; as proving that not only does elevation go on at a different rate, but that motion takes place in opposite directions, adduces, as instances of similar phenomena, the islands of Saltholm and of Bornholm as well as the Danish coast of the Sound. The island of Saltholm opposite to Copenhagen, and hardly five feet above the level of the Sound, being mentioned from the thirteenth century as a source of income to the chapter of Roeskilde; must have been elevated at a slower rate than Bornholm, which rises about one foot in a century; for if it were now to sink only two feet a very small portion of the island would be left.

On the Danish coast however of the Sound, six miles to the north of Copenhagen, a well characterized beach is observed, six feet above sea-level; hence the author infers that the change of level on the Danish, proceeds in a different proportion from that on the Swedish shore; which he ascribes to the slight earthquakes so frequently felt in Sweden, but never observed in Denmark.

With respect to the Danish island of Bornholm, the author observes, that its whole eastern shore is composed of a granitic rock, rising abruptly out of the sea, and covered to the height of 250 feet by a stiff loamy soil, containing numerous fragments of the slates and limestones of the transition formation; of which the calcareous specimens may easily be traced to the island of Gothland. From these facts, and the absence of the plutonic rocks so frequent in the boulder formation of Denmark, and from the absence of this clayey loam on the western side of the granitic ridge; he conceives that it is the result of a violent inundation from the north-east of the Baltic. The effects of this may be seen both in the form of the Danish coast, and in the deposits of sand which cover a great part of Denmark; but which have been evidently swept away from the more easterly beds of the boulder formation.

At a height of about forty feet may be observed the first beach formed on Bornholm: wherever by the receding of the granitic mountains from the coast, small bays were formed, these became choaked up by the granitic pebbles of the beach, and small ponds were thus formed and separated from the sea; and in the course of ages became filled up with peat. This peat moss is separated from the sea by a beach of small breadth, ten feet high, sloping at an angle of 15°, and abutting on a horizontal plain, 160 feet in breadth, formed entirely of beach stones. Beyond this is a second plain, 100 feet broad, which slopes to the sea at an angle of 9° to 10°, and is followed by the present beach sloping at an angle of 12° to 13°. The pebbles of all are similar in size, and composed of the same granite as the solid rock.

The author, referring to the existence on the sloping beach of graves, marked only by a ring of stones, and to the testimony of antiquaries, that it was the custom to bury Christians on the beach, where the land and sea separated, about the year 900; obtains materials for a rough calculation as to the time when this beach was formed. The continuous but very slow elevation of the island, as shown by the sloping beach, would thus have been about one foot in a century; and the beginning of the regular elevation of the island about 1600 years ago. Previous to this there must have been a long and perfectly quiet time, during which the horizontal beach was formed. Supposing the rise of the island and the lateral addition to the sloping beach to have been quite regular, and the lateral extension of the horizontal beach to have been equally uniform, we require for its formation a period of 2500 years. This would carry back the sudden elevation of the island of ten feet, marked by the narrow and abrupt, as well as highest beach, which the author thinks may have been caused by a great earthquake, to a period of 4000 years from the present time.

The author also informs us that over all Denmark, Sleswig, and Holstein, shells of the German ocean of the present day, may be found sometimes at considerable elevations above the level of the sea. Thus not far from Börnhövel and Holstein, at a height which exceeds 150 feet, a bed of fossils and pebbles occurs, containing *Cardium edule*, *Littorina littorea*, *Buccinum undatum*, *Ostrea edulis*; the latter shell is however smaller than that now living on the coast, but agrees with that found fossil in the raised beds of recent marine shells of England. Subsidences must also have occurred, as between the island of Römöe and the shores of the kingdom of Sleswig, a submarine forest (said to be of fir) is found at nine feet below the present high water mark.

The author also calls the attention of geologists to the traces of an inundation of about sixty feet above the present high water mark, on the islands of the western shores of Sleswig; and which appears to have taken place since these were inhabited by man, since Tumuli are found partly destroyed by the inundation.

A paper " On the Physical Structure of Devonshire, and on the subdivisions and geological relations of its old stratified deposits;" by the Rev. Adam Sedgwick, F.G.S., & R.S., Woodwardian Professor in the University of Cambridge; and Roderick Impey Murchison, Esq., V.P.G.S., F.R.S., was commenced.

June 14.—The paper by Prof. Sedgwick and Mr. Murchison, on Devonshire begun at the last meeting, was concluded.

Chap. I.—After a few preliminary remarks the authors proceed to describe the general structure of Devonshire, which they consider as divided into five distinct geological regions.

1. The first region extends through the most eastern portions of the county, and is principally occupied by formations of new red sandstone and green sand.

2. The second region (which is prolonged into the north-west corner of Somersetshire) occupies the most northern portions of the county, being bounded to the north and west by the sea, to the east by the plains of new red sandstone connected with the Vale of Taunton, and to the south by a line which stretches across the county in a direction almost east by south, commencing at the coast on the south side of Barnstaple, and thence ranging north of South

Molton, Bampton, and Holcombe Rogos, to the plain of the new red sandstone.

3. The third region stretches across the county; being bounded to the north by the line above indicated, and to the south by a line which, commencing at Boss Castle on the coast of Cornwall, ranges to the south side of Launceston, and thence in a somewhat devious course to the northern edge of Dartmoor. This southern boundary also descends considerably on the east side of Dartmoor, inclosing some of the country near Chudleigh. The region thus bounded is composed of one great formation (occupying more than a third of the whole county), to which the authors give the name of *Culm Measures.*

4. The fourth region includes all the country occupied by slate rocks, extending from the granite of Dartmoor and the Culm Measures to the south coast of Devon.

5. The last region is occupied by the granitic rocks, which extend through the whole of Dartmoor.

Of the regions above enumerated few notices are offered respecting the first, but the other four are described in considerable detail, and in the above order.

Chap. II.—Succession of deposits between the north coast of Devon and the Culm Measures.

The authors commence with a description of the rocks in the north-west corner of Somerset, which are identical in structure with a part of the region here described. They divide them into two great groups; the lower group abounding in a coarse arenaceous strong-bedded rock (greywacké), often of a red colour, and sometimes variegated like specimens of new and old red sandstone; the upper containing some beds of like character, but abounding more in rotten thin-bedded slates (*shillot*), in which some portions are highly calcareous, and pass into irregular bands of limestone, and contain encrinital stems and obscure traces of organic remains. They then go on to describe the successive groups occupying the region of North Devon, and by help of natural sections (from the coast to the north boundary of the Culm Measures) prove, that there is an enormously thick ascending series of rocks, interrupted however by numerous contortions and by a great anticlinal line, ranging with the strike of the beds, about west by north or west-north-west. This line runs into the sea a little south-west of Linton, and in consequence one of the great groups is repeated twice over; first on the coast north-east of Linton, and secondly on the coast extending from the Valley of Rocks to Comb Martin. From these facts it follows that the lowest rocks in North Devon are in the denudation of the Lynn river, which nearly defines the position of the anticlinal region; and from the south side of that river to the Culm Measures is an ascending section, interrupted only by local contortions. They then describe the successive groups of the ascending section.

1. Lowest group. Valley of the Rocks and gorge above Linton. The structure of this group is very varied. Some beds coarse and arenaceous; others passing into a fine glossy schist, sometimes chloritic. The finer beds often contain innumerable casts of organic remains, and impressions of shells are not unfrequent in the coarser arenaceous

bands. Near the fossiliferous schists the beds become calcareous, and in one place pass into an impure limestone: many of the beds have a slaty cleavage transverse to the stratification, and cutting through the non-calcareous portions and the lines of organic remains. The thickness of this group is great, but its base is not exposed.

2. The preceding division passes by almost insensible gradations into the great red arenaceous groups already mentioned. The beds of greenish slate, shillot, &c., become quite subordinate, and the whole character of the formation is derived from the coarse arenaceous beds, sometimes passing into a siliceous conglomerate. These coarser beds are commonly red or variegated; among them, however, are grey and greenish grey beds, the colours, as might be expected, being inconstant. Oxide of iron traverses some portions of these rocks in thin veins, and that mineral abounds so much in some of the beds, that they have been regularly quarried (e. g. near Comb Martin and to the south-east of Porlock), and smelted in the iron foundries. The slaty cleavage transverse to the bedding almost disappears among these rocks, but they are much intersected by joints, some quite irregular in bearing; but two sets, one ranging with the beds and the other transverse to these (respectively called *strike joints* and *dip joints*), are described to be of common occurrence. The authors found no organic remains in this group, but they have been found, though very rarely, in some of the shillots and finer schistose masses, which are subordinate to the coarser red siliceous sandstones. Its whole thickness is computed (especially from the coast section west of the Valley of Rocks) to be five or six thousand feet.

3. The next group differs greatly from the former, in having comparatively few of the coarse siliceous sandstones, wanting the red colour; abounding in bands of rotten slate, sometimes like dark indurated slate, but more frequently greenish and chloritic, and commonly exhibiting a cleavage distinct from the stratification. It also contains many calcareous bands (in some places not less than eight or nine), which occasionally swell out into masses of limestone, and numerous organic remains, not however generally well preserved. Its thickness is estimated at two or three thousand feet, and notwithstanding some contortions, it dips on the whole towards the south: its strike, like that of the beds near the coast, is about east-south-east and west-north-west. This formation is traced far into the interior, and is identified with the calcareous system flanking the Quantock Hills.

4. The next group has the same strike as the preceding, and is of enormous thickness, though not so great as might at first sight be imagined from its breadth or the surface of the country and its high inclination, as many parts of it are violently contorted. The authors divide it into two portions, the *lower* abounding in fine glossy chloritic schist, much contorted, and having a true cleavage transverse to the bed, and generally presenting a succession of parallel fissile planes, dipping at a high angle to the south; the *upper* beds containing similar masses alternating with coarse, thick, arenaceous bands, some of which resemble the rocks of the second group of the section.

5. The last group in this part of the ascending section commences on the south side of a line drawn from Baggy Point on the coast in the direction of the strata, or east and by south. It is composed of arenaceous flag stones and soft earthy slates, alternating with harder and coarser bands: it conforms to the mineral type commonly found in the lowest part of the Silurian system, has abundance of organic remains, and is in parts calcareous; but the fossils are often ill preserved and partially destroyed by the cleavage passing through them. The group is several thousand feet thick, and though much contorted (the anticlinal and synclinal lines coinciding with the strike) at length dips regularly under the base of the Culm Measures. Such is the succession in this portion of the section across Devon. Three distinct groups of calcareous and fossiliferous slates, separated from each other by deposits of vast thickness, very little calcareous, and almost without fossils. The base of the series is not exposed, and the last ascending term conforming to the type, and probably of the date of the lowest portion of the Silurian system.

These last-mentioned rocks much resemble the lowest Silurian strata of Pembrokeshire, which also graduate into the Cambrian system, and in which the specific character of the shells is often obliterated or obscured by transverse lines of slaty cleavage. Impressions of crinoidal stems abound; trilobites occur but rarely, and among the shells are two or three which cannot be distinguished from lower Silurian fossils. As however no fixed line of demarcation has yet been established between the lower Silurian and upper Cambrian groups, their zoological contents being, as far as we know, very similar, the place of this member of the Devon series must, for the present, be considered provisional.

The sandstones of this division are in one district pretty abundantly charged with impressions of plants, for an acquaintance with which the authors express their obligations to Major Harding and the Rev. D. Williams, both of whom have sent collections to the Geological Society. Professor Lindley is of opinion that none of these plants are similar to those described in the sequel as common to the Culm Measures: some resemble decorticated *Lepidodendra*, and others *Sternbergia?*. One specimen resembles *Calamites Voltzii* of the Terrain d'anthracite inférieur (Voltz).

The authors conclude their remarks on the whole region by some account of joints.

Dip joints and strike joints abound in all the groups, and though not constant in their inclination are generally inclined at a high angle, separating the great masses into rhombohedral solids. The transverse cleavage planes are not parallel to joints, and are regarded by the authors as forming a distinct class of phænomena.

Chap. III.—Deposits of the fourth region.

The natural groups are determined by help of sections; one from Dartmoor to the coast of Torbay; another from Torbay to Start Point; and a third from Dartmoor to Bolt Head. In describing these sections the authors enter on many details not given in this place, and from a review of the whole division, the following groups, beginning, as before, with the lowest.

1. An ill-defined group near the granite, supposed to be metamorphic.

2. A great complex slate group, with two subordinate calcareous zones, in some places swelling out to a great thickness. The lower calcareous mass (called the Ashburton bands) pass into Cornwall, and range through the greater part of the county; the upper are represented in the most striking form by the Plymouth and Torbay limestone.

3. A coarse red arenaceous group, like the second group of the preceding chapter, immediately surmounts the Plymouth and Torbay limestones, and like them is of enormous thickness.

4. A great schistose deposit, striking with the other rocks in the southern region, nearly east and west. The prevailing dip is south, and it is not much contorted, but at length it is reversed to the north, being thrown off by an anomalous mass of chlorite and mica slate which occupies the promontories of Start Point and Bolt Head.

5. Mica and chlorite slate;—the relation of which to the other part of the series is unknown.

The authors then contrast the two regions above described. In the southern, trap rocks appear occasionally; in the northern they are wanting. The slaty cleavage so common in the northern is wanting in the southern region, though the rocks are in many places so fissile as to make good roofing slate, but in such cases they exfoliate parallel to the stratification.

In comparing the two regions they endeavour, first, to identify the calcareous group of Linton (No. 1 of chapter ii.) with the calcareous bands (No. 2) of this chapter. Secondly, to identify the coarse red group of North Devon (No. 2, chap. ii.) with No. 3 of South Devon. Lastly, to identify the great slate group of South Devon (No. 4) with Nos. 3 and 4 of North Devon. The absence of the calcareous band of North Devon (No. 3) is not considered to throw much difficulty in the way of this classification. By way of general conclusion, the authors consider all the above groups of North and South Devon to be *newer* than the rocks of Snowdon and central Cumberland (lowest part of the Cambrian system), and *older* (with a very limited exception in North Devon) than the Silurian system: they therefore place them in the upper and middle parts of the Cambrian system, from the more ordinary appearance of which they are chiefly distinguished by the greater abundance of calcareous matter and fossils.

The organic remains of the lower strata of South Devon are indeed so very dissimilar from those of the Silurian system that they cannot have been formed in that æra. These fossils will be described and published*.

Chap. IV.—Culmiferous series of the third region.

The authors first describe many sections to prove that the Culm Measures occupy a great trough, and dip away on both sides from

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* The Rev. — Hennah has placed his rich and valuable collection of Plymouth fossils at the disposal of the authors. Mr. Austen, F.G.S., has also contributed largely from the neighbourhood of Newton Bushel. Major Harding, F.G.S., and the Rev. D. Williams, F.G.S., have been the most zealous collectors in North Devon.

the other rocks with which they are in contact; hence whatever may be their age, the Culm Measures are the newest stratified deposits described in this memoir. *Along their northern boundary they rest on the highest group described in* Chap. ii; *and on their southern boundary they partly rest on the granite and partly on the oldest slate rocks of Devon and Cornwall. Hence they cannot form (whatever be the mineralogical appearance) a true passage into the different schistose masses on which they rest.* Again, they are overlaid by no rocks older than the new red sandstone; their age can therefore only be determined by their structure and organic remains.

The authors then describe sections of the Culm series in great detail, showing its enormous thickness and endless contortions; the anticlinal lines generally ranging with the strike, or nearly east and west.

For convenience, the series is divided into two groups.

The lower is made up of dark carbonaceous shales, sandstones, micaceous and siliceous flagstones, and calcareous shale, here and there containing subordinate beds of black limestone. All the *Wavellite* of Devon is found in this lower group. These beds are beautifully brought out both along the northern and southern boundaries.

The upper group is made up of an indefinite alternation of shales and sandstones, variable in structure, but generally rather thin-bedded: commonly, the shales are considerably indurated and resemble "greywacké," but in other places they are soft and earthy like ordinary coal-shale.

The sandstone bands vary much in texture; there are large quarries of them not to be distinguished from coarse coal grits; very rarely they put on a conglomerate form; most frequently they are close-grained, but even in that respect do not differ much from the grit-stone bands in the carboniferous system of a part of South Wales. Pyrites abounds and ironstone is occasionally found associated with the shale and sandstone.

Carbonaceous stains and impressions of plants occur in many parts of this great formation, and thin laminæ of anthracite are common in both the upper and lower group; but large masses of anthracite and beds fit to work for use are only found in the upper. The authors then describe the Culm works in the neighbourhood of Bideford, where three beds have been worked: the best is stated to average nearly four feet in thickness, while in some places it swells out to twenty feet, and in others contracts almost to nothing.

The plants differ essentially from those found in the older rocks, but are all identical with those species which are most abundant in the coal-fields of the central counties of England and in the South Welsh basin, among which *Cyperites bicarinata, Neuropteris cordata, N. gigantea, Pecopteris lonchitica,* and *P. abbreviata* are perhaps the most widely distributed. In their lithological aspect also, and in containing vast quantities of small sedge-like vegetables, these culm-bearing strata of Devon are undistinguishable from the coal measures of Pembrokeshire. No animal remains have been discovered in them, nor in the underlying sandstone (millstone grit), in which negative features these rocks further coincide with those of the Pembrokeshire coal field.

The subjacent black limestone has indeed no exact parallel in England, its organic remains being for the most part peculiar and undescribed; they are all apparently of marine origin. Among them are two genera of large, transversely ovate bivalve shells, one of which has a strong resemblance to a *Possidonia* of the Yoredale series of the Mountain Limestone (Phillips). Another is like, but not identical with *Gervillia laminosa* (Phillips). *Chambered shells* also occur, some of which are considered to be *Goniatites*, a genus which has never yet been found in the Silurian or older rocks, but is most characteristic of the carboniferous system.

In mineral characters this black limestone approaches closely to the *calp* of Ireland, which though now clearly acknowledged to be a part of the carboniferous group, is nearly devoid of characteristic fossils.

As the whole formation is of enormous thickness and exhibits no plants with distinct specific characters in its lower parts, and as the black limestone contains no species of shell absolutely identical with fossils of the mountain limestone, the authors consider the base line of the series as in a position not yet completely ascertained; though they distinctly prove that it never passes down into the older rocks on which it rests. As however, the upper group contains a fine series of vegetable fossils, every one of which agrees specifically with true carboniferous plants, they have no hesitation in placing these culm measures on the same parallel with the true carboniferous series of Great Britain. The evidence of fossils is in favour of the conclusion, and the sections, instead of opposing, confirm it; in short the culm-bearing beds of Devon are identical with the coal measures of Pembrokeshire both in mineral character and organic remains.

CHAP. V.—Granite of Dartmoor, &c.

The jointed structure of this rock is described in detail, and the joints are shown to agree in their direction with those described by Messrs. Fox, Enys, and other geologists in Cornwall. The authors also confirm a remark of Dr. Boase that the same master joints often affect the granite and bedded rocks near it: they show that the granite has in some places broken through the stratified formations without much changing their strike; hence the successive members of the culm measures abut against the granite on the north-west side of Dartmoor. In all such cases, following the beds along the line of strike, they are changed in structure as they approach the granite, the siliceous bands being converted into quartz rock, the shales into Lydian stone, felspar, porphyry, &c. They regard these facts as perfect proofs of the metamorphic nature of the rocks in contact with the granite of Devon. Lastly, they describe granite veins and elvan dykes as traversing the Culm Measures.

The conclusion is, that no rocks in Devon or Cornwall are older than the Upper or Middle Cambrian; that a magnificent development of the Upper Cambrian terminates in the ascending order about the base of the Silurian system; that these rocks are surmounted by an immense culmiferous trough, the upper portion of which is identical in fossils with the upper division of our coal measures; and that the granite is posterior to all these, but probably anterior to the new red sandstone.

A paper was then communicated, "On the upper formations of the New Red System in Gloucestershire, Worcestershire and Warwickshire, showing that the Red (Saliferous) marls with an included band of sandstone, represent the Keuper or Marnes iriseés, and that the underlying sandstone of Ombersley, Bromsgrove and Warwick, is part of the 'Bunter Sandstein,' or 'Grès bigarré' of foreign geologists;" by Roderick Impey Murchison, F.R.S., V.P.G.S., and Hugh E. Strickland, Esq., F.G.S.

In previous communications* Mr. Murchison has shown, that the system of New Red Sandstone in the central counties of England is divisible into four formations. 1. *Marls with salt and gypsum, and one included band of sandstone,* (Foreign Equiv. Keuper or marnes iriseés.) 2. *Quartzose Sandstones*, (Bunter Sandstein, or Grès bigarré.) 3. *Calcareous Conglomerate*, representing the magnesian limestone or dolomitic conglomerate, (Zechstein, &c.) 4. *Lower New Red Sandstone,* (Rothe todte liegende.)

The object of the present communication is to mark, with precision, the distinctive characters of the two upper formations of this system, and to point out how the one can be separated from the other over a wide area, by stratigraphical, lithological, and zoological distinctions.

The rocks are described in descending order.

Red and Green Marls and Sandstone, (Keuper.)—This formation includes all the variegated marls which lie between the lowest beds of Lias, and the uppermost strata of the underlying formation of sandstone.

The highest of these marls graduate into the lias, are occasionally gypseous, and at a depth of about 200 feet beneath the lias, are underlaid by a peculiar sandstone, which appears to have escaped the notice of former observers.

Tracing this rock from the borders of Gloucestershire, through Worcestershire into Warwickshire, the authors show, by various sections at Burg Hill, Ripple, Wallsfarm, Inkberrow, Hervington, and Shrawley Common, that this band, which never exceeds forty feet in thickness, invariably occupies the same stratigraphical position. It is a thin-bedded, hardish, quartzose sandstone, usually of whitish colour, but sometimes tinted light green and red; the grains of sand being frequently cemented by decomposed felspar, and the beds separated by thin way-boards of greenish marl. The courses of stone are of very irregular extension, thinning out amid the marls. The lower strata are sometimes, (as at Inkberrow,) sufficiently thick-bedded to be used as building stones, but the flag-like character prevails (tombstones, &c.).

This thin-bedded sandstone is characterized throughout its course, by a small bivalve shell, somewhat resembling a *Cyrena* in form, but the genus has not been determined. *Ichthyodorulites* occur and seem to belong to the genus *Hybodus* (Agassiz): also teeth of fishes have been observed†.

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* Proceedings, vol. i, p. 471; vol. ii, p. 115.

† It is proposed to call the Ichthyodorulite *Hybodus Keuperi.*

At Shrawley Common, near Warwick, the surface of some of the beds is impressed with foot-marks of an animal, probably a crocodile or saurian, having feet with four claws.

The marls beneath the sandstone are of great thickness, and have been sunk through at Stoke Prior, near Droitwich, to a depth of near 600 feet. Besides gypsum they contain masses of rock salt, and are the sources of brine springs. In piercing these marls no bed of sandstone has ever been met with, and no fossils have been observed. This great marly formation, comprising the fossiliferous sandstone, is compared with the Keuper of Alsace and Suabia and proved to be its equivalent. The discovery of ichthyodorulites of a genus so abundant in the lower lias, but of an undescribed species, is considered by the authors to be a good zoological confirmation of the age of the sandstone, as indicating an approach to the types which characterize the lower lias.

New Red Sandstone, (Bunter Sandstein, Grès bigarré).—The upper beds of this arenaceous formation, rising from beneath the marls, are usually light coloured; the tints of the sandstone varying from dingy yellow, to white and grey, greenish grey and red. These light-coloured sandstones are occasionally covered by thin courses of red sandstone, which graduate into the overlying marl; but they invariably pass down (sometimes by alternations) into the great red sandstone of the central counties, and are thus inseparable from that formation. This order is clearly seen at Ombersley, Hadley, Elmley Lovett, and Bromsgrove, in Worcestershire. This sandstone is lithologically distinguishable from the overlying Keuper sandstone in being softer, much *thicker bedded,* and more micaceous, though like that rock, some of the upper strata are wedge-shaped and inosculate with marl. This lower rock is the same as that described by Mr. Murchison, at Hawkstone and Grinshill in Shropshire, in which localities it is one of the best building stones in the kingdom.

In its range from Ombersley, by Hadley to Elmley Lovett, and again near Bromsgrove, the sandstone contains many *plants,* usually in a state approaching to charcoal, the jet black colour forming a striking contrast to the light-coloured matrix.

Among these plants, the greater part of which are too imperfect to be identified, Professor Lindley has, however, recognized the *strobilus* of a species of *Echinostachys;* (*E. oblongus*) figured by M. Ad. Brongniart as peculiar to the grès bigarré; a portion of a flabelliform palm leaf; parts of dicotyledonous stems with their bark; a broad leaf of some monocotyledon, and a species, probably of *Convallirites,* (Brongn).

As these fossils bear no affinity to the well-known plants of the Keuper, but have on the contrary a strong resemblance to the Flora of the "Grès bigarré," offering in one instance a specific identification with a vegetable peculiar to that formation, the authors have no doubt that this light-coloured sandstone of Worcestershire forms part of the same deposit.

By sections extending from Warwick to the north-west, the sandstone of Guy's Cliff and Leamington is shown to be of the same age as that of Bromsgrove, being also a soft, light-coloured, slightly micaceous and thick-bedded sandstone: and rising up immediately from *beneath* the red marl, it cannot be confounded with the upper or Keuper sandstone, which at Rowington Tunnel and Shrawley Common is seen to *overlie* the great mass of red marl in manner before described.

Portions of bones of saurians abound in what the workmen call the dirt bed of the Warwick sandstone; but the fragments are so mutilated, and generally in such a decomposed state, that they cannot be identified. Plants also occur, but from a similar cause their recognition is very difficult. In addition to the fossils collected by Dr. Buckland, the authors have found teeth of fishes.

As no attempt has been made to prove that the animal found in Guy's Cliff is of the same species as either of the *Phytosauri* of the Keuper of Wirtemberg; the authors throw it out as a probable conjecture, that if ever accurately determined, it will prove of the same species as one of the saurians in the bunter sandstein of the continent.

The sandstone of Warwick is therefore identified with the rock of Bromsgrove and Ombersley in Worcestershire, and Hawkstone and Grinshill in Shropshire, which has been shown to be a portion of the red sandstone representing the grès bigarré or bunter sandstein.

Although assiduous search has been made to discover a calcareous stratum between the two formations above described, which might represent the "Muschelkalk," no traces of such a rock have been detected except in Shropshire, where Mr. Murchison has noticed a band of very impure limestone, occupying the same intermediate position, but as yet no organic remains have been observed in it.

On the whole the authors conclude, that the most exact parallel exists between the upper formations of the new red system of England, and those of a large part of France, where the muschelkalk being also absent, the marnes irrisées and grès bigarré pass into each other in the manner above described*.

XXXIX. *Intelligence and Miscellaneous Articles.*

CARBOVINATE OF POTASH.

MM. Dumas and Peligot by passing a current of dry carbonic acid into a solution of barytes in pyroxylic spirit (*l'esprit de bois*) obtained carbo-methytate of barytes. The production of this compound led them to suppose that the preparation of the carbovinates would be attended with but little difficulty. When, however, this idea was put in practice, they were interrupted by the discovery that although pyroxylic spirit dissolves anhydrous barytes, yet alcohol does not possess this property; they therefore tried whether the use of an alcoholic solution of ammonia would not be attended with success. By passing a current of dry carbonic acid through

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* See the writings of M. Dufrénoy and M. Elie de Beaumont. (*Mémoire pour servir à une de scription géologique de la France,* vol. i. p. 313 et seq.)

a solution of anhydrous ammonia in absolute alcohol a salt was obtained, but it did not possess the properties of carbovinate of ammonia. They then tried the action of dry carbonic acid on a solution of potash which had been heated to redness, in absolute alcohol. As the operation is attended with the evolution of heat, it is necessary that it should proceed slowly and the vessel be kept cold in which it is conducted. The crystalline substance formed by this action soon becomes so abundant as to solidify the solution; a volume of anhydrous æther equal to that of the solution must then be added, and thrown upon a filter. By washing the product with anhydrous æther there remains a mixture of carbonate, bicarbonate, and carbovinate of potash. To separate the last salt the mixture must be washed with absolute alcohol, which dissolves it, and anhydrous æther added to the filtered solution, which reprecipitates it. This liquor immediately filtered and dried *in vacuo* affords pure carbovinate of potash. The analysis of this salt indicates exactly the following formula:

$$KO, C^2O^2; C^8H^4, C^2O^2, HO.$$

This salt is in shining scales. It decomposes by heat into carbonic acid, an inflammable gas, an æthereal fluid, carbonate of potash, and charcoal. Dissolved in water it is rapidly converted into bicarbonate of potash. Dissolved in weak alcohol, or even if it contains only slight traces of water it suffers the same decomposition, and deposits the bicarbonate in shining plates resembling the carbovinate, consisting, however, of

$$KO, C^2O^2; HO, C^2O^2.$$

This rapid and easy conversion of carbovinate into bicarbonate of potash affords but slight hopes of the possibility of isolating the acid, but it is evident that such an acid does exist; and its properties may be interesting as relative to the theory of fermentation. —*L'Institut*, April 19, 1837.

CONVERSION OF IRON INTO PLUMBAGO BY SEA-WATER.

M. Deslongchamps has found lying near La Hogue, where the naval battle was fought, some cannon balls, which although they do not appear externally to have undergone any change, yet have lost two-thirds of their weight and may be cut with a knife like a black-lead pencil: they contain no iron in the metallic state, and exert no influence on the magnetic needle.—*Jour. de Chim. Méd.* Fevr. 1837.

ON A COMBINATION OF THE ANHYDROUS SULPHURIC AND SULPHUROUS ACIDS.

By treating anhydrous sulphuric acid by gaseous sulphurous acid also anhydrous, M. Henri Rose has obtained a liquid possessing an odour resembling sulphurous acid, and which completely volatilizes by exposure to the atmosphere, with the evolution of powerful fumes. This liquid is a compound of the anhydrous sulphuric and sulphurous acids in atomic proportions. Its preparation is not attended with success unless certain precautions are taken. It is particularly necessary to avoid every trace of moisture, for if it be present, this compound decomposes very rapidly even when it is formed, and if not already formed, should one of its constituents contain the least moisture, the experiment will not succeed.

M. Rose therefore conducts the gaseous sulphurous acid into a cooled receiver, then passes it through a tube of at least four feet long and filled with chloride of calcium which has been recently fused. This tube communicates with a glass vessel containing the anhydrous sulphuric acid. This vessel is cooled to about the freezing point of water, but not below it, in order that the product may not contain any free liquid sulphurous acid. As soon as a certain quantity of liquid is formed it must be poured off from the excess of solid sulphuric acid into a small and well-stopped glass vessel. The chloride of calcium in the tube will serve for the preparation of only a small quantity of this compound, and must be re-heated to redness before it is again used.

The liquid thus obtained exhales when in contact with the air extremely powerful fumes resembling sulphurous acid. It volatilizes readily, the residue being a very small quantity of liquid sulphuric acid. If a very small portion of water be put in contact with this liquid, it immediately produces a lively effervescence with a disengagement of sulphurous acid. Dry ammoniacal gas passed into this liquid, a mixture of anhydrous sulphate and sulphite of ammonia is obtained of a yellowish colour, and soluble in water : its solution on the addition of hydrochloric acid disengages sulphuric acid, but does not precipitate sulphur, which only takes place when the liquid is boiled. A solution of nitrate of silver affords a precipitate, which is at first white, becomes yellow, brown, and finally black, particularly by ebullition. M. Rose in the analysis of this compound has ascertained the precise quantity of sulphuric acid, but not that of sulphurous acid, although he endeavoured to determine the latter in many different modes; he was therefore obliged to calculate the quantity of this acid from the loss. The results of the analysis of four distinct preparations are placed according to their age, for it is found that the compound contains more sulphuric acid when it is not analysed immediately after its preparation, a little sulphurous acid being separated; and on the contrary, more sulphurous acid in the opposite case, when it may contain a little free sulphurous acid.

	Weight of the Compound analysed.	Weight of Sulphate of Barytes obtained.	The Proportion of Sulphuric Acid contained in 160 parts of this Compound.
1.	0·529 gram.	1·122 gram.	72·9
2.	0·955	1·945	70·
3.	1·274	2·554	68·91
4.	2·550	5·021	67·68

Thus this combination does not contain, as the author supposed before it was analysed, sulphurous and sulphuric acid in the proportions necessary for the formation of anhydrous hyposulphuric acid ($\dddot{S}+\ddot{S}$) but two atoms of sulphuric acid united to an atom of sulphurous acid ($2\dddot{S}+\ddot{S}$), which by calculation gives in a hundred parts

Sulphurous acid..........	28·58	
Sulphuric acid	71·42	
	———	100·00

As the sulphurous acid is that which exists in the least proportion in this combination, it may be considered as the basic constituent, and in this point of view the compound is a neutral sulphate (bi-sulphate ?) in which the sulphuric acid contains thrice as much oxygen as the base.—*Journal de Pharmacie,* March 1837.

ON GALLIC ACID. BY M. ROBIQUET.

M. Robiquet remarks that before M. Pelouze had published his work on tannin and gallic acid, it was generally admitted that the acid was ready formed in the gall nut, and it was far from being supposed, as attempted to be shown by this chemist, that gallic acid was entirely derived from the tannin. M. Robiquet, after stating some difficulties in admitting this opinion, observes, that whether the acid pre-exists or not in the gall nut, it is certain that a large quantity separates independently of contact with the air or with oxygen, and without any action, if indeed there be one, which occasions the evolution of any gas.

M. Robiquet then details experiments which show that tannin does not yield much above half its weight of gallic acid ; and he observes that there is great disproportion between the time required to convert pure tannin into gallic acid and that required for the entire gall nut. In the latter case a month is sufficient in favourable weather to complete the action. He is therefore of opinion that the gall nut contains other principles, which facilitate the operation by acting as a kind of ferment, and M. Robiquet supposes that the gum, or rather mucilage, which may be separated by water from the residue after the action of æther upon the gall nut, performs this office.

Following out the opinion formerly stated by M. Chevreul, that tannin may be a compound of which gallic acid is one of the elements, M. Robiquet examined whether this idea was probable, and the results were the following: M. Pelouze had inferred from his analysis of tannin that it consisted of $C^{18}H^{18}O^{12}$. M. Liebig has since remarked that the analysis agrees better with $C^{18}H^{16}O^{12}$, and preferred the latter as more easily explaining the conversion of tannin into gallic acid. M. Pelouze nevertheless retained the first formula, and M. Robiquet adopts it as agreeing better with his new view of the subject. Thus, this formula $C^{18}H^{18}O^{12} = {}^{2}(C^{7}H^{6}O^{5} + H^{2}O) + H^{2}C^{4}$ represents 2 atoms of crystallized gallic acid, plus 1 atom of a carburetted hydrogen of the same composition as benzin. The formula adopted by M. Liebig will equally apply to other changes. Thus, the 3 atoms of tannin are equivalent to 6 atoms of gallic acid, plus 2 atoms of dry pyrogallic acid, $3(C^{18}H^{16}O^{12}) = C^{54}H^{48}O^{36} = 6(C^{7}H^{6}O^{5}) + 2(C^{6}H^{6}O^{3})$; or still better, by admitting that tannin may absorb an atom of water, there would result gallic and acetic acid. In fact, $C^{18} H^{16} O^{12} + OH^{2} = 2 (C^{7} H^{6} O^{5}) + C^{4} H^{6} O^{3}$ represents 2 atoms of gallic acid and 1 atom of acetic acid.—*Journal de Chimie Medicale, Mai* 1837.

SPONTANEOUS COMBUSTION OF LINSEED OIL AFTER ITS BECOMING DRY.

The heating of linseed oil when soaking into soft vegetable fibrous or porous matters, has been several times brought into public notice; but we have not observed this effect when the oil has become dry and hard.

A manufacturer at Plymouth had occasion, two or three years since, to grind some red lead in oil, and a cask of it was set aside till it had become hard, and consequently useless, which soon happens to that mixture, red lead being a rapid "dryer." Some months since, being annoyed at this cask lying about the warehouse, he ordered it to be knocked to pieces and the contents powdered, to see if anything could be made of it. This being done in the evening, and the powder put into a box, he was surprised in the morning by a smell of fire, and after searching the warehouse over, perceived smoke issuing from this box; water was thrown in, and when all was cold the contents were turned out. The bottom of the box was found charred, the matter next it brown and partly reduced, and so to about the centre of the mass, from whence it shaded off through chocolate colour to the surface, which retained its redness, but was clotted hard together like all the rest.

The same manufacturer has occasion for large quantities of oiled paper, which when quite dry and no longer adhesive to the touch, he has sometimes put together in piles, but has been obliged to separate them again on account of the heat generated, which has been such as to threaten ignition.

August 5, 1837.

PROCESS FOR INK DEVOID OF FREE ACID. BY R. HARE, M.D. PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF PENNSYLVANIA.*

Writing ink is usually constituted of the tanno-gallate of iron and a portion of sulphuric acid, which had existed in the copperas or sulphate of iron employed as one of its ingredients, the tanno-gallate being suspended and the acid dissolved in the water. This free acid is injurious to iron pens. Dr. Hare has observed that when an infusion of galls is kept over finery cinder till saturated, it forms a beautiful ink, in which of course there is no free acid.

This ink is rather more prone to precipitate than that made with sulphate of iron, and this propensity is not counteracted by the addition of gum arabic. But, on the other hand, it has the advantage of being easily suspended again by agitation, not forming any concrete matter insusceptible, like common ink grounds, of that distribution in water which is necessary to good ink. The tanno-gallate

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* The above and three following notices have been communicated by the author.

of iron when obtained from a filtered infusion of galls and finery cinder, as above described, on being evaporated to the consistency of thick molasses, gum arabic in due proportion having been previously added, forms a pigment which might, it is conceived, supersede Indian ink. When completely dried it glistens like jet with or without the gum.

This tanno-gallate of iron only requires to be dried and ignited at a low red heat, in order to be converted into a pyrophorus. A few years ago Dr. Hare ascertained that, by a similar ignition in close vessels, cyanoferrite of iron, the Prussian blue of commerce, gave a pyrophorus. But as the pure cyano-ferrite of iron, resulting from the addition of the ferro-prussiate of potash, more properly the cyano-ferrite of potassium, to a ferruginous solution did not form a pyrophorus; he was led to believe that the presence of sulphate of alumine in the commercial Prussian blue was the source of the difference, probably by being converted into a sulphide of aluminium, or potassium.

The production of a pyrophorus from the tanno-gallate proves that iron and carbon, when in a state of minute division, are capable, by ignition in close vessels, of acquiring that property of spontaneous combustibility which entitles the body which possesses it to be called a pyrophorus.

In truth these results are consistent with some facts mentioned by Berzelius, as having been ascertained by Mitcherlich, respecting the spontaneous combustibility of iron, reduced from the state of magnetic oxide to that of the pure metal in an extreme state of division. They are also consistent with the spontaneous combustibility of the residue resulting from the ignition of the oxalate of iron at a red heat.

RAPID CONGELATION OF WATER BY MEANS OF HYDRIC (SULPHURIC) ÆTHER AND CONCENTRATED SULPHURIC ACID, &C. BY R. HARE, M.D.

In freezing water by the vaporization of hydric, commonly called sulphuric, æther, there is much labour in pumping, and the ætherial vapour condensing in the pump, disqualifies it for nice experiments until cleansed. Dr. Hare finds that the interposition of sulphuric acid lessens the requisite labour, and protects the pump. By means of a globe or bottle with two tubulures, and a glass funnel with a cock, the acid being in the globe, the water in a retort, and the æther in the funnel, while the two former are exhausted, on allowing the æther to descend upon the water, the congelation of this liquid is instantaneous.

It has been ascertained by the same chemist, that a permanent self-regulating reservoir of chlorine may be made by means of the apparatus heretofore used by him for nitric oxide, substituting for the materials used in that case, manganese in lumps and concentrated muriatic acid.

In one case, Dr. Hare, doubting the purity of the gas, from some indications, among others the want of the usual degree of colour, in order to test it exposed leaves of a thin metal called Dutch gold leaf, to a jet of this gas, as he had previously done repeatedly, without any ill consequence; to his astonishment an explosion took place, which burst the apparatus and produced a detonation as loud as if one of the explosive compounds of chlorine and oxygen had been generated. Yet the only agents employed were peroxide of manganese and chloro-hydric (muriatic) acid. It was the deficiency of intensity in the colour which led him to test it by means of the leaf metal. The colour of the protoxide is known to be of a deeper yellow than that of chlorine.

SYNTHESIS OF AMMONIA. BY R. HARE, M.D.

Understanding that the synthesis of ammonia had been effected by the reaction between nitric oxide and hydrogen promoted by the presence of platina sponge, Dr. Hare, having no knowledge of the process as performed in Europe, succeeded in the following manner in the attainment of that highly interesting result.

Two volumes of nitric oxide and five of hydrogen were introduced into a bell glass with a perforated neck furnished with a cap and cock. At the bottom of a tubulated glass retort, capable of holding about four ounce measures of water, a small heap of platina sponge was made. A leaden pipe communicating with the cock of the bell at one end, and at the other terminating in a copper or glass tube, having a bore about as large as a knitting-needle, was passed through the tubulure so that the orifice of the tube was nearly in contact with the metallic heap. The pipe was made to form an air-tight juncture where it entered the tubulure, and the beak of the retort was recurved so as to be beneath the surface of some water in a wine-glass. The bell being depressed below the surface of the water in the pneumatic cistern, the cock was opened as as to allow the gaseous mixture to enter the retort and displace the atmospheric air. As soon as this was known to have taken place, by the disappearance of the red fumes resulting from the reaction of the nitric oxide and atmospheric oxygen, the gas being still allowed to pass slowly in bubbles through the water in the wine-glass, an incandescent coal was held near the part of the retort supporting the sponge. The metal being thus heated became ignited, and fumes appeared in the cavity of the retort. An absorption of the water in the wine-glass followed, which was however immediately checked by a supply of gas from the bell sufficient to cause the bubbling to recommence and continue. Under these circumstances the water in the wine-glass acquired the odour of ammonia, and gave with copper the well known blue colour.

In a subsequent experiment a small lump of the sponge was secured in a coil of platina wire and fastened to the tube so as to receive the jet of the mixed gases.

Dr. Hare published the fact, some years since, that asbestus soaked in a solution of chloride of platinum and ignited, would cause the inflammation of hydrogen with oxygen. He finds asbestus, similarly prepared, to produce the synthesis of ammonia, either when substituted for the sponge, in the experiment above described, or carried red hot from a fire and passed into a bell glass containing the mixture over mercury.

In fact a piece of charcoal soaked in a solution of chloride of platinum, (choroplatinic acid,) produced effects analogous to the platinated asbestus.

To produce platinated asbestus, it was found sufficient to dip it in liquid chloride of platinum, and then subject the mass to a red heat in a common fire.

ROTATORY MULTIPLIER. BY R. HARE, M.D.

Dr. Hare has contrived a rotatory multiplier in the following way:

Just as the needle, in oscillating, reaches its appropriate position in the meridian, by means of two pins proceeding from it perpendicularly so as to enter two mercurial globules, it completes a circuit through the coil; one end of which terminates in one of the globules. The other end of the coil of the multiplier communicates with one pole of a galvanic pair, of which the other pole communicates with the other globule. The needle is thus subjected to an impulse which causes it to revolve until it receives another impulse by the same process repeated. Each revolution therefore causes an impulse which is productive of a succeeding revolution so long as the galvanic reaction is sustained.

The construction was subsequently improved by employing two coils of copper wire of equal length, separated by paper and varnish, one being wound over the other. They were so arranged that the needle receives two impulses in each revolution, one as above described, the other when its north pole points to the south. Again, two needles associated so as to form a cross are made to complete a circuit every fourth of a revolution, and thus to receive four impulses in one revolution.

METEOROLOGICAL OBSERVATIONS FOR JULY 1837.

Chiswick.—July 1. Cloudy: fine: clear and cold at night. 2, 3. Very dry. 4—12. Very fine. 13. Overcast. 14. Cloudy and fine, heavy showers. 15. Showery. 16. Very fine. 17, 18. Cloudy and fine. 19. Very fine. 20. Fine. 21—26. Very fine. 27, 28. Very hot and sultry. 29. Heavy rain: excessively boisterous in the afternoon. 30. Cloudy: showery. 31. Very fine: showery.

Boston.—July 1. Cloudy. 2, 3. Fine. 4—7. Cloudy. 8. Fine. 9. Cloudy. 10, 11. Fine. 12. Cloudy. 13. Cloudy: rain with thunder and lightning P.M. 14. Cloudy: rain early A.M. 15. Cloudy: rain early A.M.: rain P.M. with thunder and lightning. 16, 17. Fine: rain P.M. 18. Cloudy: rain P.M. 19. Fine. 20. Cloudy: rain A.M. and P.M. 21—25. Cloudy. 26. Fine: rain A.M. 27. Cloudy. 28. Rain. 29. Rain and stormy. 30. Cloudy and stormy. 31. Fine: rain with thunder and lightning P.M.

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

Days of Month. 1837. July.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Max.	Self-registering. Min.	Max.	Min.								
S. 1.	30·336	30·332	30·233	29·82	56·7	75·3	50·0	66	37	61	E.	E.	calm	...	...	...	50
● ⊙ 2.	30·332	30·324	30·190	29·74	58·0	65·2	47·5	76	45	64·5	ENE.	E.	calm	...	...	...	52
M. 3.	30·240	30·242	29·990	29·65	65·3	72·7	57·0	81	48	66	E.	NW.	calm	...	...	...	56
T. 4.	30·142	30·148	30·126	29·54	64·6	75·0	55·7	75	49	63	NE.	W.	calm	...	...	...	58
W. 5.	30·180	30·141	30·091	29·55	63·4	71·5	55·3	78	57	63	SSE.	SW.	calm	...	...	...	53
Th. 6.	30·150	30·184	30·120	29·54	65·6	72·3	60·6	78	51	62	ESE.	W.	calm	·033	...	...	60
F. 7.	30·252	30·240	30·229	29·66	59·5	72·5	55·6	79	50	61·5	NE.	NE.	calm	...	...	...	57
S. 8.	30·196	30·173	30·127	29·58	67·3	70·0	57·7	82	49	66	SW.	E.	calm	...	...	...	60
⊙ 9.	30·164	30·123	30·055	29·63	57·9	76·4	51·6	68	46	60	E.	SE.	calm	...	...	...	50
M. 10.	29·970	29·969	29·931	29·47	61·4	63·9	50·0	77	47	61	E.	SE.	calm	...	...	...	54
☽ T. 11.	29·992	29·969	29·944	29·46	61·5	70·0	52·0	76	53	64	E.	E.	E.	...	...	...	56
W. 12.	29·958	29·999	29·919	29·44	57·7	70·2	54·8	67	48	58	E.	E.	calm	...	...	...	54
Th. 13.	29·818	29·818	29·785	29·26	62·5	64·2	56·9	76	49	63	S.	SW.	W.	...	...	...	56
F. 14.	29·776	29·768	29·700	29·14	63·8	69·5	58·7	81	55	65	S.	SW.	calm	...	·02	·18	57
S. 15.	29·894	29·888	29·847	29·21	65·4	72·2	59·3	74	50	66	S. var.	SW.	SW.	·250	·72	·03	61
⊙ 16.	29·994	29·976	29·950	29·13	64·8	73·2	54·7	75	48	66	SSW.	SW.	W.	·355	·08	·10	59
○ M. 17.	30·044	30·033	29·954	29·37	64·5	72·2	53·5	71	55	63·5	SW.	W.	NW.	·041	·32	·14	58
T. 18.	29·866	29·855	29·823	29·22	64·3	70·8	58·7	73	54	63	SW.	W.	W.	·158	·01	·11	60
W. 19.	29·852	29·835	29·783	29·25	65·4	71·4	59·2	76	53	64	SSW.	W.	calm	...	·12	·15	59
Th. 20.	29·900	29·863	29·699	29·27	64·8	72·0	58·0	71	49	62	W.	W.	calm	·047	·04	·26	60
F. 21.	30·050	30·040	30·017	29·46	63·4	67·3	55·8	73	48	61	WNW.	NW.	N.	·102	...	...	57
S. 22.	30·088	30·082	30·039	29·50	63·3	69·8	56·5	77	50	63	NW.	NW.	calm	...	·02	...	57
⊙ 23.	30·058	30·043	29·988	29·35	68·4	72·2	59·9	82	55	68	SSW.	NW.	calm	...	...	...	62
☾ M. 24.	30·146	30·118	30·100	29·53	63·7	78·3	59·0	74	55	61	NE.	NW.	calm	...	...	...	57
T. 25.	30·126	30·097	30·081	29·50	67·0	72·3	61·3	81	60	60·5	W.	N.	calm	...	...	...	60
W. 26.	30·096	30·062	30·049	29·43	69·3	73·7	64·2	85	47	68·5	S.	SW.	calm	...	...	...	62
Th. 27.	30·018	30·008	29·916	29·34	70·6	78·5	60·3	85	58	65	SSW.	S.	calm	...	...	·02	61
F. 28.	29·818	29·786	29·740	29·13	68·5	79·6	62·3	78	56	62·5	SW.	SW.	calm	...	·30	·02	63
S. 29.	29·350	29·312	29·293	28·72	64·3	74·2	60·2	71	54	60	SE. var.	SW.	S.	·255	·03	·48	60
⊙ 30.	29·544	29·640	29·567	28·86	61·7	68·7	57·0	70	45	61	SW.	W.	NW.	·091	·06	·04	58
M. 31.	29·924	29·893	29·873	29·33	62·8	68·3	53·7	73	53	63	SW.	S.	calm	·102	·06	·35	56
	30·009	29·999	29·937	29·38	63·8	71·7	56·7	75·7	50·7	63·1				Sum 1·434	1·78	1·88	57·5

THE

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[THIRD SERIES.]

OCTOBER 1837.

XL. *On the prepared or peculiar Voltaic Condition of Iron.* *By Sir* JOHN F. W. HERSCHEL, *K.G.H., M.A., F.R.S.*[*]

IN the Number of the *Annales* for the month of March of the present year, (1833, vol. lii. p. 288,) which I have recently received, I find a remark of M. Braconnot upon the manner in which concentrated nitric acid acts, when brought into contact with iron, which brings to my recollection some experiments made several years ago upon the same subject, presenting particularities sufficiently curious to merit a closer examination. I am at present unable to resume my researches, but I think that an account of them will not be uninteresting to your readers, and that it may induce one of them, perhaps M. Braconnot himself, to study in detail the very remarkable phænomena of the action in question, and to connect them with the usual laws of chemical action.

M. Braconnot says, "filings, or if they be prefered, plates of iron immersed in concentrated nitric acid, do not experience the slightest alteration, and retain all their metallic lustre, so that they are thus preserved from rust. If the same acid be made to boil upon these plates, and it be afterwards supersaturated with ammonia, it scarcely deposits a few insignificant flocks of oxide of iron." The following are my own observations. (I extract the experiments from a journal dated August 1825.)

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[*] From the *Annales de Chimie et de Physique*, vol. liv. p. 87, being the paper alluded to by Mr. Faraday in Lond. and Edinb. Phil. Mag., vol. ix. p. 122.; and now inserted to complete the series of papers on the subject to which it relates.

If a piece of soft iron wire, well brightened, be immersed in nitric acid of the density 1·399, the iron instantly becomes brown, and an effervescence more or less lively takes place, attended with the disengagement of red vapours; but this effervescence only lasts a few moments. It very soon ceases, when the iron immediately recovers its metallic lustre, and afterwards remains tranquil and intact at the bottom of the acid for any length of time that may be desired.

Iron thus treated, (which for the sake of brevity, I shall in future call *prepared iron*,) may be withdrawn from the acid and exposed to the air, or immersed in pure water or in ammonia, without by these means regaining the property of being attacked by nitric acid. In its prepared state it may be touched (gently) either in the air or in acid, with gold, silver, platina, mercury, glass, and several other substances without destroying this state. But if its surface be rubbed with force, so as to establish an intimate contact; if, for example, it be scraped with the edge of a piece of glass, upon a glass plate, its state of preparation is then destroyed, and if it be again immersed in the acid, the effervescence followed by total inaction again occurs, and the metallic lustre reappears: in a word, this is a complete renewal of the prepared state. If, on the other hand, prepared iron be touched either with copper, zinc, tin, bismuth, antimony, lead or iron not prepared, when either in the air, water or acid, the prepared state is destroyed, and the action of the acid commences again with effervescence, &c., as usual.

If a rather long piece of iron wire, prepared and moistened with acid, be touched with copper at one of its extremities while it is held suspended in the air over a glass plate, the surface may be seen to become brown, not instantaneously, and all over at once, but successively and by a movement, so to speak, propagated (with rapidity certainly) from the extremity touched to the other extremity. When, in the progress of this embrowning, the limit of the brown colour reaches a drop of acid suspended at an inflection of the wire, effervescence and the complete decomposition of the drop of acid take place. But if a wire immersed in the *acid* be thus touched, the action is instantaneous throughout its whole length.

If these experiments be performed in a capsule containing only a small quantity of acid, and many times repeated, the acid becomes incapable of producing the prepared state in the iron. This effect appears to arise partly from the heat evolved, and partly from the presence of nitrous gas; for having impregnated pure acid with this gas until it acquired a green colour, it was found incapable of communicating the prepared state to iron. A piece of iron, immersed in acid thus impregnated, continued to produce a lively effervescence until it was entirely destroyed.

A piece of prepared iron was immersed in a solution of nitrate of copper. No precipitate resulted, but when it was touched in the solution with a piece of copper, the surface became instantly covered with a thick layer of metallic copper.

Between those states of the acid, in which it is capable and incapable of preparing iron, several intermediate states intervene, in which the preparation of it is effected with more and more difficulty, and the effervescence lasts longer and longer. In these intermediate states the following remarkable phænomenon sometimes occurs: the action ceases for an instant, and then recommences, and that several times in succession, and with convulsive intermittences, which sometimes succeed each other at intervals of $\frac{1}{2}$ to $\frac{2}{3}$ of a second, sometimes with an extraordinary rapidity, so that they cannot be counted. When they are slow, it is easy to see that the cessation of the action is propagated from one extremity of the wire to the other, though a reason cannot always be assigned why it ceases at one extremity rather than at the other.

It often happens that the iron, without acting with vivacity, does not cease to have a brown surface, to colour the surrounding acid, and to give off gaseous bubbles: this slow action may be arrested immediately in a singular manner, by withdrawing the iron from the acid, holding it for an instant in the air, and then letting it fall suddenly with a little shock. In half a second afterwards it seldom fails to shine with all its brilliancy.

The same effect may be produced with greater certainty, if, without withdrawing the iron from the acid, it be touched with a thin plate of platina. The contact of the platina, and, under certain circumstances that of silver also, exercises an inverse influence to that of zinc, &c. &c., in the production of the prepared state, or in its preservation when it exists. Thus, when operating in a capsule of platina, or upon a plate of that metal placed at the bottom of a porcelain capsule, the preparation of iron may be effected, not only with concentrated acid, but with dilute acid, even when diluted with an equal quantity of water. With a larger proportion of water the preparation of the iron is no longer possible, even when there is an intimate contact of the platina; but if a portion of acid be added, the iron resumes its brilliancy and becomes prepared.

Once prepared, the iron resists perfectly the action of an acid diluted to the same, or even to a greater extent; which proves that these phænomena are owing, not merely to the absence of the water necessary to hold the nitrate of iron in solution, but rather to a certain permanent electrical state of the surface of the metal. This mode of considering the subject is confirmed by the following experiments.

A piece of iron wire was heated, and a small zone of wax placed around the middle of it to divide it into two protions. The wire being immersed in the concentrated acid, the action ceased at the same moment upon each half, and upon touching one of its extremities with copper the action was renewed in each simultaneously. The prepared state being again established, the iron was withdrawn by a glass rod attached to the wax, and one of its extremities was touched while it was in the air. The action commenced as usual at the extremity touched, and was extended through half of the wire, but was then stopped by the wax, so that one half was brown, while the other retained its metallic lustre.

A piece of iron, bent into an arc and divided as I have described by wax, was prepared, and then two-thirds of its length withdrawn from the acid, thus leaving the greater part of one half of it, A, still immersed. The other half (B) while thus in the air was touched with copper, when the action was propagated to the wax, where it stopped. The extremity B was then quickly lowered until it touched the surface of the acid; the action commenced immediately in the portion A, which was immersed, and which had hitherto preserved its lustre.

Prepared iron resists the action of the acid, when at a temperature insupportable to the hand, but not at the temperature of ebullition. When it is let fall into very hot acid, it resists for a few instants, and then begins to cause a violent effervescence. I have never found that iron could be submitted to the action of boiling nitric acid without oxidation, as is remarked by M. Braconnot, but perhaps the acid which he employed was more concentrated than mine. On the other hand I have found it impossible to make acid, of the density of 1·399, either cold or at the temperature of ebullition, act upon softened steel (*acier recuit*), or even upon those plates of steel which are employed for watch-springs. It may be kept boiling upon the plates for any length of time, without producing the least effect. But a circumstance, which to me appears very singular, is that a wholly different effect is produced upon steel which has received the highest temper, so as completely to resist the file, it being attacked with extreme violence by the hot acid, and even with considerable facility by cold acid. But when the acid is cold the steel is easily prepared and becomes brown, in the same manner as iron when touched with zinc, but slowly, and, so to speak, with resistance. But if it be prepared and touched alternately several times in succession, it finally becomes subject to intermittences of action, becomes heated, and emits torrents of gas, without there being any possibility of calming the effervescence.

Since these experiments were made, I have found a very curious memoir by Keir, in the Transactions of the Royal Society of London for the year 1790, entitled, *Experiments and observations on the dissolution of metals in acids, and their precipitations;* in which several facts of this kind are recorded.

Keir was led to remark the prepared state of iron, when studying the precipitation of silver by that metal, in which Bergman had previously found some anomalies. He even discovered that this singular state may be developed by the action of nitrous acid. But the remarkable effects arising from contact with other metals, by which these facts may be included in the class of electro-chemical phænomena, escaped his observation. That the contact of one metal should protect another from the action of a chemical agent, as long as the contact lasts, does not now surprise us; (it occurs when nitric acid is poured over a piece of copper placed upon platina;) but what to me appears extraordinary in the experiments above described is, that the effect can be indefinitely prolonged after the contact ceases; and that a permanent electrical state may exist on the surface of a metal, and be there maintained by its own power, contrary to that which ordinarily exists in the same metal, and to that which continues to exist in it at a very small depth in its interior, even during the existence of the forced state at the surface.

Slough, Aug. 19, 1833.

XLI. *A Report on the Progress of Phytochemistry in the Year 1835, in reference to the Physiology of Plants. By* J. C. MARQUART.*

[Continued from vol. xi. p. 166, and concluded.]

WE have received this year very important additions to the knowledge of the alkaloids. What was formerly described as Atropia, Hyoscyamia and Daturia must, according to the discoveries of Brandes, be expunged. They are not alkaloids; this discovery was left for Mein, Geiger and Hesse†.

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* From Weigmann's *Archiv für Naturgeschichte*, vol. ii., part iv., p. 139 *et seq.* Translated by Mr. Wm. Francis.

† Geiger and Liebig's *Annalen,* vol. vii. p. 269.

Chemists, it is true, distinguish the alkaloids from the henbane, the deadly nightshade, and the thorn-apple; they appear to us however so nearly related to each other, that their difference consists in their greater or less degree of purity. One might designate them as the bases from the Solaneæ if the genus *Solanum* did not contain an alkaloid differing from them, and which is principally distinguished from the above by the circumstance, that it causes no dilatation of the pupil, which property belongs to the former in a very high degree. The history of the alkaloids obtained from the genus *Solanum* seems to us yet to be rather in darkness: for the bases from the above-named genera possessing the property of dilating the pupil, a name should be chosen, from one of them. They are contained in all parts of the plants combined with an acid. In their pure state they are colourless transparent prisms with a silky lustre, void of smell, not volatile, and melting at 80° Reaum. A characteristic property for all three is that they lose their property of crystallizing when in contact with water, and then take the narcotic smell of the plants. They may however be reduced to a crystalline state by employing the same method as in their first preparation. They combine with acids forming neutral crystalline salts, and exhibit, especially towards tannin, an extraordinary affinity, as their solutions gelatinize with tincture of galls. We know their elementary composition from the base of the *Atropa Belladonna*; it consists of C_{34} H_{23} O_6 N_1; and the elementary analysis of daturia and hyoscyamia will shortly show whether our supposition above is sufficiently confirmed.

Geiger and Hesse* also separated from the seeds of *Colchicum autumnale* an alkaloid in a condition which showed its nature better. It was formerly considered identical with the base from the genus *Veratrum*, from which it however differs in some degree. Its taste is irritating, but not sharply caustic like veratria, and it does not cause any sneezing. By neutralization with acids it forms in part crystalline salts.

The aconita of the older chemists must also be struck out, according to the recent preparations of Hesse†, who found in the leaves of *Aconitum Napellus* a bitter highly poisonous but not acrid basis, which, in its purest state, forms white granules or transparent masses with a glassy lustre, which dissolves easily in alcohol and æther, but with more difficulty in water. The salts formed by this base with acids were not crystalline.

The alkaloid discovered by Lancelot in the leaves of *Digi-*

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* Geiger and Liebig's *Annalen*, vol. vii. p. 269.

† *Ibid.*

talis purpurea has also been obtained by Radig [*], and recognised as a real alkaloid.

We are yet unacquainted with the origin of the drug known under the name of *Cusco* bark; it belongs according to chemical observations certainly to the bark of some Cinchonaceæ. Winckler[†] prepared the Cusconin which had been discovered in it by Pelletier and Leverköhn. It possesses great similarity to the known cinchonic bases, melts like quinia, is not so easily sublimed, and differs in its elementary composition. Thus cinchonia, quinia, and cusconia appear to be oxides of a radical (C_{20} H_{24} N_2) which contain 1, 2 or 3 atoms of oxygen.

O. Henry[‡] examined the leaves and fruit capsules of *Cinchona micrantha*, which contained no cinchona-alkaloid and no cinchonate of lime; he found these substances, on the contrary, in the sap which had flowed from the stem, and in the root of this tree. We have further received notices from Winckler[§] on brucia and solania, from Vasmer and Geiseler [||] on veratria and strychnia, from Clement Lee [¶] on sanguinaria, and from Peretti[**] on pitoyn.

We must take for granted that our readers are acquainted with the earlier experiments on the chemical history of opium, or the dried milk sap of the unripe heads of poppies, so that we need make but a general mention of the two important papers of Pelletier[††] and Couërbe [‡‡]. This infinite mine of interesting substances lately presented Pelletier with two, one of which showed the same elementary composition as morphia, and was named by Pelletier, Paramorphia; to the other he gave the name of Pseudo-morphia. We can only here speak of the first, since Pelletier did not *always* obtain the second under exactly the same circumstances. Notwithstanding the similar composition of morphia and paramorphia, the latter differs from the first by its solubility in sulphuric æther, by the property of not being coloured blue by salts of iron, and by its incapability of forming crystalline salts with acids; it possesses, however, the nature of an alkaloid, and differs in that respect, as well as by its styptic and metallic taste, and crystalline form from narcotina. It belongs to the most violent poisons, as one grain produced violent rigid spasms and death in a dog.

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[*] Ehrmann, *Pharm. Novellen*, 1834, part 2.

[†] Buchn. *Repert.*, vol. li. p. 171.

[‡] *Journ. de Pharm*, October 1835.

[§] Buchn. *Repert.*, li.

[||] *Archiv für die Pharmacie*, vol. ii. part 1.

[¶] The American Journ. of Pharm., 1835. April.

[**] *Gazetta eclettica di Farmacia*, 1835. No. 8.

[††] Geiger and Liebig's *Ann. der Pharm.*, vol. xvi. part 1.

[‡‡] *Journ. de Chim. Méd.* December 1835.

Pelletier further proved that there could be no doubt as to the existence of the narceia which he had discovered in opium; that the codeia discovered by Robiquet is not the result of the reaction of a certain substance in the opium; that from one and the same quantity of opium, narcotina, morphia, narceia, meconia, codeia and paramorphia might be obtained. Of particular interest to physiology is the examination of opium obtained from heads of poppies cultivated on the grounds of General Lamarque at Eyris, *Dept. des Landes.* Pelletier found in it no trace of narcotina; on the other hand, a much larger quantity of morphia than in the oriental opium. There was also, excepting a small quantity of codeia, none of the other constituent parts of the oriental opium to be found in it. We have, however, received notice that some opium grown in Sicily contained quite as much morphia as the oriental: on the contrary, Winckler* found in the unripe heads of poppies cultivated on the Bergstrasse only a small quantity of codeia, rather plenty of narcotina, and no trace of morphia.

The latest discovery in this part of phytochemistry is the preparation of pure quassin, the long-suspected base (and as such received) of the wood of *Quassia amara*, in which Winckler† succeeded. He obtained it in the form of small colourless columns possessing a dull lustre, which were easily soluble in water and alcohol and insoluble in æther. Their solutions possess an alkaline action, taste exceedingly bitter, and are precipitated by tannin.

The following, to which different names have been given, do not belong to a less distinguished section of vegetable substances. We consider them as the so-called extractive matters in their purest state, which often appear crystalline, dissolve in water and alcohol, at times in æther also, and whose solutions are neutral. The greatest part of De Candolle's ‡ hyperhydrogenous substances, or substances similar to resins, belong to this place: we find however his designation very inappropriate.

Lasch extracted salicin §, with which we were previously acquainted, also from the female catkins and young branches of several species of willows. Tischhauser || obtained it from the bark, and Herberger ¶ from the leaves of willows along with populin, with however four times as much of the former as of the latter. Landerer ** prepared from young oranges cultivated in Greece, the bitter-tasting hesperidin, which was

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* Buchn. *Repert.*, vol. liii. part 3.

† *Ibid.*, vol. liv. part 1.

‡ *Physiologie*, German Transl. by Roeper, p. 327. [Original edition, Paris, 1832, vol. i., p. 350.—Edit.]

§ *Archiv für die Pharm.*, vol. i.

|| *Berliner Jahrbücher*, xxxiv. 2.

¶ Buchn. *Repert.*, vol. li. part 1.

** *Ibid.*, vol. lii. part 2.

discovered by Lebreton: its solution is said to act as an acid, which we doubt. T. Martin* extracted cathartin, which was discovered in the leaves of senna, also from the leaves of an American *Cassia marylandica.* Those cultivated in Europe are said not to contain it. The santonin discovered by Köhler in the calathiæ of *Artemisia glomerata* was more carefully re-examined by Trommsdorff†. It forms dazzling white compressed six-sided columns, which become yellow in the light even in confined air; they melt at 180° Reaumur, and are sublimable in open vessels. It consists according to the elementary analysis of $C_5 H_6 O_1$.

Poggiale ‡ has experimented on the salsaparilla root, and recognised as identical the substances described under the names of paryglin, and salsaparin and parillin acids, and he has given a method of preparing salsaparin in a pure state. It then appears as white small acicular crystals which are decomposed by concentrated nitric acid and form oxalic acid, and are coloured dark red, almost violet, by sulphuric acid. Poggiale found salsaparin to consist of $C_8 H_{15} O_3$. According to Petersen § it contains one atom more carbon than Poggiale has stated, has not a nauseous bitter taste, and does not possess an acid reaction.

This is so far of importance, that the chinova bitter discovered by Winckler in *China nova* (*Buena hexandra?*) has, according to the experiments of Buchner, jun., ‖ the same composition and properties as Poggiale gives to his salsaparin, and Buchner therefore considers them to be one principle, which must appear rather strange of substances from plants so different as *Smilax* and *Cinchona.* This subject therefore requires a close examination.

A very interesting recent discovery is that of the phlorrizin ¶ of De Koninck, which may be obtained in a crystalline state by boiling the bark of apple trees, and then evaporating. It is often contained in the fresh bark up to 5 per cent., and appears to be diffused in the family of the Pomaceæ, perhaps characteristic of them all. It is obtained as whitish needles with a silky lustre, which contain at the common temperature 7 per cent. of water of crystallization. It melts at 103°, and begins to decompose of itself at 193° with formation of benzoic acid. It dissolves easily in boiling water and alcohol, with difficulty in cold water, not at all in æther. Concentrated nitric

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* The American Journ. of Pharm., 1835, April.

† *Annalen der Pharmacie*, vol. xi. part 2.

‡ *Journ. de Pharmacie*, 1834, p. 553.

§ *Annal. der Pharm.*, vol. xv. part 1.

‖ Buchn. *Repert.*, vol. liii. p. 1.

¶ [See Lond. and Edinb. Phil. Mag., vol. viii. p. 444.—Edit.]

acid changes it into oxalic acid, by which circumstance it differs from populin. It consists according to Petersen[*] of $C_4\ H_5\ O_2$, from which the statement of the discoverer, $C_{14}\ H_{18}\ O_3$, widely differs.

Bonastre[†] obtained from the distilled water of aromatic pink, a crystalline substance, which according to its physical properties belongs to this place; it has been named by him eugenin, but according to Dumas it is a hydrate of the æthereal oil of pinks with a certain quantity of mixed water.

What was lately described by Roger as digitalin, is no alkaloid but belongs to this section, and has been called by Radig, who submitted the base from *Digitalis* to a more accurate examination, picrin[‡].

Fremy[§] extracted from the fruit of *Æsculus Hippocastanum*, a substance similar to the saponin of *Saponaria*, which distinguishes itself particularly by the circumstance that when treated with hydrochloric acid, a peculiar acid, æsculic acid, is obtained from it. We did not mention this under the acids, because as yet it is uncertain whether it is a product or an educt.

The berberin discovered by the Buchners, father and son[||], in the bark of the roots of *Berberis vulgaris*, belongs, from its degree of solubility, to this section, and ranges itself near to phlorrizin; it contains however nitrogen, $C_{33}\ H_{36}\ N_2\ O_{12}$, by which it differs from rhabarbarin, to which in other respects it is nearly allied. Buchner brings it under the section of subacids, a reference not very appropriate. It forms a light yellow powder, consisting of fine silky needles, which becomes red at 100° Reaum., and yellow again when it becomes cold; it melts at a higher degree of heat. It is insoluble in sulphuric æther, and petroleum; alkalies and most of the metallic salts form with it orange-yellow combinations.

The peculiar basis of the colour of Lichens was found by Gregory[¶] also in *Variolaria amara*, and must not be confounded with the variolarin mentioned in De Candolle's *Physiologie*, but is more similar to erythrin. This substance is easily soluble in alcohol and æther, less so in water, tastes bitter, loses its bitterness by the action of vapour of ammonia, and becomes red. The orcin discovered by Robiquet in *Variolaria dealbata* is volatile at 100°, and sublimes into a crystalline thick enamel-like mass. Its composition according to Robiquet[**] is $C_{14}\ H_{22}\ O_5$.

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[*] *Annalen der Pharmacie*, vol. xv. part 1.

[†] *Journ. de Pharm.* Oct. 1834.

[‡] *Pharm. Novellen, von* Ehrmann, 1834, part 2.

[§] *Ann. de Chim. et de Physique*, Jan. 1835.

[||] Buchn. *Repert.*, lii. part 1.

[¶] *Journ. de Pharm.*, Juin, 1835.

[**] *Ibid.* Aout, 1835.

Of inorganic substances which have been found in plants we can only mention those remarkable either by their rarity or quantity; and with regard to the latter, we must notice the often mentioned paper of Radig on *Digitalis*, who states that he has found in the herbaceous part 3·7 per cent. of oxide of iron. Also the memoir of Struve[*] on the siliceous contents of some plants. The author determined only the quantity of silica in the ashes; he should have taken into consideration the proportion of silica to the vegetable substance in order to render his labours of more physiological importance. Under this section belongs also *a part* of the crystalline formations discovered in plants, to which De Candolle has given the name of *Raphides*, and, among others, those found by Nees von Esenbeck in the root of *Mirabilis longiflora*, consisting, according to him, of phosphate of lime and magnesia[†]. The examination of these crystals is always connected with many difficulties; and we seldom succeed in obtaining the object so pure as in the case which Fr. Nees von Esenbeck mentions[‡], and as in that where the author [Marquart] obtained from the stem of *Alöe arborescens* pure raphides in such a quantity as to be able to submit them to careful analysis, which convinced him that in this case the acid could not be *of organic nature*, but phosphoric acid combined with lime and magnesia, as in the above-mentioned raphides from *Mirabilis longiflora*. These data do not exclude in any way whatsoever the statements, that crystals of oxalate of lime may also frequently be found in the cells of plants. Among others we may just notice the oxalate of lime in the root of *Rheum australe*, which I however have never been able to see under the microscope in a crystalline form, but more in the state of granular excrescences. Not unimportant to the explanation of the occurrence of salts of difficult solubility in the interior of plants may be the old observation of Vauquelin[§] (almost forgotten) which he made on occasion of analysing forty-seven varieties of potato, and according to which *phosphate of lime dissolves in water*, when some mucilaginous substance, gelatinous starch or animal gelatine, for instance, is mixed with it. The statement of Treviranus[||] that plants which contain raphides when cut attack the knife and blacken it, may be liable to many exceptions; and certainly the occurrence of raphides and the property of blackening the knife, stand in no relation to one another.

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[*] *De Silicia in plantis nonnullis. Dissertatio inaugural. Auctore Struve.* Berlin, 1835. [See also the present volume, p. 13, 17.—Edit.]

[†] *Physiologie der Pflanzen*, vol. i. p. 47.

[‡] Buchn. *Repert.*, vol. xlii.

[§] *Mém. du Mus. d'His. Nat.*, vol. iii. p. 241.

[||] Flora, 1835. No. 26. p. 211.

XLII. *On the Bromo-cyanide and Chloro-cyanide of Potassium and Mercury.* *By* R. H. Brett, *Esq.*[*]

THE iodo-cyanide of potassium and mercury has been described by Liebig[†], according to whom, the mercury in the cyanide is combined with twice as much cyanogen as would be necessary to convert the potassium of the iodide into cyanide of potassium, from which statement it would appear that the double salt is compounded of an atom of iodide of potassium, and an atom of bicyanide of mercury, its symbolic representation would therefore be $K\,I + Hg\,2\,Cy$. The salt in question is deposited in crystals, when saturated solutions of its component salts are mixed together, or still better when an alcoholic solution of iodide of potassium is added to a saturated solution of bicyanide of mercury in water. This salt when collected on bibulous paper and dried possesses a remarkable brilliancy, assuming the appearance of plates not unlike cholesterine; it is heavy, speedily sinking in water, in which, as well as in alcohol, it readily undergoes solution, especially with the assistance of heat. The acids even when diluted decompose it, causing a deposition of biniodide of mercury and evolution of hydro-cyanic acid. The salts about to be described, also compounded of two haloid salts, and which I shall call the bromo-cyanide and chloro-cyanide of potassium and mercury, may be obtained in the following manner.

The Bromo-cyanide of Potassium and Mercury.—If to a solution of the bicyanide of mercury in water, an aqueous solution of bromide of potassium be added, and the fluid set aside, after some time crystalline plates of much brilliancy, and very like cholesterine in appearance, begin to pervade the fluid; if cold and saturated solutions of the salts be employed, the crystalline deposit takes place at once. The readiest mode perhaps of obtaining this double salt is to mix moderately concentrated solutions of the two components, evaporating the resulting fluid to a small bulk, and then transferring the fluid to a convenient glass vessel, to plunge the whole into cold water, by which means the largest amount of double salt is obtained; this should be thrown on a filter and washed with a very small quantity of cold water, then pressed between folds of bibulous paper until all moisture is removed. By somewhat slow crystallization from an aqueous solution, this salt may be obtained in delicate acicular crystals; these under a microscope appear to be flattened quadrangular prisms.

The salt in question is soluble both in hot and cold water, also in alcohol, especially when hot.

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[*] Communicated by the Author.

[†] The iodo-cyanide of potassium and mercury was described by Dr. Apjohn in 1831, Phil. Mag. and Annals, N.S. vol. ix. p. 401.—Edit.

One part of the salt requires about 13·34 parts of water, at the temperature of 65° Fahr., for solution. It is however soluble in less than its own weight of boiling water.

Diluted sulphuric acid does not decompose this salt even when heat is applied, it however undergoes solution.

Concentrated sulphuric acid, sp. gr. 1·845, does not decompose it even upon the application of heat, it suffers solution in the hot fluid, and is not thrown down again by the addition of water.

Neither diluted nor concentrated nitric acid appears to decompose it, although it undergoes solution. Hot nitric acid, sp. gr. 1·48, dissolves it without apparent decomposition, water does not throw it down from such solution.

Hydro-chloric acid, sp. gr. 1·16, dissolves it even in the cold, the hot fluid does not appear to decompose it.

Sulphuretted hydrogen readily decomposes it with the evolution of hydro-cyanic acid, it is also decomposed by the hydro-sulphurets. The caustic alkalies, potass and ammonia, do not decompose it. By the agency of heat it fuses and blackens, suffering decomposition.

In order to determine its atomic constitution, 15 grs. of the salt, previously well dried over a sand-bath, were dissolved in water, to this solution an excess of a concentrated acid solution of proto-chloride of tin was added, and the whole boiled for a few minutes, during which time much hydro-cyanic acid was evolved; the vessel was then corked and kept at rest for some hours, the clear supernatant fluid was then tested with some more of the solution of the salt of tin; no precipitate being produced, the whole was carefully removed; the finely divided metallic mercury at the bottom of the vessel was then boiled for a short time only with pure hydro-chloric acid, after some time the clear supernatant fluid was again removed, and after boiling once more with diluted hydro-chloric acid, and removing the greater part of the clear supernatant fluid, the remainder, together with the metallic mercury, was transferred to a small glass capsule, and allowed to dry at the prevailing atmospheric temperature; the mercury which had run into globules was then weighed.

Fifteen grains of the salt were again taken, dissolved in water, and the solution treated with a slight excess of hydrosulphuret of ammonia, the whole was then boiled for a few minutes, and when cold, filtered; the filter was well washed with distilled water, and the washing being added to the filtered fluid, the whole was evaporated in a porcelain capsule to a very small bulk, the fluid thus concentrated was transferred to a platinum crucible, to which was added the washings of the capsule: the whole was then slowly evaporated over a sand-bath to dryness; the residue was then exposed to a dull red heat for some time and weighed as bromide of potassium.

The following were the results of two experiments.

Exp. 1. Mercury...... 7·5 = bicyanide of mercury 9·43
bromide of potassium 4·50.
13·93.

Exp. 2. Mercury 7·5 = bicyanide of mercury 9·43
bromide of potassium 4·70
14·13.

Calculated proportions, assuming the double salt to be compounded of an atom of each of its constituents.

Bicyanide of mercury ... 10·241
Bromide of potassium ... 4·758
14·999.

Now as the quantity of mercury contained in the calculated proportion of bicyanide is about 8·144, the difference between it and the quantity obtained by actual experiment is only 0·644, for 8·144 − 7·5 = 0·644. In the 2nd experiment the quantity of bromide of potassium obtained very closely approaches to the calculated proportion—as 4·70 to 4·758. It may, I think, therefore be fairly deduced that the double salt is composed of an atom of each of its constituents, and may be represented thus: $\mathrm{K\,Br} + \mathrm{Hg\,2\,Cy}$, or

Bicyanide of mercury 1 atom = 254
Bromide of potassium 1 atom = 118
372 atomic weight of the salt.

In 100 parts..................... 68·279
——— .... 31·721
100·000.

The *Chloro-cyanide of Potassium and Mercury* differs scarcely at all in appearance from the salt last described, and may be obtained precisely in the same way, substituting the chloride for the bromide of potassium. It is however more soluble in water than the bromo-cyanide, one part of the salt requiring only 6·75 parts of water at 65° for solution. The mineral acids and the alkalies do not decompose it. It is however readily decomposed by sulphuretted hydrogen and the hydro-sulphurets.

In order to ascertain its atomic constitution it was analysed in the same manner as the last salt.

15 grs. of the salt yielded by the 1st experiment,
Mercury 8·6 grs. = 10·81 of bicyanide of mercury.
3· of chloride of potassium.
13·81

2nd Experiment.
Mercury 8·6 = 10·81 bicyanide of mercury.
3·40 chloride of potassium.
14·21

By calculation, assuming the salt to be composed of an atom of each of its constituents.

11·291 bicyanide of mercury.
3·708 chloride of potassium.
14·999

The numbers obtained by experiment approach sufficiently near to those which result from calculation to warrant the conclusion, that the double salt is composed of an atom of each of its constituents, and may be thus represented: $\mathrm{K\,Cl} + \mathrm{Hg\,2\,Cy}$, or,

Bicyanide of mercury 1 atom = 254
Chloride of potassium 1 atom = 83·42
337·42 atomic weight of salt.

In 100 parts .... 75·277
——————— .... 24·723
100·000.

I may observe that in these experiments and calculations, the atomic weight of chlorine has been taken as 35·42, that being the number somewhat recently announced by Turner, and agreeing nearly with that deduced from the experiments of Berzelius.

July 3, 1837. R. H. BRETT.

P.S. Since sending the preceding analyses for insertion in the Philosophical Magazine, I have been informed by Mr. R. Phillips, that the bromo-cyanide of potassium and mercury has been described, and an analysis given by M. Caillot in the *Journal de Pharmacie*, tome xviii. p. 351. This salt is called by Caillot the cyano-hydrargyrate of the bromide of potassium, believing as he does that the bicyanide of mercury plays the part of the electro-negative or acid element, whilst the bromide of potassium takes upon itself the electro-positive, or base function: his mode of obtaining the salt is much the same as that which I have described. M. Caillot considers that it contains 8·74 per cent. of water of crystallization. Now the atomic weight of the salt according to my experiments is 372, and if the above per centage of water of crystallization be taken as correct, it will be found that 372 parts of the anhydrous salt will combine with 35·62 parts of water, which is very nearly equal to 4 atoms; this agrees with the statement of M. Caillot, who considers the quantity of water in his salt as equal to 4 atoms. The proportions of the constituent salts in 100 parts as given by Caillot, is very nearly the same as that given in my analysis: according to him the proportions are as follows: Bicyanide of mercury 68·49; bromide of potassium 31·51. M. Caillot says that diluted nitric acid when mixed with the salt decomposes it, forming nitrate of potass and bi-bromide of mercury, and disengaging hydrocyanic acid; and certainly, reasoning from the known action of many of the acids upon the iodo-cyanide of potassium and mercury, we might *à priori* have expected this. In my own experiments, however, I did not find it to be the case, for when the salt was carefully prepared, so that no uncombined bi-cyanide of mercury was present, the cold, and even concentrated mineral acids did not disengage any hydrocyanic acid, neither did the same acids when hot; a prolonged digestion with or without heat would in all probability effect a decomposition, and it is more than likely that this was done in M. Caillot's investigations.

I cannot find that the chloro-cyanide of potassium and mercury has been described. Being at present engaged in the investigation of the salts formed by the combination of bicyanide of mercury with certain haloid salts of the alkaline metals, as well as with the organic alkalies and the chloride and bromide of ammonium, I hope to make them the subject of a future communication.

R. H. B.

XLIII. *On the Complexion of the Ancient Egyptians.* By CHARLES T. BEKE, *Esq.*, *F.S.A.*[*]

THE main difficulty which has had to be contended with, in the consideration hitherto given to the subject of the colour of the ancient Egyptians, is this;—that, whilst the only conclusion which we are warranted in drawing from the descriptions given of that people by the earliest writers of Greece, is, that they were in outward appearance almost si-

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[*] From the Transactions of the Royal Society of Literature, vol. iii. Part I.; read before the Society on March 24th, 1836.

milar to the African negroes of the present day;—our knowledge, derived from all other sources of information, is, or at least seems to be, diametrically opposed to such a conclusion.

The chief of these conflicting testimonies may be thus summarily stated. On the one side we have;—first, a passage of the poet Æschylus, in which the crew of a vessel, seen at a distance, are said to be known for Egyptians by their black colour:—

> *Πρέπουσι δ' ἄνδρες νήϊοι μελαγχίμοις*
> *Γυίοισι λευκῶν ἐκ πεπλωμάτων ἰδεῖν.*—Suppl. 727, 8.

and, secondly, the testimony of Herodotus, who personally visited Egypt, and who, consequently, must have known full well the real colour of its inhabitants. It is true, that the historian's opinion upon the subject is to be gathered rather by inference, than from any express description given by him of the complexion of the Egyptians; but this circumstance only renders the inference the stronger, as we are thereby led to believe that their colour was a matter of sufficient notoriety among the Greeks, to make the express mention of it unnecessary. Of the two passages of this writer which are to be adduced, the one is that wherein he explains the tradition that the oracle at Dodona originated in a black dove which had flown from Thebes in Egypt, by supposing that the oracle was instituted by a Thebæan female; and that the circumstance of the bird being black, showed that the woman was of Egyptian origin: *μέλαιναν δὲ λέγοντες εἶναι τὴν πελειάδα, σημαίνουσι ὅτι Αἰγυπτίη ἡ γυνὴ ἦν·* (Euterpe, 57.) the other passage is in the account given by him of the Colchians (Euterpe, 104.), in which the historian asserts his belief in their Egyptian descent, because they were *of black complexion and woolly-headed*, *καὶ ὅτι μελάγχροές εἰσι καὶ οὐλότριχες.*

As opposed to the conclusion which is to be arrived at from the consideration of the foregoing authorities, we have, on the other hand;—first, the testimony of the Hebrew Scriptures, from which testimony (although indeed it is of a negative character only) it is unquestionably to be inferred, that the people in whose country Joseph became naturalized, so that his brethren believed him to be a native of it (comp. Gen. xlii. 23. 30. 33.);—with whom alliances were permitted by the Israelitish lawgiver (Deut. xxiii. 7, 8.);—one of which people was, in fact, the mother of the heads of two of the tribes of Israel (Gen. xli. 50—52.), and another of whom was, at a later period, the wife of King Solomon (1 Kings, iii. 1.);—could not possibly have been of a much darker complexion than the Israelites themselves. Had such been the case, we should indubitably have met with some mention of the fact, or at least with some reference or allusion to it, similar to that which is made by the prophet Jeremiah (xiii. 23.) respecting the colour of the Cushites.

Secondly: in times when the communication between Egypt and Europe was common and uninterrupted, and when so remarkable a peculiarity, had it existed, could not have failed to be noticed, we have the like negative evidence of the later writers of Greece and Rome. It is true, that there is a description given by Lucian, in one of his Dialogues, ('Navigium, seu Vota,') of a young sailor on board an Egyptian vessel, who, besides being *black*, is represented as *having pouting lips and spindle-shanks*; —— οὗτος δὲ πρὸς τῷ μελάγχρους εἶναι, καὶ πρόχειλός ἐστι, καὶ λεπτὸς ἄγαν τοῖν σκελοῖν: but, from the consideration of the context, it is impossible to regard this description as applicable to the Egyptians generally: on the contrary, it would seem rather that the individual in question ought to be regarded as having differed in appearance even from the rest of the crew of the vessel, having been perhaps a negro or Nubian slave: besides which, it is evident that the whole description is so caricatured, that much of its value as an authority is lost.

Thirdly: in the paintings which have been discovered in the temples and tombs of Upper Egypt, the natives of the country are usually represented as being of a chocolate or red copper colour, which we may reasonably infer to have been their actual complexion at the period when those paintings were made. The human faces, too, painted upon the mummy-cases, which likewise may be assumed to be representatives, although not likenesses, of the individuals whose bodies are contained in those cases, are of a similar coppery hue.

Fourthly: the naturalists who have investigated the physical structure of the skulls of the embalmed bodies, have determined, that they possess none of the decided characters of the negro; and that, indeed, they differ but little in formation from the European races of mankind*. The hair, too, upon the heads of many of these bodies, is found to be totally unlike the woolly hair of the negroes; it being, in fact, of a soft and smooth texture, like that of Europeans. From the bodies themselves no opinion is to be formed of their natural colour, owing to the changes which the process of embalming has necessarily caused in them: neither is any certain conclusion to be deduced from the colour of their hair, which is not unfrequently brown; since it is possible that that colour may not be natural, but may have been induced by the same process.

[* See Phil. Mag. First Series, vol. lxvi. p. 71; and Phil. Mag. and Annals, N.S., vol. v. p. 59, 64.—EDIT.]

Lastly: we know full well, that in the present day the complexion of the natives of Egypt is far from being black; and that, in reality, they possess the general physical characters of an European or Asiatic, rather than of an African race.

What, then, is the conclusion to be come to under this conflicting evidence? It seems utterly impossible to reject altogether the testimony of Æschylus and Herodotus, and especially of the latter, by which the fact is established, that, at about 500 years before the commencement of the Christian æra, the complexion of the natives of Egypt, if not actually black, was at all events of so dark a shade, that such an epithet might not improperly be applied to it among the fairer inhabitants of Greece: and if we admit this fact, there appear to exist no means of reconciling it with the other evidences which have been enumerated, except by the hypothesis which is advocated in my '*Origines Biblicæ**;' namely, that the natives of ancient Egypt were derived from two distinct original stocks; the one, and the earliest possessors of the country, being of Ethiopian descent, who entered Egypt from the south; and the other being the people who are mentioned in the Hebrew Scriptures under the name of מִצְרַים (*Mitzrím*), or Mitzrites, who, in all the translations of those Scriptures, from the Septuagint downwards, are incorrectly called Egyptians; and their country, Mitzraim, is, in like manner, improperly designated Egypt; and whose original country was not any portion of Egypt itself, but was situate wholly to the eastward of the isthmus of Suez.

The former of these two peoples was, as may well be conceived, of a race which came from the south, of a dark colour, approaching to, if not actually, black; and it is to this people that are applicable not only the descriptions of Æschylus and Herodotus, but also (see '*Orig. Bibl.*' p. 295, note) the allusion of the prophet Jeremiah;—the Cushites, or Ethiopians, and the primitive Egyptians being in fact identical.

The latter people, the Mitzrites, being sprung from an Arabian and northern stock, would not have been of much, if any, darker complexion than the Israelites themselves; and hence we can satisfactorily account for the absence in the Hebrew Scriptures of all reference or allusion to their colour.

A remarkable exemplification of the distinction which thus existed between the Egyptians and the Mitzrites, is afforded by the comparison of a notice of Ælian concerning the former people, with a statement contained in the Hebrew Scriptures respecting the latter. The Roman writer informs us, that

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* '*Origines Biblicæ*, or Researches in Primeval History,' vol. i. London, 1834.

the Egyptians used to boast that their women were not confined to their beds by childbirth, but could immediately after their delivery resume their domestic avocations: — *Εἰ δὲ Αἰγυπτίων αἱ γυναῖκες μέγα φρονοῦσιν, ὅτι κἀκεῖναι τὴν ὠδῖνα ἀπολύσασαι, καὶ ἐξαναστᾶσαι, τῶν ἔργων ἔχονται τῶν κατὰ τὴν οἰκίαν.* '*De Nat. Animal.*', lib. vii. c. 13 *. On the other hand, we learn from the Scriptural history, that among the people over whom the oppressor of the Israelites reigned, childbirth was far from being so easy:—"because the Hebrew women are not as the Mitzritish women, for they are lively, and are delivered ere the midwives come in unto them," (Exod. i. 19.) is the excuse of the midwives who were commanded by Pharaoh to destroy the new-born male infants of the Israelitish mothers.

It will be right here at once to anticipate an objection, which might be made in accordance with the opinion entertained by J. D. Michaelis, (see his '*Supplem. ad Lex. Hebr.*' in voc. חַיָּה) that the word in the text, חָיוֹת, should be pointed חַיוֹת, and translated *midwives*; whence the passage would have to be read, "because the Hebrew women are not as the Mitzritish women, *for they are themselves midwives*," &c.; or, as the Vulgate has it, "ipsæ enim obstetricandi habent scientiam;" in which sense the expression in question is understood in many other ancient versions. To this objection a sufficient answer is given by Rosenmüller (*Scholia in loc.*); namely, that as throughout the whole relation the Mitzritish midwives are called מְיַלְּדוֹת, and there does not appear any reason why the historian should employ two different words to express one and the same idea, the meaning attached by Aben-Ezra to the word in question, (and adopted also in our authorized version,) namely, "lively," "robust," is to be preferred. Jarchi says, that the Rabbis understood the expression to mean, "because they are like the beasts of the field, which

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* My attention was called to this subject, in its present form, by the perusal of the article 'Ægyptus' in 'Lempriere's Classical Dictionary,' (by Barker, second edition, London, 1832,) in which, § 8, 'On the Complexion and Physical Structure of the Egyptians,' pp. 43—46, the various authorities which are thus far cited are collected and commented upon; the principal matter of the remarks being apparently taken from Dr. Prichard's 'Physical History of Mankind;'—a work, to which I have not at present the opportunity of referring. As might be supposed, however, no satisfactory conclusion could be arrived at by the author, whilst the notion of the identity of Mitzraim and Egypt was retained. My own conclusions, upon all material points connected with the subject, had been expressed, though not in this developed form, in my '*Origines Biblicæ*,' long previously to my perusal of the article in Lempriere.— 20th July, 1835.

bring forth without assistance;" which comes to the same thing.

But admitting for a moment that Michaelis's construction be the correct one, it is still manifest that among the Hebrew women childbirth is stated to have been so easy, that they could dispense with the aid of the midwives; and that, in fact, they were able to deliver themselves. This assertion may, or may not, have been true: from Exod. i. 17. it would seem rather that it was not so, and that, on the contrary, the Mitzritish midwives did actually assist the Hebrew mothers; but that they "feared God, and did not as the king of Mitzraim commanded them, but saved the men-children alive." The truth, or untruth, of their assertion is however entirely immaterial to the consideration of the present question, which relates to the Mitzritish, and not to the Hebrew women. Now, as the midwives expressly told Pharaoh that the Hebrew women could dispense with their assistance, and that in this respect "they were not as the Mitzritish women," it was equivalent to the assertion, that the latter, on their part, did require such assistance: and as this excuse was allowed to pass current with the tyrant, (which would scarcely have been the case, had it been untrue in this respect also,) it affords the strongest evidence of the physical character of the Mitzritish women in this particular; and hence the distinction is sufficiently established between them and the women of Egypt, as described by Ælian.

For various other arguments in confirmation of the distinction existing between the land of Mitzraim of the Hebrew Scriptures, and the Egypt of profane history, it is sufficient for me to refer to the work already alluded to.

The separation and distinction between the Egyptian and Mitzritish nations continued (there is reason to consider) until about the time of the Israelitish king, Solomon. At that period, wars between them ensued, in which the Mitzrites were at first the conquerors; but after a time, the Egyptians regained their independence, and in the end acquired the supremacy. Of these occurrences we have manifest traces in the corrupted and distorted fragments of Egyptian history which have come down to our time, although the period when they took place is thrown back to a much earlier date. The country of Mitzraim being thus subjected to the dominion of Egypt, and being further devastated by continual aggressions on the part of the Assyrians and Babylonians, whilst, from its peculiar locality, it was obnoxious to the desolating action of physical causes also, became gradually deprived of its political existence, and at length was merged and altogether lost in its more prosperous neighbour, Egypt. That Herodotus and other writers should not in any manner allude to the separate existence of Mitzraim, is, in reality, not more remarkable than that they should omit all mention of either of the neighbouring kingdoms of Judah and Israel; whilst the corrections which have been made in the early history of most nations, when they have been subjected to the test of extensive research and severe criticism, plainly show how little dependence is to be placed upon the unsupported traditions and fables of native writers, who are but too often found to be willing to enhance the antiquity and glory of their country at the total sacrifice of the truth.

The natural result of the union between the Egyptians and Mitzrites would have been an amalgamation, to a certain extent, between the two races, and (as we see continually instanced in the present day) the offspring of connexions between them would, in complexion and other physical characters, have been intermediate between the two parent stocks. It may be added, that besides the partial change of colour which would hence have ensued, many important alterations in the customs of the Egyptians must necessarily have been consequent upon their original subjection by the Mitzrites. Among these is particularly to be mentioned the introduction of the practice of embalming the dead; a custom, which we are expressly told was not of Ethiopian origin, (see Herod. Thalia 24. Diod. ii. 14. Strabo xviii. 23.) but which we know to have been common among the Mitzrites as early as the time of the patriarch Joseph. (See Gen. l. 3. 26. 30.) A corollary upon this will be, that no Egyptian mummies can be of a date anterior to the Mitzritish invasion of Egypt, (circa 1000 B. C.): many, nay, most, of those which have, up to the present time, been brought to Europe, are manifestly of the period of the Ptolemies only.

But, in addition to the cause of variation in the colour of the Egyptians which has already been mentioned, another cause, and one of which the results would have been yet more perceptible, had, about two centuries previously to the time of Herodotus, begun to operate: this was the introduction of Greek settlers by Psammeticus, and the encouragement which was given to the immigration of that people during three whole centuries previous to the accession of a Greek dynasty to the throne. Subsequently to this latter event, and whilst, during three centuries longer, the Ptolemies continued sovereigns of Egypt, the inducement to Greek settlers became still greater; and thus, during the long period of six centuries next preceding the commencement of the Christian æra, continual additions of European blood would have been made to that of the already mixed breed of the Egyptians and Mitzrites. Hence the complexions of the inhabitants of Egypt must necessarily have become fairer and fairer in each successive generation; so that, at the time when the Romans acquired the supremacy of that country, its natives, or at all events those of Lower Egypt, would have been little, if at all, darker in colour than the inhabitants of the neighbouring countries of the Levant. I will say nothing of the immense number of Jewish emigrants in Egypt; although, not improbably, they likewise would have aided in bringing about this result.

Whilst however these changes were gradually taking place, it is evident that shades of colour of every degree, from the darkest up to the very fairest, would have existed at one and the same time among the inhabitants of Egypt, in the same manner as, in the present day, we find to be the case in places where the population is compounded of European whites and African blacks; and these diversities must, to a certain extent at least, have continued to exist in the time of the Ptolemies, and even of the Cæsars;—and this, doubtless, to a greater degree in the southern than in the northern portions of the country. Hence it may not be unreasonable to imagine, that cases would sometimes arise, in which it might be considered advisable, in documents of a legal nature, to state, not merely the names, descriptions, and ages of the parties to them, (in like manner as is customary in such documents now-a-days,) but also their complexions. That the complexions of the parties to legal documents were sometimes described, is clear, from the Greek papyri translated by Dr. Young ('Discoveries in Hieroglyphical Literature,' London, 1823, pp. 66. 69.); and without attaching any undue importance to this circumstance, I think it is at least deserving of consideration in the light above suggested.

It is however most natural that the lower classes would have been those whose blood derived the smallest proportion from an exotic source, and who, consequently, would longest have retained the physical characters of the primitive Ethiopian stock: hence it is not impossible, that the young Egyptian sailor, described by Lucian, may have been an individual of the lowest class [of the Egyptians]; although, as before stated, it is more probable that he was a negro or Nubian slave.

In thus attributing the origin of the primitive Egyptians to a black African stock, I must however be distinctly understood as opposing the notion, that the type of that stock is to be sought for in the negro of the present day. On the contrary, I conceive the negroes of Africa to be the descendants, in an extremely low state of degradation, of the primitive people, who first entered that continent by the way of Ethiopia, and who were possessed of a much higher degree of cultivation than the Egyptians themselves; for it is manifest, that this latter people, instead of advancing, were, until the period of the arrival of the Greeks, gradually descending the scale of civilization, and that the state of manners described by Herodotus and other writers, (like that which we observe in the Chinese, among whom imitation is almost all that is left in the place of the intelligence possessed by their predecessors,) was the natural result of that degeneracy, which, when unchecked, is inevitable to human nature.

I am aware, that in this hypothesis of the original separate existence of Egypt and Mitzraim, I am directly opposed to the results which are considered to have been arrived at, upon indisputable premises, by the many learned persons who have devoted themselves to the study of Egyptian antiquities. With the highest opinion of the value and importance of the materials collected by them, which cannot fail to be of the greatest service to future investigators of this interesting subject, I cannot but feel convinced that they have been engaged in the propping-up of a system of Egyptian history, which, being founded upon altogether erroneous principles, must ultimately fall to the ground and be entirely abandoned.

In the three papers of mine which had the honour of being read before the Royal Society of Literature, on the 15th of January, 19th February, and 11th June, 1834, (as well as in my '*Origines Biblicæ*,') I have expressed my conviction that the writings attributed to Manetho are not authentic. This conviction is only strengthened, the more I have occasion to investigate the subject of ancient Egyptian history; a proper insight into which will, I feel, never be acquired until those writings are deprived of authority, and of that appearance of truth which they have derived from the coincidences said to have been found between them and the results of the system of hieroglyphical interpretation discovered by Dr. Young, and adopted by M. Champollion le jeune.

That in the time of Josephus, these writings were, among the Egyptians, or rather among the Greeks and Jews, who composed at that period the most important portion of the inhabitants of Egypt, (see '*Joseph. cont. Apion.*' lib. ii. § 3.) believed to be the composition of such an individual as the Sebennite priest, can in no wise be taken as conclusive evidence of their authenticity. On the contrary, when we call to mind the distracted state in which Egypt had existed during so many centuries preceding that time, and the changes which had taken place in the government, the manners, and even in the lineage of its inhabitants, we can have no difficulty in conceiving that much of the real history of the country was forgotten and lost, and that in its place, the traditions of the Jewish settlers (nay, any fables that they may have invented, of which an example is to be seen in the story of the composition of the Septuagint version,) may have met with a ready reception; and hence, that the whole system of Egyptian history should have been remodelled, so as to tally with those traditions. The want of agreement between the history thus formed, with the particulars of the ancient history of Egypt afforded by Herodotus and Eratosthenes, and even by Diodorus, (though this last writer, from his much later date, had acquired a mixture of the false and the true,) afford the strongest proof of the little reliance which ought to be placed upon the former; and this independently of the conclusions arrived at from other sources, which are directly opposed to the statements bearing the name of Manetho.

That coincidences are said to exist between these writings of Manetho and the results come to by M. Champollion and the disciples of his school, is, I fear, only so much the more in disfavour of the phonetic system of interpretation; and it may perhaps give validity to the opinion, that that system has not merely been stationary, but has actually retrograded, since the death of its illustrious founder; and that, in order to cultivate it with the prospect of ultimate success, it will be necessary to go back to the point at which it was left when science sustained so severe a loss through his untimely death, and from that point to pursue future investigations of the subject, upon the same philosophical and solid principles on which his splendid discovery and its subsequent development were based.

January 4, 1836. CHARLES T. BEKE.

XLIV. *On the Influence of the Rotation of the Earth on the Currents of its Atmosphere; being Outlines of a general Theory of the Winds.* *By Prof.* H. W. DOVE *of Berlin.*

[Continued from p. 239, and concluded.]

I WILL now pass from these direct and indirect observations to the following stricter proofs, calculated from the mean motions of meteorological instruments. The calculation of thermal and barometrical wind-means shows that the wind-compass (*windrose*) possesses two poles of pressure and of heat, *i. e.* that there are two points nearly opposite to one another in it, at one of which it is coldest, and at which the barometer stands highest; at the other warmest, and at which the barometer stands lowest. The barometric and thermal wind-means decrease uninterruptedly from the maximum of the pressure to its minimum, as also from the maximum of heat to its minimum. The first point lies near to NE., the other near to SW. If we now proceed from SW. through W. to NE., the mean conditions of the thermometer decrease, while the mean conditions of the barometer increase; if we go further from NE. through E. to SW., the mean conditions of the thermometer increase, while those of the barometer decrease.

That which shows itself in the thermal and barometric wind-means must also appear in the transitions of the same into one another, *i. e.* in the mean thermal and barometric variations, and indeed as well on the supposition of a changeable velocity of rotation as of one which always remains the same (Poggendorff's *Annalen*, vol. xxxi. p. 473). Since however the elasticity of the aqueous vapour, with respect to its distribution in the wind-compass, is closely related to the thermal, and the pressure of the dry air to the barometrical, wind-compass; it follows, that the changes of the pressure of dry air and of the barometer are exactly in inverse proportion to the changes of the temperature of the air and of the elasticity of the aqueous vapour contained in it. If we now suppose as the necessary consequence of the former theoretical observations, that the NW. acts the same part in the southern hemisphere as the SW. in the northern, that a SE. there represents a NE. here, we shall have the following result:

A. *Mean Variations of the Meteorological Instruments.*

Northern Hemisphere.	*Southern Hemisphere.*
1. The barometer falls by E., SE. and south winds, passes with SW. from falling into rising, rises with W., NW. and north winds, and passes with NE. from rising into falling.	1. The barometer falls with E., NE. and north winds, passes with NW. from falling into rising, rises with W., SW. and south winds, and passes with SE. from rising into falling.
2. The thermometer rises with E., SE. and south winds, passes with SW. from rising into falling, falls with W., NW. and north winds, and passes with NE. from falling into rising.	2. The thermometer rises with E., NE. and north winds, passes with NW. from rising into falling, falls with W., SW. and south winds, and passes with SE. from falling into rising.

3. The elasticity of the vapour increases with E., SE. and south winds, its increase changes with SW. into decrease, it decreases with W., NW. and north winds, and with NE. its decrease changes into increase.	3. The elasticity of the vapour increases with E., NE. and north winds, its increase changes with NW. into decrease, it decreases with W., SW. and south winds, and with SE. its decrease changes into increase.
4. The pressure of the dry air decreases with E., SE. and south winds, its decrease changes with SW. into increase, it increases with W., NW. and north winds, and with NE. the increase changes into decrease.	4. The pressure of the dry air decreases with E., NE. and north winds, its decrease changes with NW. into increase, it increases with W., SW. and south winds, and with SE. its increase changes into decrease.

What both hemispheres possess *in common* consists in this, that the variations of the meteorological instruments are the same for east winds in the northern hemisphere as for east winds in the southern hemisphere. The same is the case with the west winds. The *difference* of both hemispheres is only quantitative with NW. NE. SW. and SE. winds; qualitative, on the contrary, with north and south winds; *i. e.* the variations of the meteorological instruments are on an average in the northern hemisphere greatest with NE. and SW. winds, smallest (by compensation of the opposite motions) with NW. and SE. winds; in the southern hemisphere smallest (by compensation of the opposite motions) with NW. and SE. winds; greatest on the contrary with NE. and SW. winds. The variations with north winds in the northern hemisphere are however, according to the sign, different from the variations with north winds in the southern hemisphere under equal climatic influences, but, according to the magnitude, are the same in both. If therefore an instrument in the northern hemisphere rises with N., it falls with N. in the southern, and *vice versâ.* This applies also to the south winds.

The proof of the positions above-stated for the northern hemisphere are scattered here and there in separate memoirs; I will here bring them again together in order to have a better general view of them. The confirmation or refutation of the positions given for the southern hemisphere must be set aside until made known by journals of observation.

(+) signifies rising, (−) signifies falling.

1. The barometer falls with E., SE. and south winds, passes

with SW. from falling into rising, rises with W., NW. and north winds, and passes with NE. from rising into falling.

	Paris.		Danzig.	London.
	5 Years.	10 Years.	15 Years.	3 Years.
SW.	$+0^{\text{mm}}\cdot 1200$	$-0^{\text{mm}}\cdot 2079$	$-0'''\cdot 088$	$-0''\cdot 023$
WSW.	+0 ·0362	+0 ·0674	+0 ·157	
W.	+1 ·0788	+0 ·9992	+0 ·059	+0 ·011
WNW.	+1 ·1679	+1 ·3622	+0 ·483	
NW.	+1 ·2153	+1 ·1573	+0 ·491	+0 ·032
NNW.	+1 ·1060	+1 ·3714	+0 ·663	
N.	+0 ·4746	+0 ·2941	+0 ·375	+0 ·049
NNE.	−0 ·1140	−0 ·1633	+0 ·076	
NE.	−0 ·1414	−0 ·2329	+0 ·311	+0 018
ENE.	−0 ·7890	−1 ·1633	−0 ·097	
E.	−1 ·0911	−1 ·2702	−0 ·078	−0 ·012
ESE.	−1 .2999	−1 ·3935	−0 ·022	
SE.	−1 ·2090	−1 ·1704	−0 ·122	−0 ·049
SSE.	−0 ·6924	−1 ·1575	−0 ·386	
S.	−1 ·0057	−1 ·1350	−0 ·515	−0 ·048
SSW.	−1 ·1602	−1 ·1306	−0 ·500	

The variations in Paris are calculated in millimetres for twelve hours; in the first 5 years the direction of the wind at midday was observed, in the last 5 years the mean of the entire day. The observations made at Danzig are variations in sixteen hours expressed in Parisian lines; those made at London in English inches from morning to evening, without closer determination of the hour of observation. I have omitted the inconsiderable correction for the daily variations at Danzig.

The magnitude of the variation is in Paris for the morning and evening observation,

In the 5 years	−0·336	−0·01
In the 10 years	−0·2785	+0·0075
In London	−0·007	−0·002

a. The independence of the phænomenon *on the daily periods* is evinced by the circumstance that the motion depending thereon in the observations of Paris and London, is eliminated by bringing them together with opposite signs. In Danzig this independence follows from the circumstance (Pog. *Ann.*, vol. xxxi. p. 477) that the variations from 6 o'clock in the morning to 10 at night, coincide completely, as regards the sign, with the variations from 10 at night to 6 in the morning.

b. The *independence on the yearly periods* is evinced by the following table, which contains the motions in 12 hours from observations made at Paris for 10 years for the eight chief winds.

Variations in Millimetres.

	NE.	E.	SE.	S.	SW.	W.	NW.	N.	Correction.
Jan.	−0·259	+1·336	+1·081	+1·141	+0·641	−1·400	−2·842	−0·684	0·409
Feb.	−0·365	−0·627	+1·273	+0·057	+1·104	−1·040	−1·506	+0·129	0·596
March.	+0·129	+0·842	+1·956	+1·750	+1·004	−1·058	−2·869	−0·964	0·377
April.	+0·317	+1·156	+1·301	+0·274	−0·659	−0·863	−1·671	−0·291	0·492
May.	−0·267	+2·640	+1·299	+0·979	−0·449	−0·486	−0·321	+0·478	0·343
June.	+0·480	+1·276	+1·509	+1·999	−0·046	−0·718	−0·080	+0·020	0·346
July.	+0·944	+2·144	+1·081	+1·117	−0·036	−0·762	−1·477	−0·144	0·390
Aug.	+0·221	+0·721	+0·775	+1·333	−0·550	−1·065	−0·518	−0·566	0·525
Sept.	+0·454	+1·515	+1·707	+0·361	+0·251	−1·732	−0·324	−0·053	0·366
Oct.	+0·046	+1·273	+0·381	+0·419	−0·188	—0·438	−0·928	−1·639	0·249
Nov.	−1·238	+0·269	+1·034	+1·440	+0·310	—0·796	−1·656	−2·085	0·202
Dec.	+0·250	+0·182	+0·982	+2·333	−0·120	—1·808	−2·013	−1·208	0·233
Year.	+0·233	+1·270	+1·170	+1·135	+0·208	—0·999	−1·157	−0·294	0·286

(—) signifies rising, (+) falling.

The import of the signs is here the opposite to that of those in the other table, in order that they should coincide with the monthly determinations for Danzig.

The calculation mentioned under the former table for Danzig, will be found in Pog. *Annal.*, vol. xxxi. p. 468. It is, however, known that the mean direction of the wind undergoes a periodical change within the year, which will appear from the following table, in which the angles, calculated according to Lambert's formula, are enumerated from south, as zero, towards west.

	Paris.	Danzig.	Paris.	Danzig.
January ...	69°	50° 24′	WSW.	SW.
February ...	50 57′	60 19	SW.	WSW.
March	86 17	84 20	W.	W.
April	109 27	120 53	WNW.	WNW.
May	61 42	141 30	WSW,	NNW.
June.........	118 49	138 29	WNW.	NW.
July	89 56	107 22	W.	WNW.
August	90 18	82 43	W.	W.
September .	66 53	71 46	WSW.	WSW.
October ...	22 45	37 16	SSW·	SW.
November .	52 32	54 47	SW.	SW.
December...	40 1	48 1	SW.	SW.

From the combination of the results of these two tables it follows, *that the law of the rotation of the wind which is evinced in the barometrical variations, is independent of the variations of its mean direction.*

c. The independence of the law of rotation on the differences of the yearly mean directions of the wind of individual places will then first be accurately proved when we possess similar observations for several places. The greatest difference among the places of observation above mentioned amounts, in relation to the meandirection of the wind, to 30 degrees.

d. The rising and falling of the barometer with the various winds is so closely connected with the mean distribution of the atmospheric pressure in the wind-compass (the so-called barometrical wind-compass), that, if two maxima and two minima occur in it, even in the intermediate wind there will be observed a twofold rising and a twofold falling (Pog. *Ann.*, vol. xxxi. p. 478.). It must however be observed that such anomalies disappear much sooner in the rising and falling than in the mean barometrical values of the winds (Pog. *Ann.*, vol. xi. p. 556). The barometrical mean conditions observed during the influence of the wind acknowledge the existence of a barometrical wind-compass, but deny the existence of the law of rotation, and may therefore be said to acknowledge the phænomenon which evinces itself indistinctly and rejects the one which is clearly evident.

2. The thermometer rises with E., SE. and south winds, passes with SW. from rising into falling, falls with W., NW. and north winds, and passes with NE. from falling into rising.

Observations for 5 years in Paris (1816 to 1820) give in 6 hours.

S.	−0·36 C.	N.	+0·06 C.
SSW.	−0·15	NNE.	+0·22
SW.	−0·62	NE.	+0·61
WSW.	−0·78	ENE.	+0·93
W.	−0·76	E.	+0·79
WNW.	−1·29	ESE.	+2·36
NW.	−0·13	SE.	+1·37
NNW.	−0·50	SSE.	+0·89

(+) signifies rising, (−) signifies falling.

3. 4. That the variations of the pressure of the dry air is in proportion to the barometrical variations, and the variations of the elasticity of the vapour, on the contrary, in proportion to those of the temperature, will be evident from the following table, which is deduced from observations in the same years as the barometrical observations of London.

	Dry Air.	Elasticity of Vapour.
W.	+0″·011 E.	0″·000 E.
NW.	+0 ·039	−0 ·007
N.	+0 ·063	−0 ·014
N.E.	+0 ·023	−0 ·005
E.	−0 ·002	−0 ·010
S.E.	−0 ·055	+0 ·006
S.	−0 ·050	+0 ·004
S.W.	−0 ·025	+0 ·002

(+) signifies increase, (−) decrease.

B. *Hydrometeors*—In three memoirs contained in these Annals (Poggendorff's) viz. "On the connection of hydrometeors with the variations of the temperature and of the barometer," (vol. xiii. p. 305), "On thunder-storms," (vol. xiii. p. 419), and "Some remarks on rain," (vol. xxxi. p. 541), I have specially proved that the atmospheric precipitates depending on the direction of the wind, whether they appear as rain, hail, or snow, whether accompanied with disengagements of electricity or not, are nothing less than a necessary consequence of the law of rotation. Since there is at present no reason to modify the positions laid down in those papers, and as these may rather be extended to the southern hemisphere, if we consider the line drawn on it from SE. to NW. as a line of division of the west and east side, a repetition in this place of what has been stated in them, seems to me quite unnecessary.

C. *Currents which alternately expel each other.*—In winter, which is especially the time, when, on account of the greater differences of temperature in the districts situated to the north or south of the places of observation, all phænomena of weather are for the greatest part occasioned by the wind, I have found by comparison of the direction of the wind, as observed below, with the passage of the higher clouds and the direction of the finer fibres of the cirrus, that with SW. and NE. winds the under direction of the wind coincides with the passage of the highest clouds; that on the contrary with W. and NW., with E. and SE., the direction of the vane and of the lower cumuloid clouds is at right angles to the upper direction of the wind. Besides this I have remarked, that when after a barometrical minimum with SW., the wind veers to the west, and then goes round to north, dark mountainous cumulostrati advance from the western horizon immediately preceded by a cold wind, which raises the barometer, and which in winter is combined with falls of snow, in spring with hail-showers, and in summer with thunder-storms. This phænomenon is in general very frequently repeated, while the cirrus apparent in the superior regions of air through the intermediate space of the inferior masses of clouds continues undisturbed in its direction of SW. to NE.; with each new precipitation the barometer rises by sudden jerks (*sprungweise*); the inferior formation of clouds however continues gradually to ascend; at last the covering of the clouds breaks; the cirrus above disappears in like manner with a quick transit of the vane through N. to NE. The vane remains at NE., the sky is entirely clear, and the barometer as well as the cold have attained their maximum. As soon as the barometer begins to fall, fine cirri appear on the dark ground of the sky in the direction of S. or SW. to N. or NE., which gradually condense to that white envelope, which is admirably favourable to the formation of halos round the sun and moon, which therefore are justly regarded as signs of bad weather. The vane indicates with falling barometer E. and SE., therefore at right angles to the direction of the cirrus. If cumuli are in the lower regions of the atmosphere, they are gradually taken up by the evidently descending cirrus, and it then often rains in winter when it freezes hard below. The vane passes rapidly through S.; it rains as in general with a stormy SW.

From these observations I conclude: *That there are two opposite winds, which blow throughout the whole atmosphere.*

These winds I call *air-currents*, the one the *northern*, the other the *southern*. From the observations previously mentioned it follows that the phænomena of the *west side* are a transition of the southern current into the northern; and in fact the driving out of the southern current by the northern first takes place in the lower regions of the atmosphere, and then in the higher. The phænomena of the *east side*, on the contrary, are a transition of the northern current into the southern; and the expulsion of the northern current by the southern takes place first in the higher regions of the atmosphere, and then also in the lower. Both westerly and easterly winds therefore have often above them southerly ones; but with the difference, that with west winds the upper direction of the wind is driven out by the lower, with east winds the lower by the upper.

The difference of the density of their masses of air, proceeding from the difference of heat in both currents, is the cause of this phænomenon.

The more rapidly they expel each other, the stronger the precipitates become during the transition of the one into the other. The southern warmer current, in order to expel the heavier northern one, must possess a considerable intensity.

The greater this is, the greater also becomes the difference of the rapidity of rotation of the places at which it arrives one after the other.

The wind springs back more frequently between S. and W. on the northern hemisphere than between N. and E.; and the more frequently also, the more violently it rages.

If the southern current has prevailed for some time exclusively at one place, and if it is finally expelled by a colder, and therefore denser polar current, this takes place with greater violence. Violent storms therefore pass from SW. through W. into NW. First, then, when the wind becomes N. and NE., its violence decreases. The extremes of meteorological instruments do not coincide with N. and S., but rather with NE. or E. and SW. or W. Then, as for instance, the NE. is properly a north which comes from more northern regions than the N. it must be colder, drier, and heavier than the latter, &c. The individual character of single periods is occasioned by these currents. Its occurrence in the southern currents (an excess of SW. and W. winds) brings mild winter and cold summer, with frequent and considerable precipitations; its occurrence in the northern current, on the contrary, causes a cold winter and hot summer with great dryness. In the first case the warm equatorial air, which rushes towards the pole, advances into higher and higher degrees of latitude, its capacity for vapour continually diminishing; and thus it loses its vapour by continually repeated precipitations. In the second case, however, the cold air of the quietly flowing northern current comes gradually into lower degrees of latitude; its capacity for vapour gradually rises, and during the continuance of this current, the sky remains serene as with a NE. trade wind.

If the two currents meet one another, not at a greater or smaller angle, but lie exactly opposite each other at their meeting, they concuss one another often for a long time. If the currents possess no considerable intensity, there originates at the place of their meeting, especially in autumn and winter, a thick fog, which frequently disappears all at once, and reappears, accordingly as the place of observation is situated on the northern current, or on the boundaries of the southern one. If the southern current is of considerable violence, it flows above the opposing northern current, and prevails in the upper regions of the atmosphere, while in the lower regions there is a calm. The barometrical minimum here observed will find its explanation in the dreadful storms which prevail in southern districts. Barometrical maxima arise from a jostling of the northern current with the southern opposing one.

It is evident that since an equilibrium so produced cannot be a lasting one, barometrical maxima and minima lie in general close to each other : such irregularities generally, after various previous oscillations, balance each other.

If the equatorial currents be detained by a high land situated in the south, the quiet and dry sea of air of such districts will be moved principally by the influx of waves of the west and east winds traversing the parallels. Besides it is evident that the number of east winds will here increase, while the number of west winds will decrease.

The range of the barometrical oscillations of such districts can be but very small.

The *necessity* of the origin of equatorial currents in the temperate zones has already been treated of in a former memoir on monsoons and trade winds. The frequent occurrence of the SW. winds in comparison with the southern wind is a proof that the equatorial currents, when they are first observed at the surface, could not have originated there, but must have come down from above. M. von Buch has also mentioned in support of this view, proposed by Halley, such convincing reasons, grounded on observations made at the Peak of Teneriffe, that it has been received by almost all naturalists. The necessity of such westerly winds following from the constancy of the rapidity of rotation of the earth has recently been made so evident by Herschel, jun., that it will be proper to cite here the words of the author.

"The constant friction thus produced between the earth and atmosphere in the regions near the equator must (it may be objected) by degrees reduce and at length destroy the rotation of the whole mass. It is easy to see in the present case where and how the compensation takes place. The heated equatorial air, when at length it returns to the surface, in its circulation, which it must do more or less in all the interval between the tropics and the poles, will act on it by its friction as a powerful south wind in the northern hemisphere, and a north-west in the southern, and restore to it the impulse taken up from it at the equator."

The view developed in this memoir respecting the phænomena relative to wind which occurs in the temperate zones, removes, if I mistake not, the objection raised against the theory of the trade-winds, that the influence of the rotation of the earth could not be so considerable in the latitudes in which they prevail, because it must be much stronger in higher degrees of latitude than between the tropics. The objection in effect is none, for I think I have demonstrated that that which is required does really exist. We can also easily account for the fact, that, if we pass from Southern Europe to Northern, the westerly winds generally prevail at the expense of the south-westerly.

For the positions stated in C, proofs will be found in some of my previous memoirs. They might be considerably augmented in number, which however does not seem to me necessary.

XLV. *Account of some Experiments made in different Parts of Europe, on Terrestrial Magnetic Intensity, particularly with reference to the Effect of Height.* By JAMES D. FORBES, *Esq., F.R.SS. L. & E., &c., Prof. Nat. Phil., Edinb.*

[Continued from p. 260, and concluded.]

30. BUT to return to the calculation of the first group of observations, those including the alpine country of Switzerland, Savoy, and Italy. If we arrange the observations relatively to Geneva as a fundamental station, taking the data from Table VII. and writing the equations of condition in the form (3.) art. 28, where a_1 denotes the excess of northern latitude of the given station above that of Geneva in minutes; b_1 the excess of *eastern* longitude in minutes of a degree; c_1 the excess in height, reckoned in hundreds of English feet in round numbers (using of course negative signs to represent the reverse of all this), we shall have the following equations of condition, distinguishing from one another the absolute numbers obtained by the two needles, in order that they may be separately calculated.

TABLE VIII.

Equations of Condition for the Alpine Series.

		By No. I.	By "Flat."
Geneva (Aug. 1832)	$0'x+0'y+0'z+\delta I'=$	·002	·005
Geneva (Nov. 1832)	$0x+0y+0z+\delta I'=$	—·002	—·005
Mont Salève	$-6x+2y+32z+\delta I'=$	·001	·002
Mont Breven	$-16x+41y+71z+\delta I'=$	·013	·024
5. Chamouni............	$-17x+43y+21z+\delta I'=$	·009	·005
Jardin	$-17x+50y+77z+\delta I'=$	·007	·009
Col des Fours	$-27x+36y+76z+\delta I'=$	·012	
Aoste	$-28x+71y+6z+\delta I'=$	·020	·018
St. Bernard........	$-20x+61y+68z+\delta I'=$	·006	·000
10. Martigny..........	$-6x+56y+3z+\delta I'=$	·007	·004
Interlaken	$30x+103y+6z+\delta I'=$	—·008	—·009
Schmadribach	$19x+104y+39z+\delta I'=$	·003	—·002
Grindelwald (1.) ..	$26x+113y+24z+\delta I'=$	—·001	·000
Grindelwald (2.) ..	$26x+113y+21z+\delta I'=$	—·004	·007
15. Meyringen	$32x+123y+7z+\delta I'=$	—·001	
Grimsel	$22x+130y+49z+\delta I'=$	—·002	—·007
Münster	$18x+128y+29z+\delta I'=$	·002	—·003

TABLE VIII.—(*continued.*)

			By No. 1.	By "Flat."
	Gemmi	$13x+88y+62z+\delta I'=$	·002	—·002
	Frütigen	$24x+89y+10z+\delta I'=$	—·003	
20.	Faulhorn	$28x+111y+76z+\delta I'=$	—·005	—·011
	Engelberg	$37x+138y+21z+\delta I'=$	—·005	
	Surennes	$37x+144y+64z+\delta I'=$	—·005	—·009
	Klus (near Altorf)	$37x+150y+3z+\delta I'=$	—·004	—·010
	St. Gothard	$22x+145y+58z+\delta I'=$	—·005	—·009
25.	Locarno	$-2x+159y-6z+\delta I'=$	·008	·008
	Orta	$-25x+135y-3z+\delta I'=$	·016	
	Bellaggio	$-12x+187y-6z+\delta I'=$	·019	·019
	Reichenau	$37x+195y+7z+\delta I'=$	·000	
	Wallenstadt	$55x+191y+z+\delta I'=$	—·007	—·008
30.	Lucerne	$51x+130y+2z+\delta I'=$	—·008	—·013
	Rigi-Culm	$51x+140y+46z+\delta I'=$	—·014	

31. From these thirty-one equations of condition for needle No. I, and twenty-four for the flat needle, we obtain by the method of least squares the following values of the four unknown quantities, the calculations having been verified by independent methods.

	No. I.	Flat.
x = Variation of intensity for 1′ of latitude N increasing	—·000364	—·000505
y = Variation of intensity for 1′ of longitude E increasing	+·000055	—·000106
z = Variation of intensity for 100 English feet of height	—·000033	—·000027
$\delta I'$ = Correction applicable to the registered intensity at Geneva	+·0016	—·0040

32. To deduce from these numbers the lines of equal horizontal intensity, we must remark that the minute of longitude is shorter than the minute of latitude in the ratio of $7\frac{1}{4}$ to 10 nearly, on an average, in the Alps. The variation of y for a geographical mile or minute would therefore be

For No. I. = +·000076. For "Flat" = +·000146.

And the angle made by the isodynamical lines with the meridian towards the east from north would be

$$\text{Arc whose tang.} = \frac{364}{76}, \text{ and arc whose tang.} = \frac{505}{146}$$

$$= 78^\circ\ 12' \text{ and } 73^\circ\ 52'.$$

33. Of these results I conceive that the former is to be preferred. The discrepancy of the results obtained by No. I. and "Flat" are, I presume, attributable to one or both of two causes,—a progressive change in the magnetic state of the needle somewhat different from what has been allowed for,—and a slight error in the correction for temperature, which, during the period of observation (the autumn), was generally

diminishing. Now both these points being best ascertained for No. I., I prefer abiding by its indication. In fact, it appears by Table VII. that the intensity of the flat needle decreased from August to November (by the Geneva observations) faster than the mean rate of decrease allowed; the consequence of which would necessarily be, that the standard intensity at Geneva for purposes of comparison would be assumed too high, and, as the general order of the observations lay southward and eastward, the apparent increase of intensity in those directions would be smaller than the true, which would give rise to an error of the kind mentioned in Art. 31. The stability of No. I. renders its indications the most certain. The agreement as to the effect of height is very satisfactory, considering the minuteness of the quantity. The source of error just alluded to would scarcely affect this result. The most probable intensity for Geneva will be 1·0776 for No. I.*, and 1·0670 for the flat needle. The results are projected in the map†.

34. The observations in the Pyrénées lie within smaller compass, and were chiefly conducted with a view to deduce the influence of height. The sources of local error arising from metalliferous deposits are, however, perhaps greater in this case.

35. Proceeding exactly as before, taking Luz in the valley of Lavedan or Baréges, Hautes Pyrénées, as our point of reference, we obtain the following equations of condition from Table VII., which may be arranged exactly as in Table VIII. incorporating the results of *both* needles. In this case the longitudes being westerly, the variation in longitude must be reckoned the opposite way from that in the former case.

TABLE IX.

Equations of Condition for the Pyrenean Series.

Luz (1.)................	$0'x + 0'y + 0z + \delta I' = {\cdot}000$
Luz (2.)................	$0x + 0y + 0z + \delta I' = -{\cdot}001$
Ste Marie..............	$8x - 13y + 4z + \delta I' = -{\cdot}004$
Pic du Midi	$4x - 6y + 72z + \delta I' = -{\cdot}005$
Pic de Bergons (1.)......	$-x - 2y + 45z + \delta I' = -{\cdot}010$
———— (2.)......	$-x - 2y + 45z + \delta I' = -{\cdot}010$
Gavarnie	$-8x + y + 21z + \delta I' = +{\cdot}001$
Brèche de Roland	$-10x + 0y + 69z + \delta I' = -{\cdot}001$

36. Combining these by the method of least squares, we obtain the following values:—

$$x = -{\cdot}000210$$
$$y = +{\cdot}000100$$
$$z = -{\cdot}000053$$
$$\delta I' = -{\cdot}0028.$$

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* The intensity varies ·01 for 27′5 of latitude.

† See p. 375.—EDIT.

Hence it appears, that on the same parallel of latitude the intensity increases in a *westerly* direction, which is the reverse of the result found for the course of the isodynamic lines in the Alps; but, in truth, I do not attach much importance to these observations, unless for the sole consideration of height, on account of the small area of country over which the observations were made. There were probably in the Pyrénées some sources of local disturbance which the observations on the Pic de Bergons particularly indicate, and which, having been repeated with coincident results, could not be owing to an error of observation*. At the same time it is satisfactory to find that the influence due to height is the same in direction, though greater in amount than that obtained in the alpine series. On this subject I proceed to offer some remarks.

37. The first experiments which seem to have had even remotely in view the question of the decrease of magnetic intensity with height are those of Saussure, made during his memorable stay on the Col du Géant in 1788. The observations were too rude, and differ too widely from each other to deserve much confidence; but those made at Chamouni and on the Col du Géant, which were fortunately under almost the same temperature, agree very closely, but give a slightly *greater* intensity to the latter, which is the effect due to the latitude†. The great diminution of intensity in going from Geneva to Chamouni, observed by Saussure, is certainly erroneous, as the reverse has been shown to take place.

38. In 1804 M. Gay-Lussac performed his celebrated aërostatic ascent, and from his magnetic observations concluded that no appreciable difference of intensity existed at the surface of the earth and at the height of 23,000 feet. This, however, can only be considered as referring to great and palpable change. The difficulties inseparable from the experiment prevented many oscillations from being observed, or great precision in the times from being attempted, whilst corrections for arc, diurnal variation, and temperature, were not applied. The last of these, however, could hardly fail to

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* Since this passage was written, on mentioning to Professor Necker of Geneva the anomalous result as to the direction of the isodynamic lines in the Pyrenees (anomalous, decause differing from the supposed direction inserted in Hansteen's map, which is deduced from analogy, and not, I believe, from direct observations in that country), he pointed out the curious (though perhaps accidental) coincidence which this result offers to the views he has long entertained as to the general parallelism of the lines of geological elevation, and those of magnetical intensity, which the bearing of the isodynamic lines which I have given for the Alps remarkably confirms.

† Saussure, *Voyages aux Alpes*, § 2103. Tom. iv.

be sensible, the variation of temperature being no less than 36° Cent., and as cold tends to increase the apparent intensity, if no such increase was observed, it might plausibly be argued that the *real* intensity had diminished. It must, however, be observed, that the observations lasted only in general from one to two minutes, and that in so short a period (and depending on a single value of the elapsed time) the acceleration due to the above-mentioned cause would hardly be perceptible. Taking the mean result of the effect of temperature ascertained by myself for No. I. and "Flat" needles, we find the factor $-\cdot00037$ applicable to the time for a decrease of 1° R. of temperature, which agrees exactly with Hansteen's mean correction. If we apply this to Gay-Lussac's observation we find a correction of $-\cdot0108$ as a factor applicable to the time, for the effect of $-36°$ Cent. of temperature. Yet large as this is, amounting to $\frac{1}{100}$th part of the whole, the discrepancies of observation often amount to double that quantity*. Still we admit with M. Kupffer that the probability deducible from M. Gay-Lussac's observations, is in favour of a slow diminution.

39. The next series of observations includes those of Humboldt and Gay-Lussac, recorded in the *Mémoires d'Arcueil*†, which include observations made in the Alps, though at no great heights; and here no particular influence of height was observed, nor was indeed looked for‡.

40. Since that period the subject seems to have met with little practical attention, until the recent publication of M. Kupffer's "Voyage au Mont Elbrouz" by the Petersburg Academy. From his observations with a needle by Gambey, half a metre long, M. Kupffer attempts to deduce, not only the fact, but the amount of the diminution with height, and this upon the authority of a single experiment, and at no considerable elevation§. In fact, all the intricate corrections which this delicate observation requires were little more than guessed at. The difference of geographical position of the two stations (12′ in lat. 38′ in long.) was allowed for by observations made with a different apparatus,—the effect of temperature was deduced from indirect experiments, far from

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[*] See the details of the observations in the *Annales de Chimie. An* xiii. (1805), tom. lii. p. 75.

[†] Tom. i. p. 1.

[‡] *Ibid.* p. 10.

[§] The observations were not made at the *summit* of Mont Elbrouz, as stated in the *Annuaire du Bureau des Longitudes*, 1836, p. 288, but near the foot of it, and the difference of height of the stations was less than 5000 English feet. The stations were "Pont de Malka," and "Hauteur de Kharbis."

presenting a mutual agreement; and the whole difference of level (4500 French feet) offered a very small basis for so general a conclusion. But, besides this, there is an oversight in M. Kupffer's deductions (first pointed out to me by Professor Necker of Geneva), which tends yet further to diminish the probability of his conclusions. The estimate of the effect of geographical position on the magnetic intensity, M. Kupffer conceives to be such, that the variation for 12′ in lat. (diminishing from the lower to the upper station) would exactly counterbalance the variation due to 38′ in E. long. (also diminishing from the lower to the upper station); the one increasing the duration of an oscillation as much as the other diminished it. Now it appears from his own statement on the preceding page (Memoir, p. 87), as well as from the known direction of the isodynamic lines, that these variations conspire with one another, so as to render the anomaly attributed to height *greater* than before. The upper station is S. W. of the lower, the direction of the isodynamic lines is from N.W. to S.E., consequently the variation of position is such as would diminish the time of vibration of the needle, whilst in effect it was found to be increased. From M. Kupffer's data, I find that the time of one vibration of his great needle by Gambey ($24^{s}\cdot05$ nearly) would be *diminished* about $0^{s}\cdot104$ for the change of latitude and longitude, whilst it was observed to be *increased* by $0^{s}\cdot063$. The anomaly, then, instead of being $0^{s}\cdot063$, as M. Kupffer states it (and which he attributes to the effect of 4500 French feet of elevation), would be $0^{s}\cdot167$, or nearly three times as great. M. Kupffer's *law* of an increase of ·000583 of the whole time of vibration, for a rise of 1000 French feet, will therefore, when corrected, amount to ·00155, and the factor for the diminution of intensity to twice as much, or ·0031, which is just *ten times* as great as my observations indicate, and is so considerable, that, were the conclusion just, it could not fail to be detected by the most ordinary instruments at the most ordinary elevations.

41. But if the anomaly be admitted to exist in M. Kupffer's observations, whence does it arise? I have no difficulty in answering the question. I shall not dwell upon the incomplete data from which the corrections due to temperature, latitude, &c. are derived; nor upon the entire incompetency of a single observation, which unknown causes (for instance, an iron mine, or the occurrence of an aurora borealis) may affect. I take M. Kupffer's own statement in the geological section of his work, which pronounces the whole country surrounding Mont Elbrouz to afford one continued evidence of

ancient volcanic eruption*, to abound in hot ferruginous springs†, to be so intersected by trachytes‡, lavas§, and diorites‖, that there is distinct evidence of this tract being nothing else than a crater of elevation, raised by the upheaving force of the trachyte of Mont Elbrouz itself¶, which he states to be undistinguishable from the rock of Pichincha, the great South American volcano**. Any one who has the slightest acquaintance with the connection between magnetism and volcanic rocks will be at no loss to explain anomalies even greater than those which M. Kupffer has observed.

42. It was from a persuasion of the entire inconclusiveness of M. Kupffer's results, as well as of all preceding ones, that I undertook the experiments already detailed, in the hope of compensating for the imperfections of the apparatus by the number and extent of the experiments. I own that until I came to calculate the results by the method of least squares, I had little confidence in having obtained any positive result. A careful examination of the station marked on the map, will show that they were almost invariably chosen so that an elevated station lay between two others at a lower level, by which the effect of change in latitude and longitude might be eliminated. When we criticize these groups of three series, we find for the most part an agreement greater than I had myself anticipated that the instrument could insure; yet the combination of all with two independent needles, and likewise in two series in different countries and different years, unite in giving a negative coefficient to the height, which I believe to be true and not accidental, though it could not safely be inferred from one or two insulated observations. I should be disposed to deduce its probable value thus, taking the circumstances of the observations into consideration:

	Weight.	Coefficient of Varied Intensity for 100 feet of Elevation.
Alps, Needle No. I. ...	4	·000033
Alps, Flat Needle	2	·000027
Pyrénées, both	1	·000053
	Probable mean	·000034

Hence to produce a variation of ·001, an elevation of 3000 feet is necessary. At the height to which Gay-Lussac ascended, the change of intensity would be nearly ·008 of the whole; but

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* Voyage, p. 39.

† P. 39, p. 44, p. 55.

‡ P. 44, p. 61, p. 65.

§ P. 60, p. 66.

‖ P. 63.

¶ P. 65.

** P. 35.

the variation in the time of an oscillation would be only half as much.

43. The smallness of the variation fully explains the difficulty of ascertaining its existence from a very limited number of observations. It is hoped that, notwithstanding the imperfection of the instruments, the extent of the induction will entitle the result to some confidence. By adding together the elevations of the distinct stations contained in Table VII. it will be found that the aggregate of the heights to which I have ascended amounts to above 160,000 feet, or *more than thirty vertical miles.*

§ 5. *On the Magnetic Dip.*

44. Although the horizontal magnetic force be only a sort of mathematical abstraction, and bears no direct relation to the earth's action until the effect of dip is considered, we do not therefore think it improper to be made a separate subject of inquiry. From the projected lines of equal horizontal intensity and of equal dip, the lines of equal total intensity are deducible. The two elements may therefore be made the subject of distinct inquiry; and though these elements are probably in a condition of continual change, yet, considering the present errors of observation, any moderate lapse of time between the formation of these curves will not be productive of serious anomalies. By deducing the total intensity curves from the two partial sets of curves, we also increase the probability of accuracy, since intensity is likely to be so much oftener observed than dip; that the lines of equal horizontal intensity will be better determined than if those points alone were used where the dip was also observed; and thus the whole acquires additional consistency.

45. The general relations of dip and horizontal intensity have been pointed out in the excellent charts of Hansteen. Though it is very probable that mountain chains may cause inflections in the general course of the curves, and local attractions produce occasional anomalies, yet the general variation of one or other quantity is always graduated; and though an insulated observation may be spoiled by an abrupt change in either element, the conclusion from a series of experiments cannot be so affected.

46. I state this chiefly to meet two objections to conclusions from experiments of the kind I have detailed, which have at different times been urged. The first is, that the influence of height (for example) upon the horizontal intensity may not be due to a change in the total intensity, but only of the dip.

To this we would reply, that no reason can be assigned why the dip should more naturally vary than the intensity; and that it is contrary to all probability that the variation in the latter should be wholly due to the indirect influence of the former. We admit that the change may be due to *both* causes conjointly; but further, if we adopt Humboldt's estimate (I quote from a reference which I have been unable to verify), which assigns a *diminution* of 2′·5 of dip for 1000 feet of extent, we should have an apparent *increase* of horizontal intensity, if the total intensity remained constant. The *second* objection to which I alluded, I believe no one accustomed to treat such problems will apply to my observations after due examination, namely, that though three stations be in one straight line and equidistant, the elevated station being in the centre, we can draw no conclusion as to the variation of the intensity by comparing the extreme observations with the middle one, because the dip may have altered in the interval. We may indeed have, by a strange accident, a solution of continuity which might produce this effect in a single instance, but its capability of affecting a whole series of observations cannot for a moment be sustained.

47. Observations of dip I have not, however, neglected. My instrument was a very small one (three inches diameter), constructed by Mr. Robinson for the late Captain Kater, and incapable of indicating such small variations as are required to fix with great accuracy the lines of equal dip. Nor can I hope that the small number of observations which I have accumulated can throw any light upon the influence of height on the dip. Still these observations may fix the dip at several stations with considerable accuracy, and the collation of them shows that tolerable precision may be attained even with an instrument of very small dimensions. Had the observations been as much multiplied as those of intensity the isoclinal lines would, even by this instrument, have been determined with very considerable exactness. The following table contains observations made with only one needle (marked No. II.), the other (from having too thick an axis) having been found to give much more anomalous results.

TABLE X.

Observations of Magnetic Dip with a Three Inch Circle.

Date.	Station.	Marked End.			Remarks.
		N. Pole.	S. Pole.	Mean.	
1832. April 23.	Edinburgh Greenhill House	71·0′	71°·46′	71°·23′	Mean 71° 33′·2. But the two last observations are the best. These observations are the only ones made in a house.
April 23.	Edinburgh	71·26	71·42	71·34	
May 23.	Edinburgh	71·31	71·46	71·38	
May 28.	Edinburgh	71·29	71·45	71·37	
July 11.	Brussels Observatory, (same place as intensity)	68·55	69·1	68·58	68° 49′ according to M. Quetelet, May 1832.
July 17.	Spa (same place as intensity)	68·20	68·24	68·22	
July 27.	Heidelberg. *a.* On the bank of the Neckar, two miles above the town ..	66·23	66·45	66·34	Mean 66° 37′·1. I have changed the leadings at the first station from 67° to 66°, considering the former as an undoubted error. This is confirmed by observations made at the same three stations by Needle No. I., which gave 66° 48′, 66° 18′, 66° 57′—Mean 66° 41′.
Aug. 7.	*b.* Prof. Leonhard's garden	66·20	66·44	66·32	
Aug. 7.	*c.* Terrace of the Castle	66·33	66·59	66·46	
Aug. 4.	Laach; SE. side of Lake	67·58	68·22	68·10	Good observation.
Aug. 5.	Andernach; foot of the hill west of the town	67·33	67·45	67·39	Good. The difference between this and the last probably due to volcanic influence.
Aug. 18.	Cologny, near Geneva, Prof. Necker's garden	64·45	65·10	64·57	

Aug. 20.	Geneva Observatory....	64·56	65·14	65·5	65° 48′·5 in 1825 (Arago). The dip diminished at Paris about 27′ between 1825 and 1832.
Aug. 22.	Mount Breven, summit	64·38	65·11	64·54	
Aug. 23.	Chamouni (same place as intensity)	64·45	65·16	65·0	
Aug. 25.	Jardin	64·45	65·7	64·58	
Aug. 29.	Aoste (same place as intensity)	64·36	64·58	64·47	
Aug. 31.	St. Bernard near the Lake	64·43	65·7	64·55	
Sept. 3.	Martigny	64·35	64.43	64·39	Difficult observation. Local influence being suspected, the operation was repeated at Bex.
Sept. 3.	Bex; in an orchard SW. of the town	64·47	65·14	65·0	Very good observation.
Sept. 10.	Interlaken; side of the Aar	65·13	65·30	65·22	
Sept. 22.	....	65·10	65·39	65·25	
Sept. 29.	Hospital; St. Gothard near the old castle ..	65·22	65·31	65·26	Indifferent observation.
Sept. 30.	St. Gothard Hospice....	64·50	65·30	65·10	
Oct. 2.	Locarno (Lago Maggiore) below the convent	64·48	65·13	65·0	
Oct. 11.	Pfeffers; near the Bath-house	64·55	65·23	65·9	
1835.					
June 13.	Paris Observatory; M. Arago's cabinet......	67·8	67·26	67·17	67° 24′ by M. Arago. July 1835.

Remark. —The dip at Edinburgh is undoubtedly affected by being made within a house. Some observations roughly made at the time in the open air confirm this; and more recently, I have found the dip by the same instrument to be 71° 44′·5 (2nd February 1837) and 71° 50′·5 (best, 4th February)—although the dip has been diminishing every year.

48. A careful review of these observations, compared to those of the usual dipping needles, gives, I think, a favourable impression of the powers of a small instrument. The observations were put in the form of equations of condition for the alpine series, exactly as in the case of intensity; x representing the variation of dip in minutes for 1′ of latitude N. increasing; y the variation for 1′ of longitude E. increasing; z the variation for 100 feet of height. Geneva is taken for the standard of comparison as before; $\delta\Delta'$ representing the correction of the dip at that place.

Table XI.

Equations of Condition for Dip.

Geneva......	$0'x + 0'y + 0z + \delta\Delta' = 0'$
Cologny	$0x + 2y + 3z + \delta\Delta' = -8$
Breven	$-16x + 41y + 71z + \delta\Delta' = -11$
Chamouni ..	$-17x + 43y + 21z + \delta\Delta' = -5$
Jardin	$-17x + 50y + 77z + \delta\Delta' = -7$
Aoste........	$-26x^{*} + 71y + 6z + \delta\Delta' = -18$
St. Bernard ..	$-20x + 61y + 68z + \delta\Delta' = -10$
Martigny	$-6x + 56y + 3z + \delta\Delta' = -26^{\dagger}$
Bex	$3x + 52y + z + \delta\Delta' = -5$
Interlaken ..	$30x + 103y + 6z + \delta\Delta' = 17$
Interlaken ..	$30x + 103y + 6z + \delta\Delta' = 20$
Hospital	$24x + 135y + 36z + \delta\Delta' = 21$
St. Gothard ..	$22x + 145y + 58z + \delta\Delta' = 5$
Locarno	$-2x + 159y - 6z + \delta\Delta' = -5$
Pfeffers	$47x + 200y + 17z + \delta\Delta' = 4$

49. The method of least squares gives us from these equations the following values of the unknown quantities:—

$$x = 0'{\cdot}543 \quad y = -0'{\cdot}028 \quad z = 0'{\cdot}080 \quad \delta\Delta' = -3'{\cdot}4.$$

As already stated, I consider these numbers (particularly z, which gives an *increase* of dip of 1′ for 1250 feet of ascent) as considerably uncertain.

50. If the variation of y for 1′ of longitude, be increased in the ratio of the length of 1′ of latitude to 1′ of longitude (as in Art. 32), it will become $= -0'{\cdot}039$, and the direction of the isoclinal line to the east of north will be

$$\text{Arc whose tang.} = \frac{543}{39} = 85^{\circ}\ 53'.$$

Hence the lines of equal dip would appear to approach nearer to the parallels of latitude than the lines of equal horizontal intensity (Art. 32). The corrected dip at Geneva would be 65° 1′·6, and the dip would increase 10′ for an increase of 18′·4 of latitude.

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* The coefficient ought to have been 28.

† This observation is certainly erroneous, and should have been discarded.

51. The lines of equal dip and equal horizontal intensity being known, the direction of the lines of equal total intensity may be deduced geometrically. I am, however, too well aware of the great uncertainty which a small error in the elements produces, to attempt to assign a result which might prove very erroneous indeed.

Postscript.—Since this paper was written, and the results made public, a suggestion has been made in a quarter entitled to attention, as to how far the apparent diminution with height may be due to the hour of the day at which observations at great heights have usually been made. I have already stated, that I have attempted no correction for the hour of the day, owing to the want of accurate data, but I thought it worth while to inquire how far there was any general ground for such an explanation of the observed difference. I accordingly divided my observations into 18 series above 4000 feet, and 22 below that height. I found that the mean hour at which the former were made was $11^{h}\ 12^{m}$, the latter at $12^{h}\ 42^{m}$. According to the best observations, the intensity would be somewhat less at the former period than the latter, and would so far give a false indication of diminished intensity with height. But the variation for $1^{h}\ 40^{m}$ would undoubtedly be trifling, compared even with the small variation which the preceding paper assigns for 5110 feet, which corresponds to the mean difference of height for the two series, the mean height for the first being 7160 feet, and for the second 2050.

[Prof. Forbes's paper, as printed for the Transactions of the Royal Society of Edinburgh, is accompanied by a "Map of the Central Alps," shewing the lines of equal horizontal magnetic intensity, and equal dip, in 1832. As the magnetic lines in this map have been projected entirely from the observations recorded and discussed in the paper, we have deemed it unnecessary to copy it.—EDIT.]

XLVI. *On a permanent Soap-bubble, illustrating the Colours of thin Plates. By* JOSEPH READE, *M.D.*[*]

NO subject in natural philosophy has more engaged the attention of the learned than the discovery of a permanent soap-bubble. Mr. Boyle, Dr. Hooke, and Sir Isaac Newton were among the first, Dr. Herschel and Sir David Brewster among the last experimenters. After such characters it may appear presumptuous to enter the lists unassisted by

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[*] Communicated by the Author.

novelty of experiment; I therefore rest my claims principally on that ground, and hope in this paper that the reader may find that interesting subject simplified. The first account of the colours produced by thin plates is to be found in Mr. Boyle's works: "To show the chemist that colours may be made to appear or vanish, where there is no accession or change either of the sulphureous, the saline, or the mercurial principles of bodies, he says that all chemical essential oils, as also good spirits of wine, by shaking till they rise in bubbles, appear of various colours, which immediately vanish when the bubbles burst, so that a colourless liquor may be immediately made to exhibit a variety of colours and lose them in a moment without any change in its essential principles: he then mentions the colours that appear in soap-bubbles, and also in turpentine. He sometimes got glass blown so *thin* as to exhibit similar colours." Here we may remark, that although Mr. Boyle did not advance any theory from these experiments, yet it is evident that he connected the production of colours with the thinness of the substance, as appears from his endeavours to blow glass sufficiently thin. This suggestion in all probability afterwards gave the idea to Dr. Hooke, and finally to Sir Isaac Newton, who has the merit of clothing Hooke's suggestion in a mathematical dress, beautiful and interesting in the extreme.

Dr. Hooke was the next to investigate this subject; at a meeting of the Royal Society, 7th March 1672, he promised to exhibit at their next meeting something which had neither reflection nor refraction, and yet was diaphanous; he then produced a bubble of soap and water. It was no wonder that so curious an experiment should excite the interest of one of the most learned, liberal and scientific societies in Europe; they requested him to bring an account of it in writing at their next meeting. "By means of a glass pipe he blew several small bubbles out of a mixture of soap and water, when it was observable that at first they appeared white and clear, but that after some time the film growing thinner, there appeared upon it all the colours of the rainbow, first a pale yellow, then orange, red, purple, blue, green, with the same series of colours repeated." Sir Isaac Newton's experiments as exhibited in his Optics are so well known, that I shall not enumerate them in this paper, merely remarking that his bubble was so evanescent that it burst before he had time to make an accurate examination. Melville of Edinburgh thought to make a permanent soap film by means of freezing. This was impossible. It occurred to me that by taking off the atmospheric pressure 112 pounds to every square inch, I might accomplish my purpose; I therefore made the following experiment.

Exp.—Having put two ounces of distilled water into an eight-ounce phial, and having added about the size of a large pea of Castile soap, I placed the bottle in a saucepan of boiling water on the fire; the bottle was speedily filled with a dense volume of vapour, which expelled all the air. I now corked it, and after cooling, and thus condensing the vapour, had perhaps as perfect a vacuum as could be formed, even by the best air-pump*. I now held the bottle laterally between my hands, and by means of a circular and brisk motion formed a circular film, on which by resting the bottle on an inclined plane, were formed after a short time all the parallel bands or series of colours in the following order: 1. a white or silvery segment at top; 2. a snuff-coloured brown inclining at bottom to a deep red; 3. blue; 4. yellow; 5. red; 6. blue; 7. green; 8. red; 9. green; 10. red; 11. green. (See Figures, p. 378.)

After some time a black segment was seen to form at the top of the white and continually to increase in size. After a few minutes the parallel bands increased in breadth, and running into one another only three or four distinct bands were seen. Nothing can exceed the beauty of these colours, equal to those of the rainbow, or the plumage of the tropics: whilst writing this description I have these bands in a bottle before me, feasting my eyes on their beauty. In a few minutes more this black segment or aqueous film occupies, perhaps, half the circular film, and the lower half becomes white tinged with orange.

If we now incline the bottle towards the experimenter's breast, the saponaceous atoms producing these colours are seen to float in the region of the black or aqueous: when placed again on the inclined plane they fall to the bottom of the films. In some time more the entire film becomes black, and all the colours disappear.

Having now placed the bottle in a basin of boiling water the evaporation was increased, and the black film soon became clothed with saponaceous atoms, which being variously condensed produced all the colours of the clouds when the sun is setting on a summer's evening. On again placing the bottle on the inclined plane the parallel bands were again formed by the attraction of cohesion, and the colours afterwards gave place to the black film. I held the bottle laterally be-

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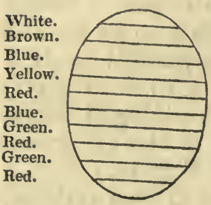
* This vacuum, we apprehend, may be vitiated by the entrance of atmospheric air through the cork, indicating the necessity of covering it with cement.—Edit.

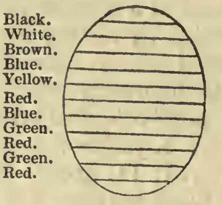
tween my hands, and by means of a circular motion washed it, and thus clothed it with saponaceous atoms, which went through the same process on placing the bottle on the inclined plane. By means of washing the film every morning, I preserved it for more than three weeks. This simple experiment opens a wide field of investigation to the natural philosopher, and enables him at his leisure to examine the interesting phænomena of these colours.

London, Sept. 4, 1837.

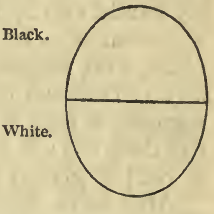
1.



2.



3.



[Since the date of the above paper we have received a communication from Dr. Reade, stating that on the 14th of September he exhibited the mode of preparing a permanent soap-bubble before the Section of Physics at the meeting of the British Association at Liverpool, Sir D. Brewster in the chair.—EDIT.]

XLVII. *On a large and very sensible Thermoscopic Galvanometer.* By JOHN LOCKE, *M.D., Professor of Chemistry in the Medical College of Ohio.*

To Richard Taylor, Esq.

DEAR SIR,

THE announcement of a new galvanometer will, perhaps, scarcely attract attention. But as I have been kindly encouraged by several eminent British philosophers to communicate some notice of my modification of the thermo-multiplier, I venture to send you the following sketch. Although a great labour has already been performed in electricity and magnetism, yet the adepts are aware that much remains to be executed, and that among the numerous principles already clearly established, it is probable that those proportions and arrangements which will produce the *maximum* effect have been in few instances fully ascertained. The chief novelty of the instrument which I am about to describe, consists in its proportions and the resultant effects. The object which I proposed in its invention was to construct a thermoscope so large that its indications might be conspicuously seen, on the lecture table, by a numerous assembly, and at the same time so delicate as to show extremely small changes of temperature. How far I have succeeded will in some measure appear by a very popular, though not the most interesting experiment which may be performed with it. By means of the warmth of the finger applied to a single pair of bismuth and copper disks, there is transmitted a sufficient quantity of electricity to keep an eleven-inch needle, weighing an ounce and a half, in a continued revolution, the connexions and reversals being properly made at every half turn.

The greater part of this effect is due to the *massiveness* of the coil, which is made of a copper fillet about fifty feet long, one fourth of an inch wide, and one eighth of an inch thick, weighing between four and five pounds. This coil is not made in a pile at the diameter of the circle in which the needle is to revolve, but is spread out, the several turns lying side by side, and covering almost the whole of that circle above and below. The best idea may be formed of the coil by the manner in which it is actually modelled by the workman. It is wound closely and in parallel turns on a circular piece of board eleven and a half inches in diameter and half an inch in thickness, covering the whole of it except two small opposite "segments" of about 90 degrees each. The board being extracted leaves a cavity of its own shape to be occupied by the needle.

The copper fillet is not covered by silk or otherwise coated for insulation, but the several turns of it are separated at their ends by veneers of wood just so far as to prevent contact throughout. In the spreading out and compression of the coil it is similar to Melloni's elegant apparatus; though in my isolated situation in the interior of America I was not acquainted with the structure adopted in his prior invention. In the *massiveness* of the coil my instrument is perhaps peculiar, and by this means it affords a free passage to currents of the most "feeble intensity," enabling them to deflect a very heavy needle. The coil is supported on a wooden ring furnished with brass feet and levelling screws, and surrounded by a brass hoop with a flat glass top or cover, in the centre of which is inserted a brass tube for the suspension of the needle by a cocoon filament. The needle is the double astatic one of Nobili, each part being about eleven inches long, one fourth wide, and one fortieth in thickness. The lower part plays within the coil and the upper one above it, and the thin white dial placed upon it, thus performing the office of a conspicuous index underneath the glass*.

I have not yet made any very extensive experiments with this instrument, being only just now prepared to do so. It is very sensible to a *single* pair of thermo-electric metals, to the action of which it seems peculiarly adapted; but the efficiency of such metals is increased by a repetition of the pairs, as in the thermo-pile of M. Melloni, especially if they be massive in proportion to the coil itself. With a battery of five pairs of bismuth and antimony, the needle was sensibly moved by the radiation from a person at the distance of 12 feet, without a reflector, the air being at the temperature of 72°.

In a recent interview with M. Melloni, to whose politeness I am much indebted, he expressed his opinion that with a thermo-pile massive in proportion to the coil, my galvanometer might be made to exhibit his thermo-experiments advantageously to a large class. Some idea may be formed of its fitness for this purpose from the result of a single trial on "transmission." The heat from a small lamp with a reflector, at the distance of five feet, passed through a plate of alum, and falling on a battery or pile of five pairs of bismuth and antimony deflected the needle only a fraction of one degree, but on substituting a similar plate of common salt, the same heat produced, by impulse, an immediate deflection of 33 degrees.

Although the instrument is finely adapted by its size for the purpose for which it was intended, class illustration, yet from the weight of the needle and the difficulty of bringing it to rest after it once acquires motion, it is not so suitable for experiments of research as the Mellonian galvanometer. When a massive thermo-pile, such as has lately been made by Messrs. Watkins and Hill of Charing-cross, is connected with the coil and excited by a heat of about 200°, the needle being withdrawn a distinct spark is obtained on interrupting the circuit; in producing this effect it is less efficient however than the ribboncoil of Prof. Henry. The tube for suspension, placed over the centre of the instrument, is so constructed as to admit of being turned round by means of an index, which extends from it horizontally over the glass cover, and thus any degree of torsion may be given to the suspending filament or wire. A wire of any desired thickness may be easily substituted for the cocoon filament, when the instrument becomes

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* This instrument has been made by Messrs. Watkins and Hill, Opticians and Philosophical Instrument Makers, No. 5, Charing Cross.

adapted to measuring the deflecting forces of the galvanic battery. By using a thick wire it was ascertained that the calorimotor of Professor Hare having 40 plates, each 18 inches square, acted on the needle with a force equal to 92 grains, applied at the distance of 6 inches from the centre. In attempting to force the needle by torsion into a line parallel to the coil, where the deflecting current acts with the greatest strength, I accidentally carried it too far and reversed its *position*, when instantly it became reversed in *polarity*, that which had been the north pole becoming the south. This showed how unfit is the magnetic needle to measure such a quantity of electricity as was then flowing through the massive conductor. The instrument is well adapted to show to a class the experiments upon radiant heat with Pictet's conjugate reflectors, in which the differential or air thermometer affords, to spectators at a distance, but an unsatisfactory indication. For this purpose the electrical element necessary is merely a disk of bismuth as large as a shilling, soldered to a corresponding one of copper, blackened, and erected in the focus of the reflector, while conductors pass from each disk to the poles of the galvanometer. With this arrangement the heat of a non-luminous ball at the distance of 12 feet will impel the needle near 180°, and if the connexions and reversals are properly made will keep it in a continued revolution.

I have thus given you a brief sketch of an instrument which seems to supply a desideratum on the lecture-table, when the common thermometer is too small to afford to a class that direct and full satisfaction which, in a subject so important as that of heat, is very desirable to every professor. I have not so far attempted to use it extensively as an instrument of research, yet it shows evidently the importance of massiveness in conductors for feeble currents, such as those produced by thermo-combinations; nor am I certain that I have arrived at a maximum in this particular, for so far as I have proceeded in using thicker conductors for the coil the deflecting effects have been increased. I am, &c.

London, Aug. 30, 1837. JOHN LOCKE.

XLVIII. *A Report of the Progress of Vegetable Physiology during the Year* 1836. *By* J. MEYEN, *Professor of Botany in the University of Berlin.*[*]

IT is delightful to see that during the past year not only have the works published in the province of botany been in-

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[*] From Wiegmann's *Archiv für Naturgeschichte*, 1837, Part 3. Translated by Mr. Wm. Francis.

creased in number, but the results of the labours recorded in them continue to become from year to year greater and more important. Systematic botany has received in the year which is past numerous and valuable contributions, for a whole series of most important works have appeared, not only on Phanerogamia, but also on Cryptogamia; vegetable physiology, also, has been enriched by a great amount of new data, and more correct views have been diffused in numerous publications upon many subjects, respecting which less accurate notions had heretofore been entertained. Nay, the number of works which appeared last year on subjects of physiological botany is so great that it is impossible in the small space allowed us to go fully into their contents, and it is more especially difficult to accomplish this with respect to the rich contents of some of the manuals which have appeared.

Several subjects of vegetable physiology which have been very fully treated of in former reports must also be now again noticed with greater minuteness: this perhaps might seem superfluous, but the object aimed at by the writer in these elaborate reports is to produce an unity in the views and an accordance in the observations and doctrines relative to the structure and functions of vegetables; so that this science may in the end become worthy to take its place by the side of animal physiology.

Great is the loss which the circle of botanists has sustained during the year which has terminated: Schrank, Persoon, Jussieu, and Schrader are no longer among them. Their labours are known, and will long impart lustre to the history of our science.

Since the first publication of this work (Wiegmann's *Archiv*) several yearly reports have appeared in Germany and in France, the contents of which more or less resemble ours. From the geographical position of Sweden, Wickström's year's report on the progress of botany must always reach us very late, and can never be so complete as if it had been prepared in the interior of the Continent; in order to remedy this defect Beilschmied has undertaken to translate those reports into German, and at the same time to enrich them with the most recent literature in which they are deficient. Thus last year we received Wickström's report for 1834*. A second volume of the *Archives des Découvertes et Inventions nouvelles faites dans les Sciences, les Arts et les Manufactures tant en France que dans les Pays étrangers pendant l'année* 1835† has ap-

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* Translated with additions and index by C. T. Beilschmied. Breslau, 1836.

† Paris, 1836, 8vo. (An extremely poor piece of manufacture.—Wiegm.)

peared; and Valentin of Berne* has given a critical view of the results of the principal physiological labours belonging to the year 1835; there is no want of competition, therefore, in works of this class, and it is only to be hoped that no one will go so far as to make up reports out of year's reports. The author of the present report designs to continue his labour in future years, and, circumstances permitting, will extend it to systematical botany also.

Convenient as it is to science that most of the learned societies now publish more or less copious notices of the labours of their members, it must still be remarked that short reports on the contents of several memoirs, read at the meetings of these societies, often appear in print, until at last, too frequently after a very long interval, the entire memoirs are published. Since however these short reports often contain but very incomplete statements, we have in many cases thought it necessary to wait the appearance of the original paper itself.

On the Symmetry, Arrangement, and Characteristics of the Nature of Plants.

The new edition of Link's *Elementa Philosophiæ Botanicæ*, which appeared last year, begins with the remark that natural bodies, when in a perfect state, possess a more or less symmetrical figure. In p. 30 he adduces proof that the entire plant or its parts are symmetrical, yet differing a little from exact symmetry. The plant is a compound organic body; each individual part is almost quite symmetrical, its combination however often not so, for many exterior circumstances hinder or accelerate the origin and growth of branches. A variation from the symmetrical form often takes place when superincumbent parts appear to retard complete development.

A small tract by Mohl† treats more copiously on the symmetry of vegetables. It is there shown that most organs of plants tend more or less evidently to symmetrical formation. The concentric, symmetrical, and the diaphorical mode of formation is in the first place distinguished, and then specially exemplified in a great number of plants. The structure of the lower order of plants is conceived extremely well, and the author observes that a correct notion of those plants in which stem and leaf are separated, can only be attained by a

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* Vide Valentin's *Repert. für Anat. und Physiol.*, &c. Berlin, 1837, vol. i. p. 1—77.

† On the Symmetry of Plants (an Inaugural Dissertation). Tübingen, 1836, 8vo.

comparison of them with the formation of the *thallus* in the lower plants. "We have seen," says he, p. 38, "according to what has been said, in the organs of vegetation a constant progression from the symmetrical to the concentric formation; not however a fixed progression, but one interrupted by fluctuations. The pure symmetrical formation in the lower order of plants raised itself to the concentric on the stems of the *Jungermanniæ* and *Lycopodiæ*; this however did not yet appear openly, but still showed a considerable affinity to the symmetrical formation. In the Phanerogamia a weak tendency to the symmetrical formation is yet often evident in the stem; on the contrary, however, in general the most determined concentric organization shows itself; while in the leaves the symmetrical formation takes place in a manner not less remarkable than in the thallus of the Cryptogamia. In the branches we often observed a return to the symmetrical formation, while in the more highly developed leaf-forms many phænomena pointed to the tendency of the petiole of the leaf to raise itself to a concentric formation. We observed in the leaved stems and in the pinnate leaves the symmetry showing itself in a double form: first, in a narrow circle, in the corresponding formation of both side-halves of the individual leaflet; and secondly, in a wider circle, in the symmetrical formation of the two opposite leaf-lines sacrificing the symmetry of each individual leaflet."

In flowers it rarely occurs that they are not separated by a perpendicular section into two equal halves; and the general rule is that all terminal flowers are regular; that on the contrary irregular flowers are allotted to such inflorescences as are not terminal. According to this the symmetrical formation of the flowers stands in connection with their position.

Fries* has endeavoured to solve the question, which vegetables might be regarded as the most perfect, in a very ingenious manner. He first shows how the views of earlier botanists on this subject were untenable; he refutes most admirably De Candolle's view, according to which the *Ranunculaceæ* were the most perfect plants; for perfection in vegetables does not consist in the more perfect development of any individual organ, but in the harmonious development of all the organs collectively into a typical whole. Fries enumerates the following among the criteria of the perfection of a vegetable:

1. The greater number of degrees of metamorphosis a plant has to pass through, before the fruit is developed, the

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* Essay towards a new answer to the question: Which vegetables are the most perfect? Transl. from the Swedish by Hornschuch. *Flora*, 1836, p. 1—16.

more perfect it is. 2. The more complete the metamorphosis the more perfect is the vegetable. 3. The most perfect vegetables have also the most regular and symmetrical formation of the flower. 4. Those are the most perfect which not only possess all organs, but have these also combined in the most perfect harmony. 5. The greater stress nature has laid on the development of the seed, the more perfect is the plant. 6. Those vegetables are the most perfect which express in the purest manner by structure, form, numeral relations, and vital manifestations the type of their section. And, 7. since the typical form is the result of the most general relations, it follows from thence that the most perfect groups must be the most numerous and the greatest.

According to these fundamental positions, which in general are to be admitted, Fries pronounces the *Compositæ* to be the most fully developed plants.

We have received some interesting observations on the generation of some of the lower *Algæ*, which continue to bring nearer to a decision the great question, whether the *Bacillariæ*, and those beings nearly related to them, are to be classed under vegetables or animals. Mohl* first made known some observations on *Conferva glomerata*, according to which an increase of the members of this plant takes place by separation. The branches of this vegetable originate constantly on the end of the upper side of a member of the Conferva, and in such a manner that no communication takes place between the cells from which the branches originate and the inferior member of the branch, but both members are entirely separated by a septum. However, observations on branches just beginning to shoot show that at first this septum is wanting, and that there is present only a hook-like protuberance of the member, which grows in a cylindrical sac of about the common length of the members. A contraction then takes place, and appears in the form of a circular septum, pierced in the centre, which is gradually developed, till at last the connection between the cell of the branch and that of the stem is completely interrupted; and thus two cells entirely separated from each other have originated from the cell of the branch. The newly originated cell increases in size and again divides itself, &c. In consequence of this observation, Mohl supposes that a similar mode of increase takes place also in the genera *Scytonema* and *Oscillatoria*; and in this we almost entirely agree with him. The same occurs also in the *Rivu-*

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* On the Increase of the Cells of Plants by Separation. Tübingen, 1835. (Published towards the end of 1836.)

lariæ, although in this case the separation does not take place at the point of the sporangia, which however, as will be immediately shown, occurs also in Confervæ. From various phænomena it seemed probable to Mohl that in the several species also of the genus *Spirogyra*, Link, (*Zygnema*, Ag.), the single cells possess the property of dividing themselves in their centre by a septum. This supposition I can fully confirm; for experiments made on *Spirogyra* when budding (which since Vaucher's* observations it appears no one had repeated,) have shown it in a most evident manner. At the commencement in this case it is always the last member remaining in the burst capsule, which increases considerably in length, and divides itself by a new septum into two cells, upon which the inferior cell lengthens, &c. Soon after some of these new cells lengthen and divide again.

These data, viz. the increase of the cells in microscopical plants by separation, are of great importance, and have hitherto been but rarely mentioned, and never with so much certainty. Carus† formerly observed how the ends in *Achyla prolifera* Nees, separated by an apparent cellular septum from the other parts of the sac; Carus in the same memoir has also mentioned several observations on the gradual contraction down to the complete separation. The origin of the fruit of the *Vaucheria* by constriction was also known; hitherto however no general conclusions on the growth of these plants by simple separation of the cells had been mentioned till Dumortier discovered a similar increase at the terminal cells of *Conferva aurea* ‡. As soon as the terminal cell of this Conferva had become considerably longer than the preceding members, a septum formed in its interior; this observation is quite similar to that of Mohl on *Conferva glomerata*. A similar mode of increase, viz. that by forming septa, was also observed by Morren § in the *Closteriæ*, which this accurate naturalist is completely justified, by his very convincing reasons, in classing amongst vegetables, on which subject however we shall by and by have more to say.

It would now be of the greatest importance, if the datum, first confirmed by Dumortier, that cells can increase by a formation of septa, could also be demonstrated in the more perfect plants; this has been accomplished with tolerable certainty

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* *Hist. des Conferves.* Pl. 4, 5 et 6.

† *Nova Act. Acad. C. Nat. Cur.*, t. xi. p. 503.

‡ *Recherches sur la Structure comparée et le Développement des Animaux et des Végétaux.* Bruxelles, 1832, p. 10.

§ *Sur les Clostéries, Ann. des Scienc. Nat.*, vol. i. p. 274.

by Mirbel's beautiful observations on the formation of the pollen in the *Cucurbitaceæ.* I myself have often thought I saw in the formation of the *glandulæ capitatæ* of several plants the origin of septa in the cells; the peculiarly formed hairs on the internal surface of the sacs in the genus *Utricularia* also appear to originate only by contraction, excrescence, and separation. Nay, even such a formation of more or less complete septa is evident in the diachymous cells of the leaves of *Pinus silvestris*; they may be seen in diagonal sections as runners from the internal side of the cellular septum; but a complete division of these cells is in truth not to be perceived.

An increase of vegetable cells by separation has been already proved in a direct manner; and therefore those distinctive characters which Ehrenberg* established between animals and vegetables are by no means so conclusive, but might on the contrary be used to prove what Ehrenberg endeavours to refute. Ehrenberg considers an increase by division as a character which belongs to numerous beings plainly evincing themselves to be animals, and which is totally wanting in plants, since the latter always grow by increasing in length and the formation of buds; and that on that account the *Bacillariæ* are not plants, but must be classed with animals. As it has now been proved that the division of cells takes place exactly in the same manner in well-defined plants as in the *Bacillariæ,* and as it can be shown that the separation in the increase of Infusoria and other lower animals is very different from this separation of vegetable cells, such a separation by septa might even furnish a character to distinguish vegetables from animals.

Mohl† observes that the character mentioned by Ehrenberg, viz. the power of separation in animals, the want of it in plants, suffers the fate of various other distinctive characters which have been started separately: in general they are right, but in special and doubtful cases they are not to be depended on. Mohl here refers to his observation on the separation of the conferval sacs, of which we have already spoken. Mohl also confesses that after many years' observation he still remains quite in doubt as to the place which the *Bacillariæ* should occupy, that however their increasing by separation does not justify us in classing them as animals.

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* Memoir read before the Academy of Berlin 25th of April. *L'Institut,* p. 195. Also Scientific Memoirs, vol. i. p. 405.

† On a Character for distinguishing Animals and Plants proposed by Ehrenberg. *Flora,* 1836, vol. ii. p. 491—494.

I may also mention that Link*, Unger†, and Morren‡ have of late remarked that these doubtful creatures which are known under the name of *Bacillariæ* ought to be arranged with vegetables; according to this there would remain no other botanist, with the exception of Corda, that had paid any considerable attention to vegetable anatomy who did not consider the *Bacillariæ* to be plants.

From this we may judge of the contradictions on this subject, which are found in the reports edited by Wiegmann and myself on the progress of zoology and physiological botany for the year 1835§; as these creatures are at times mentioned as plants, at times as animals, and indeed under quite different denominations||.

Morren, in the above-mentioned highly important memoir on the *Closteriæ*, has very fully treated the question whether they should be arranged with animals or vegetables; he succeeded, by employing very high magnifying powers, in showing that those red and very moveable little points discovered by Ehrenberg at the ends of these beings were nothing else than minute vesicles which afterwards change into new individuals. It was these moveable and as it were oscillating points which were considered as organs of motion, and appeared to justify the placing of the *Closteriæ* among animals, which however at present, after Morren's discovery, falls to the ground. Besides the occurrence of these self-moving propagula in the interior of the *Closteriæ*, Morren has observed a formation of fruit by conjugation quite similar to the mode of formation of the fruit in the *Conjugatæ*¶; and besides this, there also takes place an increase of the *Closteriæ* by separation.

The siliceous envelope which surrounds the *Closteriæ* as well as all other *Bacillariæ* is regarded by Morren as a formation analogous to the so-called *cuticula* of plants, a fact which is capable of confirmation only in certain relations; for in the perfect plants this fine plate of silica lies in the substance of the

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* *Philos. Bot. Edit. alt.*, p. 400.

† Vide his treatise on *Algæ* in Endlicher's *Genera Plantarum.*

‡ *Sur les Clostéries, l. c.*

§ Wiegmann's *Archiv.*

|| I am sorry to say that these contradictions must also occur in this year's report; as I do not think Ehrenberg's view as to the animal nature of the *Bacillariæ* weakened by the reasons here stated.—Wiegmann.

¶ The same observation has been already made by Corda and noticed by me in the report for the preceding year (1836, vol. ii. p. 186.). It had already been mentioned by Ehrenberg also in 1834. (*Beitr. z. Kenntn. gr. Organis. in der Richtg. d. kl. Raumes,* p. 95.)—Wiegmann.

cuticula, and is only separated from this by the destruction of the organic parts. Besides this siliceous envelope Morren supposes the existence of two other distinct membranes, which form the cuticle of the *Closteriæ* and inclose the green substance; he however remarks that they only become evident upon the metamorphosis of the plant. I consider the inner pellicle to be the analogue of the inner envelope which is formed in the members of Confervæ when their spores are ripened, or they begin to increase in any other manner, as for instance by excrescence and separation. Morren thinks it possible to explain the motion of the *Closteriæ* by the action of opposite electricities. The author also gives a very complete description, accompanied with drawings, of the very manifold forms which the *Closteriæ* exhibit at different periods; and by this he shows how at least six of the new species of the genus *Closterium*, described by Ehrenberg, belong to one and the same species.

De Brébisson* also made observations on the enigmatical *Diatomeæ* in order to decide the question whether they should be classed with animals or vegetables. On burning a great number of *Fragilaria pectinalis* an animal smell was noticed. Such a smell would however be a very indefinite character, for various other Algæ produce a similar odour on their being burnt to a coal. After the burning of the *Fragilaria pectinalis*, and various other beings of the same kind, Brébisson found siliceous envelopes surrounding them in a very perfect state, and precisely similar to those exhibited by the fossil *Diatomeæ* discovered by C. Fischer in the peat-bog near Franzensbad, and which led to those beautiful observations that Ehrenberg made known on this subject in the course of last year†. The results of those latter observations belong properly to geognosy; but we must add this one remark, that under the fossil Infusoria hitherto discovered, only those beings are to be understood which botanists, as has been previously shown, receive as plants. The occurrence in a fossil state of these minute microscopical plants is caused by the hard siliceous envelope, which resists all destroying influences. Kützing's discovery that the envelope of the *Bacillariæ* consists of *silica*, which was mentioned in our first year's report, has by this circumstance been rendered more important. If we observe the same minute plants in the living state, it often happens that amongst them some dead ones occur, which ex-

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* *Observations sur les Diatomées.—L'Institut de* 1836, p. 378.—*Ann. des Scienc. Nat.* 1836, ii. p. 248.

† Vide On Fossil Infusoria, Wiegmann's *Archiv*, 1836, p. 333. A translation of Ehrenberg's two papers on this subject is given entire, and with engravings, in the Scientific Memoirs, vol. i. p. 400.—W. F.

hibit that perfectly transparent and colourless siliceous envelope; it is therefore proved by this circumstance that a great mass of such siliceous envelopes might also be produced by the *decomposition of the plants, or in the moist way*; and also that the mountain masses, which consist more or less of such siliceous envelopes, might not always be regarded as being produced by the action of heat at the bottom of the sea*. Brébisson tries to bring the *Diatomeæ* into two divisions, viz. the proper *Diatomeæ*, which exhibit a siliceous envelope, and the *Desmidiæ*, which are without a siliceous coating and entirely reduceable to carbon. In the more perfect plants, the epidermis of which is penetrated by a siliceous envelope, it would at least be improper to make such divisions; in this case, however, they may be of some use.

In a recent memoir Mohl† has again declared himself against the animal nature of the *Bacillariæ*. "I admit," says he, "that the doubt which was raised respecting their vegetable nature is not yet removed; their animal nature however has been as little proved, and we find evident transitions from them to vegetables, &c.

[To be continued.]

XLIX. *Notice relative to the Theory of the Winds. By* John Dalton, *D.C.L., F.R.S.*

To Richard Taylor, Esq.

Dear Friend, Manchester, Sept. 5th, 1837.

I PUBLISHED a theory of the *Trade Winds*, &c., as Mr. Dove has published‡,—it was forty-four years ago, as may be seen in my Meteorology, 1793 and 1834. It was first published by G. Hadley, Esq., in 1735, as I afterwards learnt. It is astonishing to find how the true theory should have stood out so long.

John Dalton.

L. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

A LETTER addressed to C. Lyell, Esq. was then read from Dr. Mc Cleland, who has been associated with Mr. Griffith in the scientific deputation sent under Dr. Wallich into Upper Assam to investigate the natural history of the country where the tea-plant is found growing

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* Ehrenberg's opinion is that these masses owe their origin to the action of volcanic heat on the bottom of the sea. Vide Scientific Memoirs, vol. i. p. 400.—W. F.

† On the Symmetry of Plants. Tübingen, 1836, in December. (Published as an Inaugural Dissertation.)

‡ See our last and present Numbers.

wild. The deputation left Calcutta in the end of August, and reached the Kossia Mountains early in October. In crossing the Delta of the Ganges and Bramaputra (Burrampooter), a high tract of land, two or three hundred feet above the level of the plain, was observed (but not examined) near Dacca. Between this and the Kossia Mountains, distant sixty miles, the old channel of the latter river extends, so that the above high land is situated between the channels of the two great rivers. The country is generally composed of silt, but at Mymensing (where the navigation is most obstructed), the Bramaputra crosses a bed of yellow clay containing ferruginous and calcareous (the *kunkar* of India) concretions. The author thinks this bed of clay extends from the above high land to the foot of the Kossia Mountains, or the extreme point of the portion called the Garrow Hills, where the late Mr. Scott found the fossil remains of animals. The partially inundated plain at the foot of these mountains is interspersed throughout with small rounded hills, which reposing on the above-mentioned yellow clay are themselves composed of various layers of sands, clays, gravels, and boulders, even at the greatest heights. These appear to be the remains of a talus of great extent, which had covered the foot of these mountains and been swept away by the Soorma and other great rivers falling from them.

The foot of the mountains is composed of a rock in which Nummulites are imbedded in a compact calcareous basis. Sandstones seemed to rest upon this, but the relative position of the two rocks could not be clearly ascertained from the dense vegetation and the short time available for the examination.

On ascending to Cherraponji, a sanatory station established at an ascertained height of above 5000 feet, the limestone was soon lost sight of; but great masses of sandstones presented themselves, which had been rent and the fissures filled up with boulders and gravel. If the mountain acclivity be supposed to be divided into three stages, the first forms a steep slope covered with deep soil and vegetation; the second is precipitous and more or less naked; and the third is composed of sloping ridges terminating in mountain and table lands from 5000, to near 6000 feet high.

At the top of the first stage, or at about fifteen hundred feet above the sea level, the author discovered a well-defined marine beach, containing shells and other marine exuviæ about two feet deep, and reposing upon sandstone and covered with soil. The shells consist of Pectens, Cardia, Ostreæ, Terebratulæ, and Melaniæ, mineralized by a fine yellow sandy matter, and united together by a brown indurated clay. Between the curved slaty layers of animal matter and shells, as well as in nests, loose sand is found. The whole presented the form of shingles caused on a beach subject to tides. In the course of an hour several hundreds were, and with more time many thousands might have been obtained. These shells were compared with a collection of about one hundred and fifty species from the Bay of Bengal and the estuaries of the great rivers, but not one was found to correspond; nor with those found by the late Dr. Gerrard in the secondary strata on the north of the Himalaya; but a small collection of about one hundred species from the Paris basin were at once recognised by the author as familiar objects, from his acquaintance with those from Kossia and Cherraponji : these consisted of about an equal number of species, and on being submitted to systematic comparison about twenty species were found to be identical in the two collections.

On crossing this deposit, the rock composing the superficial strata of sandstone forming the precipices of the second part of the ascent, was found to contain the impressions of shells and other organic remains, which the author believes to be ramose Alcyonia ; these continued to appear all the way up to Cherraponji, where they occurred in the greatest numbers.

On this sandstone reposes a deposit of compact limestone, from which twenty-seven species of shells were extracted, as species of Trochites, Cerithia, and of Modiola of Lamarck, with *Piliolus plicatus* of Sowerby. On this formation reposes a bed of coal to the depth of above twenty or thirty feet, in which was found an exogenous plant. On descending towards the plain from a point five or six miles west of Cherraponji, and one or two miles below the village of Mumloo, an elongation of the same fossil beds and sea beach was met with at about the same elevation. The shells and characteristic remains, with six species of Medusæ, were found imbedded in a red sandstone, or rather indurated sand, immediately beneath the soil.

On crossing the mountains towards the centre of the group, the sandstone, on which the limestone and coal rest at Cherraponji, was found for fifteen or eighteen miles, forming in horizontal strata lofty undulating lands, with little variation except in ravines and the banks of streams. Beyond the above distance the strata displayed marks of confusion, and in the first deep river valley, a mass of greenstone was found with the adjoining sandstone tilted up in highly inclined tabular masses, formed of quartzose pebbles imbedded in felspar. This form continued to the second deep valley, where the greenstone was again met with, and the adjoining sandstone compact, glossy, and columnar.

Leaving this (the Boga Pany) and ascending the opposite acclivity, all traces of the earthy sandstone are lost, and the centre of the mountains from Mufling to the highest ridges is composed of syenite. Granular quartz, slaty and in vertical strata, is found in contact with this, and interposed between it and the common sandstones ; displaying progressive changes from one to the other. The northern side of the mountains from Muflong into Assam is composed of granular foliated felspar, penetrated by quartzose veins, and more irregularly with beds of mica. Extensive beds of syenite and central nuclei of granite are found as far as the Valley of Lower Assam, which here is about thirty miles broad, and bounded on the north by the Bustan Mountains. Groups of rocky hills extend from the Kossia and Garrow Mountains across the valley, threatening as it were to interrupt the waters of the Bramaputra and convert the interior of the valley into a lake. These called the Meeker Hills, are composed of insulated rocky protrusions of metamorphosed gneiss, in some instances syenitic, in others as a sandstone whose base is earthy felspar containing crystals of the same mineral, and in others as hornblende slate; all, and especially the second, with veins and beds of quartz projecting upwards, the veins often radiating irregularly from the beds. Along the veins a laminated structure is produced in the rock; this structure follows the course of the vein, and disappears as you recede from one vein, re-appearing again as you approach another.

These, as well as many other objects of geological interest, could only be cursorily observed from the necessity of accompanying the expedition. Hot and salt springs,—from the latter the natives derive a muriate of soda,—as well as fossil bones, as first discovered by the late Mr. Scott, present themselves along the base of the mountains.

The author also collected about one hundred and sixty species of the animals, chiefly birds, from the forests of Assam, which have been forwarded to the India House, as well as one hundred and twenty species of the fishes of the Bramaputra in Upper Assam; many of which are identical with Hamilton's Gangetic species, but several are new; regarding the habits and peculiarities of which the author states having collected considerable details.

On the remains of a fossil Monkey from the tertiary strata of the Sewalik Hills in the north of Hindoostan; by Captain P. T. Cautley, F.G.S., Bengal Artillery, and H. Falconer, M.D., Bengal Medical Service.

The authors commence their paper with some general observations on the differences in habit in different animals, which prevent the remains of some being so frequently preserved as those of others in a fossil state, and they adduce as instances birds and quadrumanous animals. So speedily are the remains of these carried away by the hyæna, the chacal and wolf, the scavengers of torrid regions, that in India, the traces of casualties are so seldom seen,—even where monkeys occupy in large societies the groves of mango trees round villages,—that the simple Hindoo believes they bury their dead by night.

The authors since engaged in the examination of the Sewalik fossils, were early led to anticipate the finding of some quadrumanous animals, and several months ago obtained an astragalus of the right hind leg, which they minutely describe, and compare with that of the recent *Semnopithecus Entellus*; which, though certainly belonging to a distinct species, it closely resembles both in size and general form, as is exemplified in the specimen sent with the fossil astragalus. This was completely mineralized, having a sp. gr. of about 2·8 and appearing to be impregnated with hydrate of iron. Although only a solitary bone of the foot, the relations of structure are so fixed that the identity of the fossil is as certain as if the entire skeleton had been found. But the authors deferred making the announcement, in the hopes of soon finding specimens of the cranium and teeth; these have been discovered by Messrs. Baker and Durand of the Bengal Engineers, who have obtained a considerable portion of the face, and the whole series of molars of one side of a quadrumanous animal, belonging to a much larger species than theirs.

In the debris or different beds of the formation which yielded the quadrumanous fossil astragalus, the authors have also discovered *Ano-*

plotherium Sivalense, F. and C., with *Crocodilus biporcatus* and *C.* (*Leptorhyncus*) *gangeticus* or the Magar, and Garial (Gavial), which now inhabit the Ganges, showing that the quadrumana existed, with a member of the oldest Pachydermatous genus of Europe and reptiles of the present day.

The camel (*Camelus Sivalensis*, F. and C.), antelope, and anoplotherium have been exhumed from the same bed. There have been found also the elephant, mastodon, hippopotamus (*H. Sivalensis* and *H. dissimilis*, F. and C.), rhinoceros, hog, and horse, together with the *Sivatherium giganteum*, a huge ruminant, exceeding in size the largest rhinoceros, armed with four enormous horns, divided and foliated like the dicranocerine antelopes. There is also a musk deer scarcely larger than a hare; specimens of the cat (*Felis cristata*, F. and C.) and of the dog tribe; the hyæna, bear (*Ursus Sivalensis*, F. and C.), and ratel, with other carnivora. Of the feathered tribe, there are Grallæ much larger than the gigantic crane of Bengal (*Ciconia Argala*). Of reptiles, besides the magar and gavial, there are other crocodiles of enormous size (*C. Leptorhyncus crassidens*, F. and C.); and of Testudinata ordinary-sized species of emys and trionyx, with humeri and femora as well as corresponding fragments of the bucklers of a species as large as the corresponding bones of the Indian rhinoceros. The authors refer to the "Journal" and "Researches of the Asiatic Society of Bengal" ["the Asiatic Researches"] for descriptions of their new species.

ZOOLOGICAL SOCIETY.

[Continued from p. 201.]

December 27th, 1836.—The remainder of M. F. Cuvier's Paper on the *Jerboas* and *Gerbillas* was read.

M. Cuvier commences this memoir with observing that his attention has been particularly directed to the *Rodentia*, with a view of arriving at a natural classification of the numerous species composing that order, among which considerable confusion had hitherto prevailed, particularly in the genera *Dipus* and *Gerbillus*, the relations of which to other allied groups have been but very imperfectly understood by previous writers.

The species included in the genus *Dipus* have been formed by M. Lichtenstein into three divisions, which are distinguished by the absence and number of rudimentary toes upon the hind feet. In the first section are placed those with three toes, all perfectly formed; in the second, those with four, one of which is rudimentary; and in the third, those with five, two of these being rudimentary. M. Cuvier states that he is unacquainted with the second division of M. Lichtenstein, but in the examination of the species belonging to the first, in addition to the absence of rudimentary toes, he finds they are also distinguished from those of the third by the form of the teeth, and the osteological characters of the head. These points of difference he considers of sufficient importance to justify his making a distinct genus for the *Jerboas* with five toes, adopting the name *Allactaga*, given by Pallas to a species, as the common generic appellation.

"We know," observes M. Cuvier, "that the three principal toes of the *Allactagas*, as well as the three only toes of the *Jerboas*, are articulated to a single metatarsal bone, and that the two rudimentary toes of the first genus have each their metatarsal bone; whence it results that the penultimate segment of the foot is composed of three bones in the *Allactagas*, and of one only in the *Jerboas*. The incisors of the *Allactagas* are simple, whilst those in the upper-jaw of the *Jerboas* are divided longitudinally by a furrow. The molars of the latter genus are complicated in form, and but little resemble those of the former. They are four in number in the upper-jaw, and three in the lower, but the first in the upper is a small rudimentary tooth, which probably disappears in aged individuals."

The structure of the grinding teeth is then described in detail, and illustrated by drawings which accompanied the paper.

"The general structure of the head of the *Allactagas* and *Jerboas* is evidently the same, and is characterized by the large size of the *cranium*, the shortness of the muzzle, and above all by the magnitude of the suborbital *foramina*. The cranium of the *Jerboa* is distinguished by its great breadth posteriorly, resulting from the enormous development of the tympanic bone, which extends beyond the occipital posteriorly and laterally as far as the zygomatic arch, which is by no means the case in the *Allactagas*, where all the osseous parts of the ear are of moderate dimensions. Another differential character between the two genera, is presented by the maxillary arch, which circumscribes externally the suborbital foramina, and which, in the *Allactagas*, may be said to be linear, and presenting a very limited surface for the attachment of muscles. Lastly, we may note a difference in the relative development of the jaws, the lower being comparatively much shorter in the *Allactagas* than in the *Jerboas*."

The author then proceeds to describe a new species of *Allactaga*, a native of Barbary, for which he proposes the name of *A. arundinis*. Its length from the origin of the tail to the end of the muzzle, 5 inches; length of the tail, 5 inches and 2 or 3 lines; of the ears, 1 inch; length of the tarsi from the heel to the extremity of the toes, 22 lines. All the upper parts of the body are of a beautiful greyish yellow, with yellowish sides and tail of the same colour, terminated by a tuft of a blackish brown at its origin, and white at the extremity. The sides of the cheek, the ventral surface of the body, and the internal limbs are white; large brown moustaches adorn the sides of the muzzle. The incisors are white and entire, the ears almost naked.

M. Cuvier next proceeds to consider the characters and affinities of the genera *Gerbillus* and *Meriones*, and enters into a critical examination of all the species referred to that group. To these he adds another species, the habits of which he details, and describes at length under the name of *G. Burtoni*. The species which he thus includes are, 1st, *G. Egyptiacus*, syn. *Dipus Gerbillus*, *Meriones quadrimaculatus*, Ehrenberg; 2nd, *Gerbillus pyramidum*, syn. *Dipus pyramidum* Geoff., *Meriones robustus* Rüpp.; 3rd, *G. pygargus*, syn. *Meriones Gerbillus*, Rüpp.; 4th, *G. Nidicus*, syn. *Dipus Nidicus*, Hardwicke; 5th, *G. Africanus*, syn. *Meriones Schlegelii* Smutz., *G. Afra* Gray;

6th, *G. brevi-caudatus*; 7th, *G. Otaria*; 8th, *G. Burtoni*. The author enters into detailed descriptions of each of these species from original specimens. M. Cuvier lastly considers the affinities of the *Gerbillas* and *Allactagas* to the *Gerboas*, and concludes that the *Gerbillas* have a much nearer affinity to the *Muridæ*.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: MEETING AT LIVERPOOL.

Sept. 13.—Professor Graham made a communication on the subject of the Inorganic Salts, and in particular, on the function which water discharges as an element of their composition. He had been requested, at the meeting of the Association held in Dublin, to report at some future meeting on the present state of our knowledge of saline bodies; and his communication to the Section was understood to have been made with the view of discharging the duty which had been thus imposed on him.

The Professor developed at some length his own views respecting the constitution of salts. The hydrated acids are unquestionably salts, having water as base, and they correspond in a remarkable manner with the salts having for base magnesia, oxide of zinc, oxide of copper, or any other oxide isomorphous with magnesia. Hence water as a base belongs to the magnesian class of oxides. Super or acid salts have two bases, of which water is one. They are double salts, and correspond with the double salts of the same acids containing magnesia, oxide of copper, &c. Thus the salt called the binoxalate of potash is really a double oxalate of water and potash, and corresponds in constitution with the double oxalate of copper and potash, as will be seen on comparing their formulæ below:

$$\dot{K}\ \dot{C}\ddot{C}\ \dot{H}\ \dot{C}\ddot{C}\ \dot{H}^2, \text{ and}$$

$$\dot{K}\ \dot{C}\ddot{C}\ \dot{Cu}\ \dot{C}\ddot{C}\ \dot{H}^2.$$

Mr. Graham's researches tend to prove that all salts are neutral in composition, with the exception of certain specified classes. One of these classes is the phosphates, of which there are three kinds, containing respectively one, two, and three atoms of base to one atom of acid, and for which the names of monobasic, bibasic, and tribasic phosphates are proposed, in substitution for the old names of metaphosphates, pyrophosphates, and common phosphates. In some of the tribasic phosphates, the three atoms of base are all different, as in microcosmic salt, in which we have an atom of soda, of ammonia, and of water, all united together, to one atom of phosphoric acid. Only one class of arseniates exists, but it is the tribasic class; it is likewise probable that the phosphites are all tribasic; but all the other classes of salts at present known, such as the sulphates, nitrates, &c., are monobasic. In the case of those combinations which are at present called subsalts, Mr. Graham finds that there is really only one atom of base to one atom of acid. In the ordinary neutral salts, such as nitrate of copper, we have several atoms of water in combination with the salt, and known as water of crystallization, but which Mr. Graham distinguishes as constitutional water. Now it appears that metallic oxides may be substituted for this constitutional water; and it is in this way that subsalts come to be produced. Thus the salt called subnitrate of copper is really a nitrate of water with three atoms oxide of copper attached to that combination in the place of water of crystallization. The nitrate of water, or nitric acid of specific gravity 1·42, the nitrate of copper, and the subnitrate of copper, are all of similar constitution, and are represented by analogous formulæ, viz,

$$\dot{H}\,\overset{\stackrel{\cdot\cdot}{\cdot\cdot\cdot}}{N}\,\dot{H}^3,$$

$$\dot{Cu}\,\overset{\stackrel{\cdot\cdot}{\cdot\cdot\cdot}}{N}\,\dot{H}^3,$$

$$\dot{H}\,\overset{\stackrel{\cdot\cdot}{\cdot\cdot\cdot}}{N}\,\dot{Cu}^3.$$

These formulæ illustrate the constitutional neutrality of salts. In each of them we have one atom only of oxide in the relation of base to the acid, (which is expressed by placing its symbol to the left of the symbol of the acid;) while in each salt we have three atoms of oxide in another and totally different relation to the acid. Certain salts appear to be capable of combining with anhydrous acids, and then a new order of saline combinations is produced. The sulphate of potash and chloride of potassium absorb anhydrous sulphuric acid without decomposition, as has been proved by H. Rose. The red chromate of potash is analogous to Rose's salts, but more permanent. It is not a true *bi*chromate of potash, but a binary combination of chromic acid with the neutral chromate of potash without any water. The red chromate of potash is therefore not an exception to the law, that all salts are neutral in composition. It is well known that all the ordinary salts of ammonia contain an atom of water, which forms part of the base, and that they may be represented as containing the oxide of a hypothetical radical ammonium. Mr. Graham considers water as the true base of these salts, and that ammonia is not a base itself, but belongs to a class of bodies which may be called *basic adjuncts*, which admit of being attached to the oxide of hydrogen or to the oxides of metals, the only true bases. Thus the sulphate of ammonia is truly the sulphate of water, with ammonia as a basic adjunct. The sulphovinates contain sulphate of water, with olefiant gas as a basic adjunct. The nature of the constitution of the combinations of dry salts with ammonia can now be explained. In these combinations the metallic oxide is in the place of the basic water of the ordinary ammoniacal salts. Thus, chloride of hydrogen (muriatic acid) combines with one atom of ammonia; chloride of copper does the same thing, and the ammonia cannot be expelled or separated by heat in either case. These combinations are represented by analogous formulæ:

$$NH^3\,H\,Cl, \text{ and}$$

$$NH^3\,Cu\,Cl.$$

Anhydrous sulphate of copper absorbs, at an elevated temperature, half an atomic proportion of ammonia, and retains it by a most powerful affinity. It is curious that in similar circumstances oil of vitriol or the sulphate of water absorbs the same proportion of ammonia, the bisulphate of ammonia being produced. The two products are of the same constitution. The basic adjunct, ammonia, is attached to oxide of copper in the one case, and to oxide of hydrogen in the other. Both are double salts, and may be expressed by the following formulæ :—

$$\dot{\mathrm{Cu}}\,\dddot{\mathrm{S}} + \mathrm{NH}^3\,\dot{\mathrm{Cu}}\,\dddot{\mathrm{S}}, \text{ and}$$

$$\dot{\mathrm{H}}\,\dddot{\mathrm{S}} + \mathrm{NH}^3\,\dot{\mathrm{H}}\,\dddot{\mathrm{S}}.$$

It thus appears that the ordinary ammoniacal salts which contain water, are a particular class of an extensive order of salts; as, for the water there may be substituted oxide of copper, oxide of zinc, nickel, cobalt, and many others. Many of these combinations are capable of assuming an additional dose of ammonia, which, however, is feebly retained, and is in a relation to the salt like that of water of crystallization.

In the discussion which took place on Prof. Graham's communication, Mr. Richard Phillips gave it as his opinion, that the difference between constitutional water and basic water arises from the well-known law, that when one principle combines with more proportions than one of another, the first proportion is held with a stronger affinity than the others.—Mr. G. Bird could not conceive how water could be considered as a base, and inquired what view Professor Graham would take of the function of the atom of water in oil of vitriol and in caustic potash.—Dr. Faraday expressed his satisfaction that such a variety of opinions should be advanced, and even be maintainable by powerful arguments, upon so interesting a subject; for, from this collision of opinion, it was most likely that the truth would ultimately be struck out. He also cautioned chemists against considering electrical relations as affording, in every instance, conclusive proofs of what is a base and what is an acid.—Prof. Johnston concurred in the observation of Dr. Faraday, and professed that he had a very strong leaning to the theoretical views in reference to the constitution of salts which had been just propounded by Professor Graham.—Dr. Kane made some remarks on the same subject, objecting to some of Professor Graham's statements. To these Professor Graham briefly replied, and the discussion closed.—*Athenæum*, Sept. 23, p. 695.

LI. *Intelligence and Miscellaneous Articles.*

ON THE THERMO-ELECTRIC SPARK, AS OBTAINED FROM A SINGLE PAIR OF METALLIC ELEMENTS. BY MR. FRANCIS WATKINS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HOPE you will allow me to make known in your forthcoming publication a fact in thermo-electricity which I have observed since my last communication to you, and which I believe has not been noticed in print in this or any other country.

With a pair of metallic elements, consisting of one bismuth and one antimony, weighing each five grains and measuring 0·5 of an inch long and 0·12 diameter, when their extremities were unequally heated, I have obtained, with a Henry's flat ribbon coil, a very perceptible and brilliant spark.

I have had the pleasure of showing the experiment to MM. De la Rive, Plateau, and Netschayef, and I need not add that these distinguished philosophers were much delighted on seeing the *thermo*-electrical light developed by a single pair of metallic elements.

Now I have pen in hand permit me to state that with thermo-piles I actuate most of the apparatus usually employed for illustrating electro-magnetic phænomena, so that the public teacher may now show by the same apparatus the several rotations, &c. with thermo-electricity, as he does with voltaic electricity.

I remain, Gentlemen, &c. Francis Watkins.

5, Charing Cross.

ON THE ARTIFICIAL PREPARATION OF FORMIC ACID.

Prof. J. B. Emmet of the University of Virginia makes the following observations respecting, and proposes the annexed methods of preparing formic acid :

One part of tartaric acid (or sugar), one and a half of peroxide of manganese, one and a half of sulphuric acid, diluted with about two and a half parts of water, when well mixed and subjected to distillation, will furnish the formic acid according to Dœbereiner's process. In order to diminish the inconvenience arising from the frothing of this mixture, and which is exceedingly great, it is directed to add only half the amount of dilute acid at first, and to make use of a retort having five or six times the bulk of the matter to be put into it.

The explanation given by Dœbereiner and other chemists, assigns to the *peroxide of manganese* an agency absolutely necessary for success, viz. that, while it parts with a portion of its own oxygen and combines, as the protoxide, with sulphuric acid, it is enabled by the oxygen thus detached, to convert the tartaric acid (or sugar) into the *formic* and *carbonic acids*.

The whole of this explanation is, however, incorrect, as will appear from the following results of my inquiry.

1. The presence of peroxide of manganese, (or any other peroxide,) is not only *unnecessary*, but positively injurious and productive of much inconvenience. It is positively injurious in consequence of the power which all peroxides have of decomposing formic acid, and productive of inconvenience in consequence of the vast amount of carbonic acid which it produces with the formic acid and the carbon deposited during the operation. The latter is, in fact, the cause of the excessive frothing.

2. Sulphuric acid is not essential. The formic acid was prepared by phosphoric acid as well as by the chloride of tin; and no doubt all other substances, capable of converting alcohol into æther, may be shown to possess the same power. In no case does sulphuric acid,

phosphoric acid, or chloride of tin undergo any decomposition, unless incidentally.

3. The formic acid may be procured from almost every kind of vegetable matter that is capable of being promptly blackened by contact with strong sulphuric acid. It *rarely* appears previous to the carbonization*, and *only* when the sulphuric acid possesses a powerful affinity for *water*.

It would appear, from these particulars, that the process for obtaining formic acid artificially is analogous to those operations for converting cotton, ligneous matter, &c., into gum—gum or starch into sugar, and alcohol into æther or olefiant gas, as far as regards the integrity of the sulphuric acid; but, in another respect, namely, the abundant deposition of carbon, previous to the escape of the formic acid, the action more resembles what occurs when alcohol changes at once into carbon and olefiant gas. The resemblance is still closer, if, as I suppose to be the case, the agency of the sulphuric acid consists in removing *water*, or its elements, from the organic substances, which yield the formic acid when under its influence. I have mentioned that the phosphoric acid may be substituted for the sulphuric. In the experiment to determine this, the absence of the latter acid was accurately proved by muriate of baryta; starch was employed, and the phosphoric acid had the consistency of syrup. But although important for the investigation, as a fact, the substitution really cannot, in practice, be made with advantage, because the phosphoric acid has not the same degree of affinity for water, and before the essential action occurs, (well indicated by the separation of carbon,) the organic matter becomes decomposed, more or less, from simple exposure to heat, which thus imparts to the formic acid an unpleasant empyreumatic taste. The same remark applies to the *chloride of tin*.

There is little doubt, therefore, that, under the influence of strong sulphuric acid, gum, sugar, starch, lignin, &c., bear the same general relation to *formic acid*, and the latter to *oxide of carbon*, that *alcohol* does to *hydric æther*, and the latter to *olefiant gas* or ætherine. Thus,

Sulphuric acid, by subtracting	*Water* from	*Alcohol*—	furnishes	*hydric æther.*
		Sugar, &c.	,,	*formic acid.*
	Water from	*Æther,*	,,	*olefiant gas.*
		Formic Acid,	,,	*oxide of carbon.*

By a comparison of combining proportions, it will be seen that this explanation enables us to dispose of all the elements except two of hydrogen.

Thus, by adopting $(C+O+H)$ as the formula for one atom of *sugar;* and, supposing *four atoms* to be the smallest amount involved

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* When the chloride or sulphate of tin is employed, perfect carbonization does not take place, yet the formic acid is generated readily. There is no doubt, however, that some variety [or combination] of carbon separates at the same time. Sugar, for example, gave a large deposit of a snuff brown colour, and resembling in its properties the *ulmin* of rotten wood.

in the process, we shall have ($4\,C+4\,O+4\,H$), from which subtract $\dot{H}$, or one atom of *water*, (removed by the sulphuric acid,) and we shall have $4\,C+3\,O+3\,H$, which is equivalent to one atom of *formic acid* ($2\,C+3\,O+H$) together with 2 *carbon*, precipitated, and 2 *hydrogen*, unaccounted for. Again, assuming ($6\,C+5\,O+5\,H$) as the formula for one atom of *starch*, and subtracting $2\,\dot{H}$, or two atoms of water, removed by the sulphuric acid, the remainder will be equivalent to one atom of formic acid, ($2\,C+3\,O+H$) together with 4 atoms of carbon deposited, and 2 atoms of hydrogen unaccounted for.

During the preparation of formic acid by Döbereiner's process, as well as by my own, in which no peroxide of manganese is employed, there is always formed, previous to the carbonization, a considerable quantity of *volatile oil*, which, at first, might be considered as arising from the excess of *hydrogen* and carbon in the process; but a special inquiry has convinced me that this is not the case, although the oil is so abundant that it may actually be observed floating in drops down the neck of the retort. When the sulphuric acid is so far diluted as not to carbonize the mixture at the heat of boiling water, little else than this spicy oil passes over by distillation; but as soon as the matter becomes black, its formation ceases, and if we begin at once with sulphuric acid about one half diluted, it does not appear at all; but, instead of it, strong formic acid, without any foreign odour, and quite colourless. This volatile oil would not be regarded as objectionable by many, since it imparts an aroma to the acid like that of cassia or cinnamon, and a taste somewhat similar to that produced by hydrocyanic acid.

The process which I recommend, as having been found the most convenient and perfect, for obtaining strong formic acid, is the following.

Mix together in a glass tubulated retort, equal measures of *water*, *oil of vitriol*, and clean, but unground *rye*, (or cracked maize), let them be heated to the boiling point, and, as soon as the mass has become thoroughly blackened, add another measure of water and distil off one measure of formic acid.

By the addition of a further quantity of water, and by fresh distillation, a weaker acid may be obtained, which will answer very well to be added in subsequent operations. Besides being too weak, the product of this second distillation will often contain some *sulphurous acid*, which seldom appears in the first, and never is essential to the process. It occurs in company with oxide or carbonic acid, and may be removed by agitating, for a short time, the *cold* formic acid with peroxide of lead, as recommended by Berzelius.

By employing the whole grain, when small enough, as of rye, wheat, oats, &c., and in the great proportion here recommended, the contents of the retort become too solid to froth up easily, so that the medium sized vessels may be employed. Indeed, still smaller ones may be substituted, by simply allowing water to enter through a dropping funnel at the tubulure, in proportion as it is removed by the distillation.—*Silliman's Journal*, vol. xxxii. p. 145.

EDWARDSITE, A NEW MINERAL.

Dr. Shepherd, Prof. of Chemistry, South Carolina, gives the annexed account of this substance:

Mineralogical description.— Primary form. Oblique rhombic prism. M on M=95° (common goniometer). Base oblique from an obtuse edge.

Secondary form. The primary, with the acute lateral edges replaced by single planes inclining to the adjacent lateral faces under 137° 30′ (common goniometer). In very minute crystals, the summits are occasionally surmounted by four-sided pyramids whose faces correspond to the lateral edges of the prism.

Cleavage parallel to the bases sometimes distinct, but more commonly uneven: in the direction of the longer diagonal very perfect. Surface generally not very smooth, but nearly of the same quality on the different faces.

Lustre vitreous to adamantine. Colour hyacinth-red. Streak white. Transparent to translucent.

Hardness=4·5. Sp. gr.=4·2. . . . 4·6

Chemical description.—Alone before the blowpipe, in very thin fragments, it loses its red colour, becoming pearl grey with a tinge of yellow, and fuses with great difficulty on the edges into a transparent glass. With borax, in little fragments, it turns white and gradually dissolves, forming a globule which is bright yellowish green while warm, but colourless when cold. When powdered, it is acted upon very slightly, by aqua regia. A small quantity placed on platinum foil and moistened with sulphuric acid, tinged the flame of the blowpipe green.

General observations.—The crystals are rarely above one third of an inch in length by one sixth in breadth. The replacement of the acute lateral edges is deep, imparting to the prism a flattened appearance, except in the case of very minute crystals surmounted by pyramids; these scarcely exhibit any alteration of the primary prism. The terminations of the larger crystals are always incomplete. In some of them, however, the cross cleavage is eminent, in which instances the lateral faces exhibit cross striæ parallel with this cleavage, analogous to certain varieties of Hornblende and Pyroxene. The nearest approximations to the value of the angle of inclination between the base and the prism was 100° for P on M. More perfect crystals, however, are needed than any I have yet seen for deducing the incidence of P to the obtuse lateral edge. The diagonal cleavage is almost as perfect as the corresponding cleavage in Sillimanite. So close is the resemblance between the smaller crystals above alluded to and Zircon, that on first inspection I mistook them for that species.

The Edwardsite occurs disseminated through Bucholzite in gneiss at the falls of the Yantic in Norwich, Connecticut. The Bucholzite is here considerably abundant, forming apparently a small bed, through which are dispersed also individuals of red feldspar, black mica, and more rarely small crystals of blue corundum. The variety of Bucholzite is intermediate in the size of its fibres between that of Chester, Conn. (the Sillimanite,) and that found at Chester, near Philadelphia, and denominated Fibrolite.

Having discovered the mineral above described, while occupied along with my colleague, Dr. Percival, in the geological examination of the state, I have thought proper to name it in honour of his Excellency Henry W. Edwards, the governor of the state; since the survey was first recommended by his Excellency, and is still in progress under his administration.

Edwardsite submitted to analysis yielded the annexed substances:

Protoxide of cerium............	56·53
Phosphoric acid	26·66
Zirconia.......................	7·77
Alumina	4·44
Silica.........................	3·33
Protoxide of iron, glucina, and magnesia traces and loss	1·27—100.

Dr. Shepherd remarks that the phosphoric acid and oxide of cerium are in such proportions as to constitute a *basic sesquiphosphate of the protoxide of cerium.—Ibid.* p. 162.

SILICEOUS AND CALCAREOUS PRODUCTS OBTAINED BY MEANS OF SLOW ACTIONS; REPORT BY MM. GAY-LUSSAC AND BECQUEREL, ON A NOTE OF M. CAGNIARD-LATOUR.

M. Cagniard-Latour states that by the means of several processes which he has devised, and which are dependent upon slow action, he has succeeded in forming various substances analogous to those which are found in nature. The following are some of the results which he has obtained.

" *First Experiment.*—Some lamp-black was treated with hot concentrated nitric acid; the liquor after having been poured off was exposed under a bell-glass for several months to the action of solar light; in proportion as the acid diminished, water or acid was added; by degrees siliceous concretions formed, some of which inclined to the pyramidal form. Analysis indicated two per cent. of carbon; these concretions submitted in a platina crucible to the action of caustic potash, heated by the flame of an alcohol-lamp, diminished in size; their hardness is sufficient to scratch rock crystal.

" *Second Experiment.*—Some of the bog iron (*fer limoneux*) of Berry was taken; after having reduced it to a very fine powder, it was treated with hydrochloric acid; the solution was diluted with water and was filtered; it was next put into a large retort, and a glass capsule containing a piece of white marble was then suspended in it. The marble was gradually attacked, carbonic acid gas was disengaged; oxide of iron was deposited, and crystals several millimetres in length having the form and principal properties of felspar with a calcareous base.

" *Third Experiment.*—Milk of lime (*lait de chaux*) was poured into a solution of perchloride of iron, to which had been added a brown infusion of roasted corn. The precipitate having been well washed in water, then mixed with this liquid, the mixture was heated in a kind of Papin's digester until the interior pressure amounted to eleven atmospheres; siliceous grains were precipitated produced from the milk of lime. The matter was then taken and redissolved anew in hydrochloric acid; the solution having been filtered, it was again filtered through chalk of Meudon, which had been passed through very fine cambric, by means of water, to separate the grains of quartz from it. Oxide of iron was deposited in the chalk. When the filtration was difficult, the liquor was acidulated. At the end of fifteen days the Meudon whitening was again strained through the cambric, and the part which had not passed was treated with hydrochloric acid; small opalascent siliceous concretions were obtained, of which several have the form of crowns and are split from the centre to the circumference; they are not fusible with the blowpipe and scratch glass; those which were coloured being moderately heated, acquired a smoky tint in consequence of the organic matter which they contain.

"*Fourth Experiment.*—125 grammes of powdered Meudon whitening were put into a glass tube about two inches in diameter, and four feet and a half in height; the lower part of the tube was then closed with a piece of linen rag intended to serve as a filter. Afterwards water was put into the tube, and the whitening was shaken so as to mix it well. After having completely filled up the tube with this water, some water very weakly acidulated with hydrochloric acid was prepared; and in proportion as the water first put into the tube filtered away through the whitening and the linen upon which it rested, acidulated water was poured into the tube. The filtered water deposited by degrees in a bottle in which it was received, crystalline grains of carbonate of lime; and at the same time the linen serving as a filter, became covered over a great part of its exterior surface with a crust which, examined with a magnifying glass, had the appearance of saccharoidal marble. The experiment lasted about three months. The quantity of whitening of Meudon which was dissolved during the time that the filtration continued was about 75 grammes, that is to say, a little more than the half of all the whitening which had at first been put into the tube."—*Comptes Rendus,* No. 25. June 1837.

NEW CARBURETS OF HYDROGEN, RETINNAPTHE, RETINGLE, RETINOLE, AND METANAPHTALENE.

MM. Pelletier and Walter have examined the products obtained during the conversion of resin into gas for gas lights; the results are stated to be:

1st. The instant the resin falls into the red-hot cylinder there are formed with the gas a certain number of extremely hydrogenated compounds which have been separated by chemical analysis.

2nd. Among these substances there occur three new carburets of hydrogen, to which the author has given the names of *rétinnapthe, rétingle,* and *rétinole*; these are all liquid: there are two solid carburets of hydrogen, *naphtalene,* already known, and *métanaphtalene,* a new compound.

3rd. Rétinnapthe is a very light and volatile fluid; its composition, determined by the density of its vapour, may be represented by $C^{28} H^{16}$. This product, M. Pelletier observes, is at least isomeric with one carburetted hydrogen, which is still hypothetical, but which appears to play a great part in the benzoic compounds, if indeed it be not itself this carburetted hydrogen; it gives rise to a series of new compounds.

4th. Rétingle is a new sesquicarburet of hydrogen, which may be represented by the formula C^{36}, H^{34}, H^{24} [?]; it is susceptible of conversion by the action of chlorine, bromine, and nitric acid, into compounds which exhibit a series of new combinations.

5th. Rétinole is a new bicarburet of hydrogen, the formula of which is $C^{64} H^{32}$; it differs from the bicarburetted hydrogen of Faraday $C^{24} H^{12}$, both in its constitution and its chemical properties.

6th. Métanaphtalene is a new substance, which differs from naphtalene in its properties, but isomeric with its composition. It is remarkable for its splendour and beauty, its chemical indifference, in which property it resembles paraffine, from which it differs totally in its properties and composition.

The substances whose properties and composition have now been briefly stated, result from the sudden application of a red heat to resin. M. Pelletier states, that in a second memoir he will examine the properties of the products obtained from resin at lower temperatures.—*L'Institut,* June 1837.

DOUBLE SALT OF CODEIA AND MORPHIA.

M. Kœne, of Brussels, has published a memoir entitled, *New Observations on a double Salt of Codeia and Morphia*; in this he has arrived at the following conclusions:

1st. Codeia and morphia form, with muriatic acid, a salt which is undecomposable by ammonia.

2nd. Ammonia does not enter into the composition of this double salt.

3rd. The quantity of morphia in the double salt is less than that of the codeia, and according to one experiment only, as 1 to 3.

4th. Muriate of codeia and morphia in solution with muriate of ammonia crystallizes first, and the latter remains in the mother water.

5th. When heated, ammonia entirely decomposes the double sulphate of the two vegetable alkalis; but the combination remains stable, if during the evaporation, the ammonia is not in too great excess.—*Journal de Chimie Medicale,* Juin 1837.

CARBURETS OF HYDROGEN.

M. Laurent distilled 8 to 10 pints of the oil of bituminous schistus, and received the product in separate portions. He attempted, but in vain, by repeatedly rectifying the same portion, to obtain an oil the boiling point of which should be nearly constant; he procured a dozen different oils, the boiling heat of which varied from 9 to 10 degrees from the beginning to the end. This circumstance indicates the presence of several different bodies in the oil of the schistus; the author examined several which differed in the point of ebullition.

Oil from 176° *Fahr. to* 185°.—This oil is the most volatile. It possesses all the properties of naphtha. On this account the author would have felt disposed to consider the product really as naphtha, and the bituminous schistus as the source of it; but its composition is sufficiently different to cause it to be regarded as a new bicarburetted hydrogen.

Oil from 239° *to* 257°.—This oil resembles the foregoing considerably. M. Laurent distilled it repeatedly with concentrated nitric acid; he obtained in the receiver a colourless oil, the boiling point of which varied only from 248° to 249°, and there remained in the retort an altered yellowish oil which was heavier than water.

Oil from 248° *to* 249°·8.—This oil, which results from the action of nitric upon the preceding, had the following properties: it was colourless and very fluid. Neither the nitric, hydrochloric, nor sulphuric acid acted at all upon it. Its specific gravity was 0·753 at 59° Fahr. It yielded by analysis

Carbon......	86·2	
Hydrogen ..	13·6	99·8

The constancy of its boiling point induces the supposition that this is a new bicarburetted hydrogen.

Oil of 336°.—M. Laurent examined whether at about 336° he could not obtain eupion, and he set aside some oil the boiling point of which varied from 332° to 338°. On comparing it with eupion, he found no difference between them; by analysis he obtained the following results:

	Eupion.	Oil at 336°.
Carbon........	85·3	85·6
Hydrogen......	15·1	14·4
	100·4	100·0

It is then evident that all circumstances agree to prove that eupion is a product of the distillation of bituminous schistus.

The author made a mixture of different oils, the boiling point of which was between 185° and 662° (302°?). He analysed it after having treated it with sulphuric acid and potash, and he obtained the annexed results:

Carbon..........	85·5	
Hydrogen........	13·5	100·

On comparing all these analyses and that of paraffine, it will be observed that the different bodies contained in the oil of schistus have the same composition as bicarburetted hydrogen within a few thousandths.

AMPELIC ACID.

This acid is obtained by boiling with nitric acid the oils whose boiling point is between 176° and 302°. On evaporating the acid, white flocks of ampelic acid separate on cooling. This substance is white, inodorous, very slightly soluble even in boiling water; alcohol and æther dissolve it readily. It fuses at a temperature above 498°, and it sublimes into a whole mass consisting of minute acicular crystals. It is dissolved by concentrated sulphuric acid; water decomposes the solution. With the alkalis it forms very soluble salts.

AMPELIN.

This is a remarkable substance, and it is distinguished from all other bodies by its similarity to oils in some of its properties, and by its solubility in water. In order to prepare it, oil of schistus, the boiling point of which is between 392° and 536° is to be agitated with concentrated sulphuric acid; this is to be poured off, and there is to be added 1-15th to 1-20th of its volume of a solution of potash; the whole is to remain quiet for twenty-four hours; at the end of this time there are formed in the bottle two strata, and the lower one is more bulky than the solution of potash employed; this is to be separated, diluted with water, and mixed with sulphuric acid; the ampelin separates and rises to the surface of the solution.

Ampelin resembles a fluid fixed oil; it is soluble in alcohol, and æther dissolves it in all proportions. When subjected to a temperature of 68° it does not solidify. It dissolves in pure water in all proportions: when submitted to distillation, it decomposes, and yields water, a very limpid colourless oil, and a coaly residue.—*L'Institut*, June 1837.

ACTION OF COLD AIR IN MAINTAINING HEAT.

I believe it is not generally known that nail-makers are in the habit of supporting the heat of the iron, when hammering it into form on the anvil, by blowing a current of cold air upon it.

An opportunity accidentally presenting itself sometime since near Birmingham, I asked a nail-maker to show me the operation, which he readily did, observing that to do it with the greater effect he would put an additional weight upon his bellows. He also mentioned that it was requisite to employ the iron at a very high temperature, or otherwise the cold air instead of maintaining and increasing the heat would quickly cool the iron. The efficacy of the current of air and the necessity of making the iron very hot when employing it, were rendered as perfectly evident as the use of bellows in increasing the combustion in a common fire.—R. P.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1837.

Chiswick.—August 1. Rain. 2. Cloudy. 3, 4. Fine. 5, 6, Very fine. 7—9. Fine. 10. Overcast. 11—13. Very fine. 14, 15. Very hot. 16. Cloudy. 17. Foggy. 18, 19. Hazy: sultry. 20. Fine: slight showers: cloudless at night. 21. Fine. 22. Overcast: sultry with showers: heavy rain at night. 23. Rain. 24. Hazy: fine. 25. Very fine. 26. Slight haze: rain with thunder. 27. Very clear: fine. 28. Fine. 29. Rain. 30. Heavy rain. 31. Clear: very fine.

Boston.—August 1. Cloudy. 2. Rain. 3. Fine: rain A.M. 4. Fine: rain with thunder and lightning A.M. 5—9. Fine. 10. Cloudy. 11. Cloudy: rain early A.M. 12. Cloudy. 13—16. Fine. 17. Cloudy: rain in torrents early A.M.: rain with thunder and lightning P.M. 18, 19. Cloudy. 20. Fine: rain early A.M. 21, 22. Cloudy. 23. Rain. 24. Cloudy. 25. Fine. 26. Cloudy: rain early A.M.: rain A.M. 27. Fine. 28. Cloudy. 29. Rain. 30. Cloudy: rain early A.M.: rain A.M. 31. Fine.

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

Days of Month. 1837. Aug.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Max.	Min.	Max.	Min.								
● T. 1.	29·752	29·769	29·693	29·21	59·7	70·0	56·9	67	55	63·5	SE.	E.	calm	·036	·43	...	58
W. 2.	29·694	29·699	29·671	29·12	64·5	64·8	57·4	71	56	60	S. var.	S.	E.	·422	·07	·17	60
Th. 3.	29·716	29·697	29·665	29·08	65·0	71·0	59·9	73	53	64	S. var.	SW.	W.	·033	·34	·33	61
F. 4.	29·896	30·047	29·863	29·25	64·3	69·5	57·2	73	42	63	SW.	SW.	W.	·080	...	·03	60
S. 5.	30·182	30·187	30·158	29·60	61·3	69·3	52·2	71	45	58	SW.	NW.	calm	·375	...	·02	56
⊙ 6.	30·266	30·291	30·249	29·74	60·7	66·0	52·0	69	49	62·5	E.	NE.	E.	...	...	...	55
M. 7.	30·356	30·460	30·357	29·86	60·8	66·3	53·2	69	46	60·5	E.	E.	E.	...	...	...	57
T. 8.	30·396	30·378	30·282	29·87	61·7	65·6	51·5	69	50	61	NE.	NE.	calm	...	...	...	55
☽ W. 9.	30·142	30·260	30·013	29·65	62·9	66·4	54·0	75	55	63	NE. var.	NE.	E.	...	...	...	57
Th. 10.	29·968	29·952	29·879	29·40	62·0	70·2	57·3	77	59	64	E.	NE.	calm	...	·01	...	59
F. 11.	29·884	29·985	29·852	29·33	65·2	72·0	61·3	78	54	61	NNW.	SW.	calm	...	...	·03	61
S. 12.	30·016	30·081	29·987	29·36	65·3	72·0	60·3	79	51	62	SW.	SW.	calm	...	...	...	61
⊙ 13.	30·160	30·166	30·117	29·50	63·4	73·4	56·9	80	49	65	S.	SW.	NW.	...	...	...	60
M. 14.	30·286	30·259	30·242	29·59	68·3	74·6	59·6	85	50	71	E.	SE.	calm	...	...	...	61
T. 15.	30·278	30·261	30·211	29·65	65·0	77·0	56·5	85	55	67	E.	NE.	E.	...	...	...	60
○ W. 16.	30·140	30·118	30·031	29·56	65·2	75·0	58·5	85	57	67	E.	SE.	E.	...	...	...	61
Th. 17.	30·044	30·064	29·999	29·40	67·5	74·0	62·5	87	55	68	E.	S.	calm	·022	...	2·34	66
F. 18.	30·160	30·240	30·149	29·45	65·5	78·8	62·2	82	56	66	SW.	S.	calm	·008	...	·02	65
S. 19.	30·126	30·109	29·939	29·46	67·5	76·8	63·3	83	63	66·5	NNW.	S.	calm	...	...	...	66
⊙ 20.	29·938	30·026	29·905	29·17	70·7	77·5	63·5	82	57	72	SW. var.	S.	W.	...	·02	·01	68
M. 21.	30·100	30·079	30·047	29·37	67·3	75·0	61·0	72	53	63	SW.	SW.	W.	...	·01	...	65
T. 22.	30·172	30·155	30·084	29·46	67·5	73·5	59·0	72	63	65	SSW.	SW.	calm	...	·30	...	67
☾ W. 23.	30·054	30·058	30·034	29·43	65·8	73·6	65·2	73	53	61	W.	W.	calm	·444	·32	·20	65
Th. 24.	30·212	30·215	30·202	29·66	56·8	71·9	55·0	69	41	57	N.	NE.	E.	·638	...	·13	52
F. 25.	30·228	30·224	30·178	29·68	58·5	63·3	50·4	72	46	58	E.	E.	calm	...	·09	...	57
S. 26.	29·888	29·947	29·885	29·26	64·5	67·0	56·0	75	39	59	SSW.	S.	W.	·022	·51	·38	62
⊙ 27.	30·180	30·188	30·132	29·64	53·5	72·7	48·5	63	39	53	E.	NE.	N.	·763	...	·06	50
M. 28.	30·044	30·053	29·741	29·54	58·5	60·0	49·7	68	51	53	E.	S.	NW.	...	·17	...	54
T. 29.	29·504	29·507	29·437	29·04	55·5	65·5	53·2	59	51	54	NE.	N.	calm	·116	·58	·06	54
W. 30.	29·402	29·515	29·388	28·90	54·4	59·5	52·6	64	41	53·5	NNW.	NW.	calm	·602	·18	·07	55
● Th. 31.	29·456	29·446	29·396	28·95	56·4	59·3	49·2	67	43	54	SSW.	W.	calm	·041	·01	·05	54
	30·021	30·046	29·959	29·42	62·7	70·0	56·6	74·0	50·8	61·7				Sum 3·602	3·04	3·90	59·7

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NOVEMBER 1837.

LII. *Remarks upon the Botanical Affinities of* Orobanche. *By* John Lindley, *Ph.D., F.R.S., &c., Professor of Botany in University College London.*[*]

I DOUBT whether there is any part of natural history in which it is so difficult to distinguish between the relations of affinity and analogy as in systematic botany. It is, I conceive, to this cause that we must ascribe the widely different opinions entertained by botanists upon the manner in which the natural orders of plants should be arranged, and especially upon the sequences in which they should stand. It is only by a gradual determination of this very difficult point that we can hope to discover the true principle of classifying Exogens; a point of no little importance, considering how extensively the present fundamental distinctions of Polypetalæ, Monopetalæ, and Apetalæ or Incompletæ, interfere with affinity.

Supposing this opinion to be well founded, then, every case in which errors in regard to affinity can be made out becomes of great interest; I therefore beg leave to point out what I conceive to be a general mistake concerning the affinities of one of our British genera.

The genus *Orobanche* is placed next to *Scrophulariaceæ*, or considered a member of that order, by almost all systematic botanists. M. Kunth indeed placed it (*Handbuch der Botanik*, p. 411) between *Gesneraceæ* and *Solanaceæ*, but he assigned no reason for so doing. Subsequently Professor Schultz as-

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[*] Communicated to the Meeting of the British Association at Liverpool, September 1837.

serted that *Orobanchaceæ* have the flowers of *Personatæ* and the fruit of *Gentianaceæ*, and he accordingly stationed the order between *Gentianaceæ* and *Gesneraceæ* (*Naturliches System*, p. 395); and two years afterwards the Russian naturalist Horaninow placed the order under the name of *Phelypæaceæ*, between *Monotropaceæ* (confined to the genus *Monotropa*) and *Gesneraceæ*, stationing *Gentianaceæ* next the latter and separating the whole from *Scrophulariaceæ* by twelve natural orders (*Primæ lineæ Systematis Naturæ*, p. 73). It does not however appear whether these two last botanists formed their opinion from a correct knowledge of the real nature of the fruit of *Orobanche*; it would rather seem that they were led to their conclusion by the parietal placentation of that genus, a circumstance of no great consequence.

The great points of resemblance between *Orobanche* and *Scrophulariaceæ* are its monopetalous didynamous flowers and bicarpellary polyspermous fruit; and it is these which have led to the general opinion that the two genera are closely allied. Such marks of agreement are doubtless important; but they may be overbalanced by circumstances of disagreement of more importance. One of these is the position of the carpels with respect to the axis of inflorescence. In the whole category of *personate*, labiate, or irregular plants, the carpels stand fore and aft with respect to the axis; while in *Gentianaceæ*, and those orders which form what is generally reckoned the opposite symmetrical series, we have as universally the two carpels placed laterally. In this striking character *Orobanche* agrees with the latter series. Now as a didynamous structure is not universal in the one series, while the position of carpels is constant through both series respectively, we must assign the greater importance to the latter character, and hence *Orobanche* would be removed, far from *Scrophulariaceæ* and their allies, to the series represented by *Gentianaceæ*; of which this genus would be a didynamous form, *analogous* to what frequently occurs in the supposed opposite series.

But there is an essential point for consideration still behind. Botanists generally lay some stress upon the presence or absence of albumen as a mark of affinity between plants, even although the character, as it is usually understood, is confessedly very subject to exceptions. I have however endeavoured in another place to show, that when albumen becomes so abundant as to form the principal mass of the seed, it acquires a very different value from what it possesses when a mere stratum lying between the embryo and testa. In the former case it seems to be essential to the very existence and reproduction of the species; in the latter it may be regarded as a mere residuum of the nutrient mucilage in which the embryo is originally generated. If this be so, then it will follow that the excessive abundance of albumen is a high physiological character, which must be considered paramount to all peculiarities in the mere arrangement of the floral organs with respect to each other; for the latter, so far at least as the calyx and corolla are concerned, cannot be supposed to have any essential connection with the function of reproduction.

Now *Orobanchaceæ* differ from *Scrophulariaceæ* in their embryo being very minute and their albumen extremely copious. In these respects they again correspond with *Gentianaceæ.*

Altogether agreeing with *Orobanchaceæ* in the large mass of their albumen, and in their brown scaly parasitical habit, we find *Monotropaceæ,* placed in a distant part of the system by everybody except Horaninow. To this botanists have been led by the hypogynous stamens of the one order and the epipetalous stamens of the other; a character which so often produces artificial results, and which, I think, even the French school will be compelled to abandon as a great means of systematic distinction.

The supposed relationship of *Monotropa* to *Ericaceæ* through *Pyrola,* may seem an objection to the approximation of the former genus to *Orobanche* and *Gentianaceæ*; and if it were certain that *Pyrola was* related to *Ericaceæ* the objection would be a good one. But *Pyrola* differs from *Ericaceæ* in its albumen, just as *Gentianaceæ* and their allied orders differ from *Scrophulariaceæ*; and I cannot but regard the placing *Ericaceæ* and *Pyrolaceæ* in contact as another of those false approximations by which our ideas of classification are so much deranged. This is not the place to enter upon such a discussion, or I should be prepared to show that *Pyrolaceæ* are more nearly allied to *Droseraceæ,* and *Ericaceæ* to *Rutaceæ,* than to each other. *Pyrolaceæ* are related to *Ericaceæ* chiefly by their hypogynous stamens, porous spurred anthers and indefinite seeds, for it is an arbitrary application of terms to call them monopetalous. They are different in their whole habit, the organization of their seeds is essentially dissimilar, and the *Monotropaceæ* have not even porous anthers. But on the other hand they stand nearly related to *Orobanchaceæ* in their tendency to become leafless and parasitical, in the placentæ being half parietal in *Monotropa,* in their anthers having usually the spurs of *Orobanche,* and most especially in the proportion borne by the albumen to the embryo.

I would therefore submit that, for the reasons now assigned, *Pyrolaceæ* (including *Monotropaceæ*), *Orobanchaceæ* and *Gentianaceæ,* instead of standing distantly apart in the natural system, must be considered nearly related members of the same natural series.

I am unwilling to dismiss this subject without adverting to the placentation of *Orobanchaceæ.* That their capsule consists of two carpels standing right and left of the axis of inflorescence, and with the margins not inflected in the form of dissepiments, is incontestable. Yet in *Orobanche* and *Phelypæa* the capsule has four placentæ, placed equidistant in pairs upon the face of each valve or carpel, and considerably within the margin. In *Epiphegus* each carpel has two intramarginal placentæ, which diverge from the base upwards, and terminate before reaching the apex. In *Lathræa* there is to each valve but one placenta, which may be regarded as two confluent ones occupying the very face of the dorsal suture of the carpel. And finally in *Æginetia indica*, and I believe in *Æginetia abbreviata* also, the placenta is in like manner confined to the axis of the valve, occupying the same position upon the carpels as in *Lathræa*, but broken up into a number of parallel plates of unequal depth, over the whole surface of which multitudes of minute seeds are distributed. If we connect these facts, about which there can be no sort of question, with the well known placentation of *Flacourtiaceæ*, *Nymphæaceæ*, and *Butomaceæ*, we shall find that they invalidate a general carpological rule, that the placentæ belong to the ventral suture of a carpel, and consequently alternate with the dorsal; and we shall have to admit that the position of the placentæ with regard to the margins of the carpel is reducible to no certain rule, but depends upon specific organization. Consequently we shall no longer be unable to account for the unusual situation of the placentæ opposite the stigma, in *Papaver*, (as M. Kunth has lately noticed) in *Parnassia*, or elsewhere.

We ought not indeed to be surprised at coming to this result; for if the ovules are, as botanists generally believe them to be, a modification of buds, then the uncertainty in the position of the placentary lines will only be conformable to the uncertainty in the origin of buds from leaves. If in *Bryophyllum*, *Malaxis paludosa*, and most other cases they usually spring from the edge of the leaf, they also arise from its surface in ferns; and in the famous case of the *Ornithogalum* leaf mentioned by Turpin, they were found issuing indiscriminately from all parts of its face.

LIII. *Further Observations on the Structure of the Solid Materials found in the Ashes of recent and Fossil Plants. By the Rev.* J. B. Reade, *M.A.*[*]

IN every inquiry connected with the physiology of plants, it will be necessary to bear in mind that "the chief end and object of the various processes into which the function of nutrition may be divided, is *the manufacture of the materials* which are ultimately to be assimilated into the vegetable structure, and by which it is to be nourished and developed in all its parts[†]." In the practical application of this important principle all writers on physiological botany, whether in England or on the Continent, have adopted the opinion that carbon alone, fixed under the form of a nutritive material, is elaborated for the development of all parts of vegetable structure. "The solid materials," says M. Biot, "the fixation of which constitutes the skeleton of the plant......may be known by the analysis of the dead or withered vegetable, but even among these we have to distinguish those which are essential to the existence of the plant and those which have been accidentally raised from the earth by the roots with the water in which they were dissolved." "I shall be careful, therefore," continues M. Biot in his Analysis of the Vegetation of the *Gramineæ*[‡], "not to commit myself in these complex questions, for which all the assistance of chemistry and of the microscope is scarcely sufficient. I shall confine my remarks to a few of the alimentary products of plants which are known to be composed by them, and conveyed into their various parts, whilst undergoing the metamorphosis produced by vitality."

Among the solid materials represented by M. Biot as accidentally taken up, Professor Lindley ranks earths, salts, and metals. In fact, he considers all principles which cannot be referred to either hydrogen, oxygen, carbon, or azote to be *foreign to plants.* (Introduction to Botany, book ii. chap. 3.) "There are, however, some experiments," he adds, "which, if they could be depended on, would materially weaken these hypotheses. Schrader grew barley and rye in well washed flower of sulphur moistened with distilled water: they were afterwards analysed, and found to contain silex, lime, and magnesia as well as oxides of iron and manganese. The same plants produced in earth did not yield a greater weight of ashes than those grown in sulphur; and these experiments are confirmed by those of Braconnot as recorded in the *An-*

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[*] Communicated by the Author; whose former paper on the same subject will be found at p. 13 of the present volume.

[†] Cabinet Cyclopædia. Henslow's Principles of Botany, p. 227.

[‡] Scientific Memoirs, vol. i. p. 585.

nales de Chimie, vol. lxi. p. 187. ...... Both these observers conclude that these foreign principles were produced by the organic power of vegetation." Unfortunately, however, these experiments, and others similar to these, though conducted by able naturalists, have as yet failed to gain any very general acquiescence in the conclusions which are drawn from them. And this arises from the gratuitous supposition that not only undetected sources of error may possibly have existed, but that the very presence of foreign matter in the ashes proves error to be unavoidable.

The facts which I have already brought forward, in my former paper, have led me to adopt the conclusion of Braconnot, and to dismiss, as altogether untenable, any supposition of *accidental introduction* with respect to the elements of vegetable structure. For, if it is not to be doubted but that the presence of organization is direct evidence of the presence and agency of life, and that every organized portion of a plant is a proper product of the power of vegetation, then it will follow, that the place which the solid materials occupy is an important and an appointed one, and we shall be warranted to say, under a more extended application of Professor Henslow's own remark, that the process of respiration *prepares the organizable materials**, whether carbonaceous, siliceous, saline, or metallic, from whose subsequent elaboration are derived those infinitely varied conditions of organized matter which are essential to the development of the numerous tribes of plants; and here I cannot but add in the eloquent language of this author, that "from *these same materials* are constructed those organized substances which seem to stand as portals to the intellectual and spiritual world—channels of direct communication by which reason and revelation may tell the frail tenants of a few mouldering atoms, of that more glorious condition which will as certainly be their heritage hereafter as their hopes and yearnings after immortality are within the actual experience of their present state."

On communicating the nature and result of my experiments to Professor Henslow I was gratified by receiving the following reply: "Mr. R. Brown, with whom I have had some conversation upon the subject of your letter, confirms your conclusion that silica *enters as an organizable product* into the structure of plants, and I have now no doubt of the fact from what you and he have stated, though I had not suspected it before. Mr. Brown told me that he had long since made experiments similar to yours, but had never published

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* Principles of Botany, p. 202.

them, and I had not heard of them." I have myself also been favoured by Mr. Brown with some account of his experiments. He showed me in the ashes of one plant, what I have never seen elsewhere, a very beautiful form of siliceous cellular structure, having the walls of the cells covered with minute siliceous granules; and in another plant, owing to the presence of some alkaline product, the silica which it contained was readily fused, and Mr. Brown was therefore able, by careful incineration, to procure perfect spherical lenses.

Having now satisfactorily ascertained that the siliceous organization of recent plants is not destructible even under the blowpipe, it appeared to me to follow, as a natural inference, that the less intense heat of a common fire would not destroy the siliceous organization in the fossil plants of coal. Accordingly, I find in the white ashes of coal all the usual forms of vegetable structure, viz. cellular structure, smooth and spiral fibre, and annular ducts. An engraving of some of these forms accompanied my former paper, and perhaps a still more accurate idea may be given of them if I refer to the late Mr. Slack's drawings of the tissue of plants in the Transactions of the Society of Arts, vol. xlix. plate 6. The elementary organs represented by Mr. Slack possess of course their due admixture of carbonaceous matter, and there can be no doubt that the removal of the carbon, by fire, would be universally supposed, *à priori*, to be accompanied with the destruction of the tissue. But so far from this being the case, the most delicate markings remain undisturbed, and I can confidently refer to figures 7, 12, 21 *b*, 23, 26, 27, 28, 30, as most accurate representations of the vegetable forms which occur in the white ashes of coal.

The vegetable origin of coal, which even at this day is, by some persons, admitted rather than believed*, is thus clearly proved, and from a comparison of the ashes of coal with those of recent plants, some further insight might probably be gained into the nature of fossil vegetables. To mention only one instance, I have already ascertained that the lumps of carbonized matter which occur abundantly in the upper sandstone near the Spa at Scarborough, are in all probability portions of the stem of some arundinaceous or gramineous plant. The structure of the epidermis is precisely similar to that of the oat, consisting of parallel columns set with fine teeth, dove-tailing, as it were, into each other, while the underlying tissue consists of cubical cells, a thin horizontal section ex-

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[* In order, however, to obtain complete ideas of the nature of coal, it will be requisite to compare Mr. Reade's results, as above, with Mr. Hutton's "Observations on Coal," Lond. and Edinb. Phil. Mag., vol. ii. p. 302.—Edit.]

hibiting a series of squares. The vegetable forms which are respectively furnished by the Blyth, Newcastle, and Barnsley coal appear to be so different, that I think by the aid of the microscope it would be almost possible to assign an unknown specimen to its proper locality; and this possibility cannot but have its due weight with the geologist. From these facts it is evident that the true framework and basis of vegetable structure in the plants of coal is not only entirely independent of carbon, but that it has also resisted the bituminous decomposition which has converted all the carbonaceous materials into a highly inflammable substance.

I find, upon further investigation, that silica is not the only material which forms the framework of plants. Lime and potash also occur as vegetable skeletons. The ashes of the calyx and pollen of the mallow, for instance, consist of organized lime, and the ashes of the petals of the rose consist of organized potash. The latter quickly deliquesce, and upon the addition of nitric acid, permanent crystals of nitrate of potassa are readily formed. The ashes of the pollen of the geranium also consist of potash. Hence, it is no longer " a great question," to use the words of Dr. Dalton, " whether potash is a constituent principle of vegetables or formed during their combustion*;" for it is evident, from the facts now adduced, that potash, lime, and silica equally enter as *organizable products* into the structure of plants.

In consequence of the medicinal properties of some plants being similar to those of the fixed materials which enter as organizable products into their structure, perhaps we may fairly ask if there be any necessary connexion between the medicinal properties of plants and the nature and properties of their solid materials. It is obvious that this question could not be entertained so long as the solid materials of plants, carbon alone excepted, were looked upon as *foreign matter, accidentally introduced.*

It has been objected by some to whom I have communicated my opinions that the siliceous structure in the ashes of gramineous plants arises from the agency of heat and is due to crystallization; and others again have supposed that the process of respiration not only causes a large deposition of silica on the epidermis, but also leaves a similar coating upon the organs of the vascular tissue. The first of these objections is at once and easily removed by showing that in an unburnt portion of the husk of oat, for example, when placed in Canada balsam, we can trace the very same series of siliceous columns which the burnt specimen exhibits; and that, in fact, the similarity is so great that they have often been mistaken

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* Dalton's Chemical Philosophy, vol. i. p. 472.

the one for the other. The second objection has apparently more force, but I am able to prove that the vessels are not coated with silica, but are actually composed of silica, by showing that if the silica be removed, no trace of vessel remains. I could wish to give particular importance to the mode of working out this result, and to some of the facts connected with it, because much light appears to be thrown upon the nature of a new and beautiful portion of vegetable structure, I mean the system of, apparently, small cups, which are placed along the siliceous columns of gramineous plants*.

In order to effect the removal of silica without disturbing other parts of the plant, I placed a small portion of one of the lower leaves of a stalk of wheat for upwards of twelve hours in caustic potash. After removing the potash by dilute muriatic acid, I mounted half of the specimen in balsam, and then expelling the carbon from the other half by the aid of a spirit-lamp, I inclosed this portion also in the same substance. These I compared with each other and with the adjoining part of the leaf in its natural state. The caustic potash had effected the entire removal of the system of siliceous vessels between the ribs of the leaf, but the small cups which are duly arranged along the siliceous columns remained undisturbed. These cups, therefore, are not composed of silica; neither are they carbonaceous, for after resisting the action of potash, they resist also the action of fire. This is not the case with the ducts, &c. which form the ribs of the leaf; they are readily carbonized and dissipated. But after the carbonaceous parts have been thus expelled, and these cups alone remain, if then a sufficient degree of heat be applied to effect their fusion, they leave upon the platinum spoon a permanent light-blue stain. It would appear, therefore, that the metallic oxides, which are always found in the ashes of wheat, exist in the plant under an organized form, and are obtained by incineration from this system of cups. And, hence, I conclude generally that earthy, saline, and metallic ingredients enter as organizable products into the structure of plants.

Peckham, Oct. 2, 1837.

LIV. *On the Wave-surface in the Theory of Double Refraction.*

By J. W. Lubbock, *Esq., F.R.S.*†

SINCE Fresnel's discovery of the wave-surface published in his admirable paper on *Double Refraction* (*Mémoires de l'Institut*, tome vii.) much has been done to elucidate this

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* See the figure, Plate I. Phil. Mag., July 1837.

† Communicated by the Author.

subject by eminent mathematicians; perhaps however it may not be uninteresting to return to Fresnel's reasoning through which the equation to the wave-surface was originally established, so as to facilitate the comparison of Fresnel's ideas with those which have been since developed by M. Cauchy, and by other philosophers*. With this view I have drawn up the following very brief remarks. It seems to have been generally considered that M. Cauchy's wave-surface is identical with that of Fresnel, but I confess that I do not think that this point has been sufficiently considered; and indeed it has already been asserted by Mr. Kelland that little construction beyond the explanation of dispersion can be put upon M. Cauchy's results, from their great complexity.

Let the displacements of the particle $\mathfrak{m}$ in the directions of x, y, z be ξ, η, ζ, and those of $\mathfrak{m}'$, $\xi + \Delta\xi$, $y + \Delta y$, $\zeta + \Delta\zeta$ at the end of the time t, then

$$\frac{d^2\xi}{dt^2} = \mathfrak{m}\,\Sigma \Bigg\{ (r\phi + \psi(r)\Delta x^2\}\,\Delta\xi + \psi r\,\Delta y \Delta x\,\Delta\eta$$

$$+ \psi r\,\Delta x\,\Delta z\,\Delta\zeta \Bigg\}$$

$$\frac{d^2 y}{dt^2} = \mathfrak{m}\,\Sigma \Bigg\{ \psi r\,\Delta y\,\Delta x\,\Delta\xi + \{\phi r + \psi(r\,\Delta y^2\}\,\Delta\eta$$

$$+ \psi r\,\Delta y\,\Delta z\,\Delta\zeta \Bigg\}$$

$$\frac{d^2\zeta}{dt^2} = \mathfrak{m}\,\Sigma \Bigg\{ \psi r\,\Delta x\,\Delta z\,\Delta\xi + \psi r\,\Delta y\,\Delta z\,\Delta\eta$$

$$+ \{\phi r + \psi(r)\,\Delta z^2\}\;\Delta\zeta \Bigg\}.$$

These are M. Cauchy's equations, Exercises, vol. iii. p. 192: they are also given by Mr. Tovey, L. & E. Phil. Mag., vol. viii. p. 9; and by Mr. Kelland, Cambridge Trans., vol. vi. p. 158.

The remarks of Fresnel, p. 85 to 95, *Mém. de l'Institut*, vol. vii. amount to showing that these equations may, when the *axes of elasticity* are taken for the coordinate axes, be reduced to the form

$$\frac{d^2\xi}{dt^2} = \mathfrak{m}\,\Sigma \Big\{ \phi r + \psi(r)\,\Delta x^2 \Big\} \Delta\xi$$

$$\frac{d^2\eta}{dt^2} = \mathfrak{m}\,\Sigma \Big\{ \phi r + \psi(r)\,\Delta y^2 \Big\} \Delta\eta$$

$$\frac{d^2\zeta}{dt^2} = \mathfrak{m}\,\Sigma \Big\{ \phi r + \psi(r)\,\Delta z^2 \Big\} \Delta\zeta.$$

[* Prof. Powell's "Abstract of M. Cauchy's Views" appeared in Lond. and Edinb. Phil. Mag., vol. vi. p. 16, *et seq.*—EDIT.]

If $\Delta\xi^2 + \Delta\eta^2 + \Delta\zeta^2 = \Delta\varrho^2$, and if X, Y, Z denote the angles which the direction of displacement makes with the coordinate axes,

$$\Delta\xi = \Delta\varrho\cos X \qquad \Delta\eta = \Delta\varrho\cos Y \qquad \Delta\zeta = \Delta\varrho\cos Z,$$

X, Y, Z being the same as in Fresnel's notation,

$$\cos^2 X + \cos^2 Y + \cos^2 Z = 1.$$

The simplest case, and I believe that implicitly intended by Fresnel, is when the amplitude $\Delta\varrho$ depends only upon the distance r from the origin, then

$$\Delta\varrho = \frac{d\varrho}{dr}\Delta r + \frac{d^2\varrho}{1.2\,dr^2}\Delta r^2 + \frac{d^3\varrho}{1.2.3\,dr^3}\Delta r^3 + \frac{d^4\varrho}{1.2.3.4\,dr^4}\Delta r^4 + \&c.$$

neglecting the sums of the odd terms *

$$\mathfrak{m}\,\phi\, r\,\Delta r, \quad \mathfrak{m}\,\psi\, r\,\Delta x^2\,\Delta r, \&c.$$

we find equations of the form

$$\frac{d^2\xi}{dt^2} = a^2\cos X\frac{d^2\varrho}{dr^2} + \mathfrak{a}^2\cos X\frac{d^4\varrho}{dr^4}$$

$$\frac{d^2\eta}{dt^2} = b^2\cos Y\frac{d^2\varrho}{dr^2} + \mathfrak{b}^2\cos Y\frac{d^4\varrho}{dr^4}$$

$$\frac{d^2\zeta}{dt^2} = c^2\cos Z\frac{d^2\varrho}{dr^2} + \mathfrak{c}^2\cos Z\frac{d^4\varrho}{dr^4}.$$

$$a^2 = \frac{\mathfrak{m}}{2}\Sigma\left\{\phi r + (\phi r)\,\Delta x^2\right\}\Delta r^2$$

$$\mathfrak{a}^2 = \frac{\mathfrak{m}}{24}\Sigma\left\{\phi r + (\psi r)\,\Delta x^2\right\}\Delta r^4$$

$$b^2 = \frac{\mathfrak{m}}{2}\Sigma\left\{\phi r + (\psi r)\,\Delta y^2\right\}\Delta r^2$$

$$\mathfrak{b}^2 = \frac{\mathfrak{m}}{24}\Sigma\left\{\phi r + (\psi r)\,\Delta y^2\right\}\Delta r^4$$

$$c^2 = \frac{\mathfrak{m}}{2}\Sigma\left\{\phi r + (\psi r)\,\Delta z^2\right\}\Delta r^2$$

$$\mathfrak{c}^2 = \frac{\mathfrak{m}}{24}\Sigma\left\{\phi r + (\psi r)\,\Delta z^2\right\}\Delta r^4$$

a^2, b^2, c^2 being constant and the same as in Fresnel's notation.

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* Mr. Kelland makes $\Sigma\,\varphi(r)\,\delta x^2 = \Sigma\,\varphi(r)\,\delta y^2 = \Sigma\,\varphi(r)\,\delta z^2$.
Cambridge Trans., vol. vi. p. 159.
But this I apprehend is a different case from that contemplated by Fresnel.

$$\frac{d^2 \varrho}{d t^2} = \cos X \frac{d^2 \xi}{d t^2} + \cos Y \frac{d^2 \eta}{d t^2} + \cos Z \frac{d^2 \zeta}{d t^2}$$

$$\frac{d^2 \varrho}{d t^2} = \left\{ a^2 \cos^2 X + b^2 \cos^2 Y + c^2 \cos^2 Z \right\} \frac{d^2 \varrho}{d r^2}$$

$$+ \left\{ \mathfrak{a}^2 \cos^2 X + \mathfrak{b}^2 \cos^2 Y + \mathfrak{c}^2 \cos^2 Z \right\} \frac{d^4 \varrho}{d r^4}$$

If
$$v^2 = a^2 \cos^2 X + b^2 \cos^2 Y + c^2 \cos^2 Z$$
$$\mathfrak{v}^2 = \mathfrak{a}^2 \cos^2 X + \mathfrak{b}^2 \cos^2 Y + \mathfrak{c}^2 \cos^2 Z$$
$$\frac{d^2 \varrho}{d t^2} = v^2 \frac{d^2 \varrho}{d r^2} + \mathfrak{v}^2 \frac{d^4 \varrho}{d r^4}.$$

This equation is readily integrable if v and $\mathfrak{v}$ are constant, that is, if the direction of the displacement be invariable; and ϱ may be expressed by a series of terms similar to

$$(a \sin k r + b \cos k r) \sin n t + (a' \sin k r + b' \cos k r) \cos n t,$$

and represents a wave of light moving in the direction of r with the velocity $\frac{n}{k}$,

$$\frac{n}{k} = v \left\{ 1 - \frac{k^2}{2.3.4} \; \frac{\mathfrak{v}^2}{v^2} \right\}.$$

See Mr. Tovey's excellent paper, L. & E. Phil. Mag. January 1836.

Fresnel does not implicitly take into consideration the term $\frac{\mathfrak{v}^2 \, d^4 \varrho}{d r^4}$, which according to M. Cauchy is necessary to explain *dispersion*. We have then

$$\frac{d^2 \varrho}{d t^2} = v^2 \frac{d^2 \varrho}{d r^2} \qquad\qquad v = \frac{n}{k}.$$

Fresnel supposes that the quantity v^2 will be constant when the force revolved in a direction perpendicular to the direction of displacement $\Delta \varrho$ is also perpendicular to a given plane, and he shows that this will be the case precisely when the quantity $v^2 = a^2 \cos^2 X + b^2 \cos^2 Y + c^2 \cos^2 Z$ is a maximum or minimum. If the given plane is parallel to the plane

$$n x + n y + l z = 0.$$

Fresnel finds by geometry and without further assumptions the equation

$$(a^2 - v^2)(c^2 - v^2) n^2 + (b^2 - v^2)(c^2 - v^2) m^2$$
$$+ (a^2 - v^2)(b^2 - v^2) l^2 = 0.$$

The calculation of Fresnel's equation to the wave-surface is also now a purely geometrical problem which is easily effected by the elegant artifices suggested by Mr. Smith in the vith volume of the Transactions of the Cambridge Philosophical Society, p. 85.

This equation to the wave-surface is

$$(x^2+y^2+z^2)\,(a^2\,x^2+b^2\,y^2+c^2\,z^2)-a^2\,(b^2+c^2)\,x^2 \\ -b^2\,(a^2+c^2)\,y^2-c^2\,(a^2+b^2)\,z^2+a^2\,b^2\,c^2 = 0.$$

The coordinate axes being those of elasticity.

M. Cauchy has arrived at a similar equation, which is given at the foot of page 63, vol. v. of the Exercises. M. Cauchy seems to consider this equation identical with that of Fresnel. See *Mémoires de l'Institut*, tom. x. p. 312. See also Mr. Lloyd's excellent Report on Physical Optics, Fourth Report of the British Association, p. 391. In order that this may be the case I apprehend that it would be necessary to have

P in M. Cauchy's notation identical with a^2 in that of Fresnel.
Q b^2
R c^2

which I confess does not seem to me to have been proved. Moreover Fresnel's wave-surface depends upon the situation of certain fixed axes (of elasticity), while M. Cauchy's wave-surface does not appear to depend upon them in the same manner, and therefore I suspect that the conclusions of that eminent mathematician are so far in discordance with those of Fresnel. I submit this opinion with great deference to those more conversant than myself with physical optics and with M. Cauchy's works, but at all events I think it will be admitted that the question is of sufficient importance to deserve further elucidation by showing, if it be possible, that M. Cauchy's quantities P, Q, R have the same signification as Fresnel's a^2, b^2, c^2.

LV. *On the Chemical Composition of Vegetable Membrane and Fibre*[*]*; with a Reply to the Objections of Professor* Henslow *and Professor* Lindley. *By the Rev.* J. B. READE, *M.A.*

IT is stated by Professor Henslow in his "Descriptive and Physiological Botany[†]" that all that is known of the chemical composition of the two elementary textures of plants, viz. *membrane* and *fibre*, has been derived from experiments made upon the gross material imperfectly separated from the various matters which the cells and tubes contain; that in this state the gross material is found to be composed of the

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[*] Read before Section D, Zoology and Botany, of the Seventh Meeting of the British Association held at Liverpool; and now communicated by the Author.

[†] Cabinet Cyclopædia. Henslow's Principles of Botany, p. 13.

three elements, oxygen, hydrogen, and carbon, the exact proportion in which these are united being uncertain; and that therefore it remains to be ascertained whether the two organic elements of vegetable structure are identical in chemical composition, and whether the membrane and fibre which compose the cells and tubes in different parts of plants are always of the same kind. It is further observed by the same author that an inquiry into this subject would be one of extreme difficulty, if not *of absolute impossibility with the present resources of chemistry*.

It gives me great pleasure to be able to lay before the members of the British Association a few well ascertained facts on a subject, of which the difficulty has hitherto prevented it from being placed even among the debatable questions in the philosophy of botany. Having, on many occasions, had the good fortune to witness the admirable tact with which Mr. Robert Rigg, of Walworth, effects the chemical analysis of vegetable products, I felt convinced, when I saw Professor Henslow's remarks, that the difficulty, which has appeared to be almost insurmountable, would be readily overcome in my friend's laboratory*. I therefore separated the spiral vessels which form the central column of the roots of the hyacinth from their surrounding cellular tissue, and the membrane and fibre thus obtained furnished upon analysis the following results.

Spiral Vessels.		Cellular Tissue.	
Carbon	41·8	Carbon	39·2
Hydrogen	1·1	Oxygen	7·4
Nitrogen	4·3	Nitrogen	3·9
Water	51·8	Water	48·5
Residual matter	1·0	Residual matter	1·0
	100·		100·

Experiments made upon the gross material, and without separating the fibre from its investing membrane, give hydrogen and oxygen in the proportion to form water. This fact, which has been long known to Mr. Rigg, admirably confirms the separate experiments which were made at my suggestion, and from which it appears that the excess of hydrogen in the fibre, and excess of oxygen in the membrane, are also so nearly in the proportion to form water. It is also found that the petals of the hyacinth contain an excess of oxygen, while the pistils and pollen are marked by an excess of hydrogen. This

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[* Notices of papers by Mr. Rigg on Fermentation and the Chemical Changes of Germination will be found in Lond. and Edinb. Phil. Mag., vol. ix. p. 535.—Edit.]

I quite expected would be the case, from observing that those spiral vessels which line the interior surface of the hollow flower-stalk, emerge, not into the petals but into the pistils and stamens of the flowers.

In order to carry out this investigation still further, I separated the flower-stalk of the hyacinth into four distinct parts, among which the obvious variety both of structure and functions is not more marked than the diversity of their chemical composition. The parts to which I allude are,

The *epidermis* with its stomata.

The soft, bright green *cellular tissue* beneath the *epidermis.*

The compact *column of ducts and fibre* forming the main support of the stalk.

And the *spiral vessels* which line the inner surface of this hollow column.

Mr. Rigg has furnished me with the following analysis of these different portions of the plant.

Analysis of the Flower-stalk of a Hyacinth grown in Water.

	Carbon.	Hydrogen.	Oxygen.	Nitrogen.	Water.	Residual matter.	
Epidermis with stomata, dried at 100° Fahr.	41·7	...	2·0	4·0	50·8	1·5	100
Cellular tissue beneath the epidermis, do. do.	41·8	...	2·1	4·1	50·5	1·5	100
Column of ducts and fibre beneath cellular tissue, do. do.	39·2	0·5	...	3·7	55·6	1·0	100
Spiral vessels lining the inner surface of this column, do.	35·8	1·7	...	3·9	58·1	0·5	100

On the existence of nitrogen in these products, which is not the least remarkable feature in these experiments, an original memoir will be laid before the Association.

And as I have discharged my office, which is to record facts only, and not at all to theorize upon them, I will conclude this short notice of an inquiry, now less encumbered with difficulty, by observing that an opinion, which not long ago was a common one*, that membrane only is the basis of the tissue of plants, and that fibre is itself a form of membrane, is shown by the above analysis to be decidedly erroneous.

Peckham, Aug. 28, 1837.

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* Lindley's Introduction to Botany, p. 2.

Objections to the foregoing Statement, copied from the Report of the Proceedings of the Association given in "The Athenæum," September 16.

"Professor Henslow observed that in his work he had alluded to the great difficulty of isolating entirely either membrane or fibre. The cells of the cellular tissue must contain some fluid matter in their interior, besides the fibre that lined them externally [? internally]. Mr. Rigg had experimented on spiral vessels which contained both membrane and fibre; therefore the ultimate composition of membrane and fibre was still a desideratum. Professor Lindley said it was important that facts of this kind should be well made out. As a proof of want of care in the paper, it might be inferred, from the author's statements, that there were no spiral vessels in the petals of the hyacinth; but the fact is, they are very abundant. It appeared to him that the author had mistaken cellular tissue and woody fibre, for the elementary membrane and fibre, the chemical analysis of which was so difficult."

Reply to the above Objections.

To Professor Lindley.

Sir, Peckham, Sept. 18, 1837.

I find from the Report of the proceedings of the "British Association" which appeared in "The Athenæum" on the 16th instant, that my short notice of the chemical composition of vegetable membrane and fibre was met by Professor Henslow and yourself with three objections, which it will be incumbent upon me to remove before you can possibly attach any value to the facts which were adduced. I therefore trust that you will have the goodness to allow me to lay before you, by letter, that reply which my absence from Liverpool prevented me from making on the spot.

Following the arrangement adopted by Professor Henslow in his "Principles of Botany*," it may be stated that the *two elementary textures* of plants are membrane and fibre; of these materials the *two elementary organs*, viz. cells and tubes, are constructed; and of these organs, the *two elementary tissues*, viz. the cellular tissue and the vascular tissue, are respectively composed.

It is first of all necessary to inquire how membrane and fibre are employed in the construction of the elementary organs; whether they invariably occur conjointly in each of the two kinds of organs, or whether these organs are respectively composed of the one or the other only.

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* Cabinet Cyclopædia, Henslow's Principles of Botany, pp. 13, 14.

Now it will be found upon examination that the vesicles of the cellular tissue are formed, with comparatively few exceptions, of *membrane only*; and though "vascular tissue," as Professor Henslow observes[*], "consists of tubes which are also formed of membrane to all appearance identical with that *which composes the vesicles of cellular tissue*," yet with respect to the "true vessels or long tubes which more strictly compose the vascular tissue," we find in the particular case of spiral vessels, that the membrane is so slight, that "if a vessel be ruptured and the spiral fibre uncoiled, no trace of the membrane is to be seen excepting towards the conical extremity of the vessel, and also where the successive coils are not in contact with each other[†].

It therefore appears to follow that *membrane* may be isolated by obtaining the more common form of cellular tissue[‡], where the walls of the cells are not "lined externally with fibre," and then evaporating the fluid matter contained in the cells; and also, that spiral vessels, entirely divested of the cellular tissue in which they are generally imbedded, form the nearest practicable approximation to isolated *fibre*[§].

In order then to obtain these materials in a state fit for chemical analysis I had recourse to the roots of a hyacinth grown in water. Each root consists of a central column of spiral vessels surrounded by cellular tissue. When the plant is young, the spiral vessels, having a diameter of about the $\frac{1}{500}$th of an inch, are not easily separable, even by boiling, from the tissue in which they lie. But when the plant has arrived at maturity, the entire column of spiral vessels having now, in many instances, a diameter of nearly the $\frac{1}{250}$th of an inch[||], as well as a considerable increase in the width of the spiral thread, may by a little management (in fact, by simply rubbing the root between the fingers and the palm of the hand), and without boiling, be readily drawn out of its sheath of cellular tissue; and each vessel is so free from any admixture of cel-

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[*] Principles of Botany, p. 20.

[†] *Ibid.*, p. 21.

[‡] "Membranous cellular tissue is that in which the sides consist of membrane only, without any trace of fibre; it is the most common, and was, till lately, supposed to be the only kind that exists."—Lindley's Introduction, p. 8.

[§] "In the root of the hyacinth the coils of the spiral vessel *touch each other*, except towards its extremities."—Lindley's Introd., p. 19.

[||] "It is an axiom in vegetable physiology, that *a part once fully formed is incapable of any subsequent change*. Thus, pith never alters its dimensions, after the medullary sheath that incloses it has been once completed."—Lindley's Introd., p. 33.

This being the case, the larger vessels must have a later formation, and the number of vessels in the young and old roots ought to indicate this fact.

lular tissue that it might be represented by the engraving in your own work, Plate II. fig. 9.

With membrane and fibre isolated to this extent, Mr. Rigg commenced his experiments; and I believe that I am not in error when I state that the ultimate analysis of these textures was thus for the first time attempted. There is no record of similar experiments, for, as Professor Henslow justly observes*, "all that is known of the composition of these textures has been derived from experiments made upon the *gross material*, which is found to be composed of the three elements, oxygen, hydrogen and carbon;" a result which clearly shows that the gross material referred to consisted of *both* the elementary organs, viz. cells and tubes, as they conjointly enter into vegetable structure.

But even allowing Professor Henslow's objection its full force, and granting that "fibre externally lined the cells of the cellular tissue" analysed by Mr. Rigg; and also that "the spiral vessels contained both membrane and fibre;" then, bearing in mind that "it has not hitherto been ascertained whether the membrane and fibre which compose the cells and tubes in different parts of plants are always of the same kind†", I would offer the result of the analysis as a proof that the membrane and fibre which compose the cells are essentially different, in a chemical point of view, from the membrane and fibre which compose the spiral vessels; the former invariably containing an excess of oxygen, and the latter an excess of hydrogen. And thus we appear to arrive at the chemical composition of the *elementary tissues*, and that too in such a way as to be an important approximation to the composition of the *elementary textures*; for it cannot be doubted that membrane predominates over fibre in the cells, and that fibre predominates over membrane in the spiral vessels.

I learn, from the report in "the Athenæum," that you commenced your own observations by stating, that "it is important that facts of this kind should be well made out;" a remark which encourages me in the hope that you will favourably receive my present attempt to substantiate the results of my friend's experiments. But you go on to charge me with the fault, condemned by a great authority:

"Aut operæ nimiùm celeris curâque carentis,
Aut ignoratæ...artis."

For you proceed, "As a proof *of want of care* in the paper,

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* Principles of Botany, p. 13.

† *Ibid.*

it might be inferred from the author's statements that there were no spiral vessels in the petals of the hyacinth; but the fact is, they are very abundant." Had I been present I might at once have obviated this objection by pointing out that it was founded on misapprehension, and by appealing to the words of my paper, where I state, that "*those* spiral vessels *which line the interior surface* of the hollow flower-stalk, emerge, *not into the petals*, but into the pistils and stamens of the flowers," from which the inference would be very different from the one you suggest.

The facts brought forward are these: The spiral vessels of the root give hydrogen in excess, and the cellular tissue of the root gives oxygen in excess. It is also found upon analysing different parts of the flower that the petals contain an excess of oxygen, while the pistils and pollen are marked by an excess of hydrogen. Hence, the question arises, Is there any such connection between the root and the flower as shall enable us to trace the excess of hydrogen in the pistils and pollen of the flower, to the excess of hydrogen in the spiral vessels of the root? I answer that there is, for it is found upon examination that those spiral vessels which line the interior of the hollow flower-stalk proceed directly to those parts of the flower which are marked by an excess of hydrogen, emerging, *not into the petals*, but into the pistils and stamens. And hence I wish the inference to be that *those spiral vessels* which run within the hollow flower-stalk, and contain hydrogen in excess, *are a continuation of the spiral vessels of the root*, and that the spiral vessels of the petals and leaves of the plant have their origin elsewhere. It may be observed with respect to dicotyledonous plants that the latter part of this conclusion would appear to be inevitable, in as much as spiral vessels abound in the leaf-stalks (see Dogwood) and leaves, but "*have not hitherto been seen in the roots.*" Lindley's Introduction, p. 22. I would however take this opportunity of inviting the attention of botanists to the more accurate microscopic examination of the roots of dicotyledonous plants; for I can only conclude that spiral vessels have not been seen, merely because they have not been looked for. The root of the common garden mint, to instance one example among others, contains spiral vessels in great abundance. Their diameter is about $\frac{1}{600}$th of an inch. I will also add that the root of the wall-flower contains a very beautiful and hitherto undescribed form of reticulated cellular structure. It is perhaps necessary to state that in consequence of the great similarity between the brittle annular duct and the true spiral vessel, it is impossible to speak with decision as to the smaller forms of these tissues without using a magnifying power of at least 800 linear. I have had the advantage of making my examination with the well-known power which Mr. Bowerbank has received from Mr. Ross.

I believe that no part of the analysis gave Mr. Rigg more satisfaction than the results just alluded to. In the course of his experiments, extending to some thousands, he had heretofore discovered a difference of chemical composition in different parts of the flower of the hyacinth; but the root, which he had always examined in the mass, and without separating it into its two component parts, gave hydrogen and oxygen in the proportion to form water. I am sure, therefore, you will readily understand the pleasure which was felt in thus finding, for the first time, that the chemical character of the different parts of the flower were exhibited by the present analysis in the different parts of the root.

I will only add, without further observation, that I trust the above remarks will convince you, that I have not "mistaken cellular tissue and woody fibre for the elementary membrane and fibre, the chemical analysis of which is so difficult."

I have the honour to be, Sir, yours, &c.

J. B. Reade.

LVI. *On the Powder formed by the Action of Water on White Precipitate. By* Robert Kane, *M.D., M.R.I.A., &c. &c.*[*]

It is generally stated by chemical writers, that by the action of much boiling water, white precipitate is decomposed completely, red oxide of mercury being left behind. I never could succeed in effecting this; but the reaction that did take place appearing to me perfectly definite, and identical in its results at different times, I was induced to examine it in detail.

When white precipitate is boiled in water, it is changed into a heavy canary-yellow powder, subsiding rapidly, and very easily dried, when it appears granular. This powder is not quite insoluble in water; when heated, it gives out ammonia, azote, water, and there sublimes a mixture of calomel and metallic mercury: it dissolves readily in muriatic or nitric acids. Alkalies appear to have scarcely any action upon it, except slightly altering its colour; when digested with iodide

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[*] From the Transactions of the Royal Irish Academy, vol. xvii.; being § 2 of "Researches on the Action of Ammonia on the Chlorides and Oxides of Mercury." In L. and E. Phil. Mag., vol. viii. p. 495, we gave Dr. Kane's "Experiments on the Action of Ammonia on the Chlorides and Oxides of Mercury, and on the Composition of white Precipitate." The present article concludes his observations on these mercurial compounds.

of potassium, there is ammonia disengaged, and a brown powder formed. To this reaction I shall hereafter recur. In order to determine the composition of this yellow powder, the following experiments were made:—

A.—100 parts of corrosive sublimate were dissolved in water, and ammonia added in excess. The mass, in place of being filtered cold, was boiled until the light-white precipitate was changed into the clear yellow heavy powder; it was then filtered, and the quantity of product determined. The liquor and washings were acidulated by nitric acid, and precipitated by nitrate of silver, and the chloride abstracted from the sublimate thus determined; the liquor contained a very small trace of mercury. Several experiments were made on this plan, the results of which are exhibited in the following table:

100 parts of sublimate gave,

Exp.	Yellow powder.	Chlorine in liquor.
1	83·5	19·25
2	83·3	18·50
3	84·7	18·90
mean	83·83	18·89.

Now 100 of sublimate contain

Mercury	74·09
Chlorine	25·91.

Hence we see that there have been abstracted from the sublimate three-fourths of its chlorine; the remaining fourth, and all the mercury, existing in the yellow powder. We have therefore in 83·83 parts of it:

Mercury	74·09
Chlorine	71·02

Or in one hundred parts,

Mercury	88·381
Chlorine	8·374

B.—When white precipitate already prepared is boiled with water, there is obtained a similar yellow powder, and the supernatant liquor is found to contain only sal-ammoniac. As we know, within very strict limits, the composition of the white powder, we can make use of this reaction to illustrate the nature of the yellow product:

100 parts of white precipitate were boiled with water until completely converted into the yellow powder; the liquor, which was quite neutral, was acidulated; and the chlorine dissolved precipitated as chloride of silver, from which its quantity was obtained by calculation. The following table gives the results of experiments conducted in this manner:

100 of white precipitate gave

Exp.	Yellow powder.	Chlorine in liquor.
1	90·00	5·93
2	88·50	6·50
3	90·30	6·40
mean	89·60	6·29.

But 100 of white precipitate contain

Mercury .... 78·60
Chlorine .... 13·85

Therefore 89·60 of the yellow powder contain

Mercury .... 78·60
Chlorine .... 7·56

and 100 contain

Mercury .... 87·95
Chlorine .... 8·44.

C.—100 grains of white precipitate were boiled with water until completely decomposed; the resulting yellow powder weighed 91·15 grains. The liquor was cautiously evaporated to dryness, and gave 10·23 of sal-ammoniac, consisting of

Chlorine .... 6·76
Hydrogen .... ·19 } 10·23.
Ammonia .... 3·28

Therefore there are obtained, by this experiment, for the constituents of yellow powder,

Mercury .... 86·23
Chlorine .... 7·77
Ammonia .... 3·83.

D.—It has been already stated, that when this powder is heated, it is resolved into ammonia, azote, water, calomel, and quicksilver. Having found that, by performing this operation in a very small retort, the water and gases could be dissipated without any remarkable loss of the other constituents, I made some trials in this way to determine the amount of the chlorine and quicksilver. For this purpose a higher temperature is required than for the corresponding analysis of white precipitate, and the condensation of the mercurial vapour must be very carefully effected. In other respects the manipulation was the same, and the following table contains the results:

Exp.	Quantity of Material.	Sublimed Residue.	Sublimed Residue from 100 parts.
1	14·30	13·37	93·50
2	19·65	18·53	94·30
3	23·72	22·35	94·22
		Per cent. mean	94·01.

From this result we can easily calculate the quantities of chlorine and mercury the residue contains, for,

Let $m =$ the residue $= 94·01$
$x =$ the quantity of chlorine
$y =$ the quantity of mercury
$a =$ atomic weight of chlorine $= 35·42$
$b =$ atomic weight of mercury $= 202·8$.

This is (1.) $x = m - y$

and (2.) $\frac{x}{y} = \frac{a}{2b}$ by other processes.

Then $$\frac{m-y}{y} = \frac{a}{2b} \therefore 2bm = (a+2b)y$$

and $$y = \frac{2bm}{a+2b}.$$

We thus find 100 of yellow powder, to contain

Mercury	86·46
Chlorine	7·55

E.—105·28 grains of yellow powder were dissolved in muriatic acid, and the solution having been somewhat diluted, was decomposed by a current of sulphuretted hydrogen gas. The perfectly black sulphuret was collected on a weighed filter, and the liquor evaporated to dryness, and the residual sal-ammoniac weighed:

The filter and sulphuret	126·71
Filter	23·00

Sulphuret of mercury 103·71, consisting of

Sulphur	14·22
Mercury	89·49.

The sal-ammoniac weighed 12·86 grs. consisting of

Chlorine.....................	8·50
Hydrogen	·24
Ammonia	4·12.

Therefore, the yellow powder consisted of

in 105·28 parts,		in 100 parts,	
Mercury	89·49	Mercury	85·00
Ammonia	4·12	Ammonia	3·91.

Summing up these different results, we have

Process.	Mercury.	Chlorine.	Ammonia.
A	88·381	8·374	
B	87·95	8·44	
C	86·23	7·77	3·83
D	86·46	7·55	
E	85·00		3·91.

And taking the mean result of all, we get, for the composition of this yellow powder,

$$\left.\begin{array}{ll}\text{Mercury} \ldots\ldots\ldots\ldots & 86{\cdot}80 \\ \text{Chlorine} \ldots\ldots\ldots\ldots & 8{\cdot}03 \\ \text{Ammonia} \ldots\ldots\ldots\ldots & 3{\cdot}87 \\ \text{Oxygen and loss} \ldots\ldots & 1{\cdot}30\end{array}\right\} 100{\cdot}00.$$

In the processes, A and B, a small quantity of the yellow powder was lost, in consequence of its being not perfectly insoluble in water. This quantity, I have reason to believe, varied from one to two per cent. and by the means of calculation we employed, the mercury and chlorine constituents are given above what is correct in that proportion. As far as I can judge, by considering the circumstances of the experiments, I conceive the mean to be consequently too high; and I believe the analysis C. by itself, to approach closer to the truth. By it we have in 100 of the yellow powder,

$$\left.\begin{array}{ll}\text{Mercury} \ldots\ldots\ldots\ldots & 86{\cdot}23 \\ \text{Chlorine} \ldots\ldots\ldots\ldots & 7{\cdot}77 \\ \text{Ammonia} \ldots\ldots\ldots\ldots & 3{\cdot}83 \\ \text{Oxygen and loss} \ldots & 2{\cdot}17\end{array}\right\} 100{\cdot}00.$$

This yellow powder is generated evidently by the reaction of water on white precipitate, in which one-half of the chlorine and ammonia are converted into sal-ammoniac, a corresponding portion of the mercury being oxydized. I shall compare the results of this reaction with each of the formulæ for white precipitate that I have previously given.

$$2\left\{(2\,\mathrm{Ch}+\mathrm{Hg})+(2\,\mathrm{NH}^3+\ddot{\mathrm{H}}\mathrm{g})\right\}+2\,\dot{\mathrm{H}} =$$

$$\left\{(2\,\mathrm{Ch}+\mathrm{Hg})+(3\,\ddot{\mathrm{H}}\mathrm{g}+2\,\mathrm{NH}^3)\right\}+2\,\mathrm{Ch}\mathrm{NH}^4.$$

Here two atoms of white precipitate and two of water, mutually reacting, give two atoms of sal-ammoniac and one of the powder. On this idea, the yellow powder should consist of

$$\left.\begin{array}{ll}\text{Mercury} \ldots\ldots\ldots\ldots & 84{\cdot}12 \\ \text{Chlorine} \ldots\ldots\ldots\ldots & 7{\cdot}36 \\ \text{Ammonia} \ldots\ldots\ldots\ldots & 3{\cdot}56 \\ \text{Oxygen} \ldots\ldots\ldots\ldots & 4{\cdot}96\end{array}\right\} 100{\cdot}00$$

These numbers, with the exception of oxygen, fall below the lowest experimental result, and probabilities are therefore rather against this formula being true. Let us next try the result of reaction with that formula for white precipitate which does not include oxygen, and supposes the ammonia to exist as amidogene.

$$2\left\{(2\,\mathrm{Ch}+\mathrm{Hg})+(2\,\mathrm{NH}^2+\mathrm{Hg})\right\}+4\,\dot{\mathrm{H}} =$$

$$\left\{(2\,\mathrm{Ch}+\mathrm{Hg})+2\,\ddot{\mathrm{H}}\mathrm{g}+(2\,\mathrm{NH}^2+\mathrm{Hg})\right\}+2\,\mathrm{Ch}\mathrm{NH}^4.$$

There are here equally produced by the two atoms of white powder and two of water, two of sal-ammoniac, and one of the yellow powder, of which the composition should be

Mercury	85·72	100·00
Chlorine	7·48	
Amidogene	3·42	
Oxygen	3·38	

and it should yield in analysis 3·63 per cent. of ammonia.

The definite composition of this yellow powder is thus evident, and the decomposition by which it is formed perfectly explained. We see that all the results tend to show that in these bodies the ammonia is not united with oxide of mercury, but rather the metal with amidogene. The perfect demonstration of this principle, however, must be sought for in the other metals.

Of the Products of the Action of Alkalies in Excess on White Precipitate.

Grouvelle and other chemists have stated that by the action of an excess of alkali on a sublimate solution, there is produced the ammoniuret of mercury which was discovered by Fourcroy and examined by Guibourt, and to which I shall hereafter speedily recur; and even Dumas states, that "the same compound (the ammoniuret) is obtained by pouring ammonia into a solution of corrosive sublimate, and then adding caustic potash in excess." My anxiety to obtain pure ammoniuret of mercury, joined to the interest of the preceding investigations, led me to examine the nature of the products thus obtained; and the results, as correcting an error very generally fallen into, are worthy of being described.

When corrosive sublimate is decomposed by ammonia, the quantity of alkali in excess does not appear to interfere much with the reaction before described. If the liquors be cold, there is obtained white precipitate; and if it be boiled, the heavy yellowish powder is produced; the liquor retaining in the former, one-half, in the latter, three-fourths of the chlorine of the sublimate. Again, if white precipitate be boiled in water, rendered strongly alkaline by ammonia, we obtain the yellowish powder, and half the chlorine and half the ammonia of the precipitate are disengaged. Thus, water of ammonia acts on white precipitate only as water itself does, the nature of the reaction being the same in both instances. Again, when white precipitate was treated with potash for analysis, as in L. and E. Phil. Mag., vol. viii. p. 498, it has been seen that the ammonia disengaged was but one-half what it contained, the formation of the yellowish powder being the limit at which the decomposition stops. In these cases, however, the powder product is not so bright in colour as that produced by the action of mere water. It does not appear to be quite so pure, but in its properties it manifests complete identity.

Nevertheless, in order to leave no room for doubt upon the matter, I decomposed corrosive sublimate by a great excess of ammonia, added a strong solution of potash, and boiled for a considerable time. The yellowish white powder produced, was separated by the filter and washed until the liquors ceased to affect turmeric paper: dried carefully. It weighed from 100 of sublimate, 85 grains. When heated it gave out water, ammonia, and azote, and calomel with metallic mercury sublimed. When suddenly heated, it puffed up, more so than the pure yellow powders, which was probably the reason of its having been confounded with the ammoniuret which possesses a very slight detonating property.

To analyse it, 66·83 grains were dissolved in muriatic acid, and having been diluted were decomposed by a current of sulphuretted hydrogen gas. The black sulphuret was collected on a weighed filter, and having been carefully dried, weighed 67·70 grains, consisting of

Mercury	58·42
Sulphur	9·28.

The liquor evaporated, gave sal-ammoniac 6·58 grains, consisting of

Chlorine	4·35
Hydrogen	·12
Ammonia	2·11.

There were thus obtained from 66·83 of this powder,

$$\left.\begin{array}{ll} \text{Mercury} \;.\;.\;.\;.\;.\;.\;. & 58{\cdot}42 \\ \text{Chlorine} \;.\;.\;.\;.\;.\;.\;. & 4{\cdot}35 \\ \text{Ammonia} \;.\;.\;.\;.\;.\;.\;. & 2{\cdot}11 \\ \text{Oxygen and loss} \;.\;.\;. & 1{\cdot}95 \end{array}\right\} 66{\cdot}83.$$

But the yellow powder by water, should have given from the formula:

$$(2\,\text{Ch} + \text{Hg}) + 2\,\ddot{\text{Hg}} + (2\,\text{NH}^2 + \text{Hg})$$

$$\left.\begin{array}{ll} \text{Mercury} \;.\;.\;.\;.\;.\;. & 57{\cdot}30 \\ \text{Chlorine} \;.\;.\;.\;.\;.\;. & 5{\cdot}00 \\ \text{Amidogene} \;.\;.\;.\;.\;. & 2{\cdot}28 \\ \text{Oxygen} \;.\;.\;.\;.\;.\;.\;. & 2{\cdot}25 \end{array}\right\} 66{\cdot}83.$$

This result proves the identity of the effect in the two cases of the action of water and of an alkali.

Rose and Grouvelle have already shown that when dry ammonia is passed over melted sublimate, an atom of the former is absorbed by one of the latter, and a white mass formed. The history of its properties has been so well given in Rose's Memoir, that it is unnecessary for me to do more than mention my results as having been confirmatory of his. This compound is decomposed by water giving white precipitate and sal-alembroth, as may be at once seen:

$$4\,(2\,Cl + Hg) + 4\,NH^3 =$$
$$\left\{(2\,Cl+Hg)+(2\,NH^2+Hg)\right\} + 2\left\{(2\,Cl+Hg)+(Cl+NH^4)\right\}$$

[To be continued.]

LVII. *A Report of the Progress of Vegetable Physiology during the Year* 1836. *By* J. Meyen, *Professor of Botany in the University of Berlin.**

[Continued from p. 390.]

On the Combination, Structure, and Contents of the Cells of Plants.

We come now to the observations which were made known last year, on the combination of cells in the higher and lower orders of plants. Mohl † endeavours to establish the opinion that the tissue of plants is constituted not as a collection of contiguous cells grown together without any intermediate substance, but that there is present a homogeneous substance, an organic mucus, in which the cells are immersed, and by which they are connected with one another. This combining substance Mohl calls intercellular substance, and the discovery of it appears of such importance, that a learned botanist lately observed, that with it a new æra for vegetable physiology had arisen. Mohl ‡ had previously mentioned this notion in his observations on the structure of the pollen membranes, which Mirbel § opposed with very powerful arguments. In the above memoir, Mohl endeavours to weaken the arguments which Mirbel had advanced against his view on the combination of the cells of plants; and demonstrates a so-called intercellular substance, not only in the membranes of the pollen, but even in various families of Cryptogamia, as also in the tissue of the higher plants; but in what degree he

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* From Wiegmann's *Archiv für Naturgeschichte,* 1837, Part 3. Translated by Mr. Wm. Francis.

† On the Combination of the Cells of Plants with one another. Published as an Inaugural Dissertation in September 1835. It was then again printed, enlarged, under the title of: "Explanation and Defence of my view of the Structure of Vegetable Substance," with 2 plates. Tübingen 1830.

‡ *Vide* Meyen's Year's Report for 1834, p. 153.

§ *Ibid.*, 1835, p. 101.

has succeeded in doing this will in part be shown in the course of this report from the observations of other botanists.

Mohl finds this homogeneous substance most clearly in the Algæ between the cells, which are combined by it into a whole.

In the *Nostochineæ*, *Rivulariæ*, in *Protococcus*, *Palmella*, *Hydrurus*, *Oscillatoria*, *Scytonema*, &c. the more or less thick mucous or gelatinous substance is to be regarded as the analogue of the intercellular substance in the perfect plants. In the proper Confervæ the common mucous substance has disappeared, or only forms a very thin envelope over the fibres, so that these are lubricated and rendered slippery by it, but no longer combine into masses; instead of which they possess a homogeneous external tube. Of the true Confervæ the *Spirogyræ* of Link are those which possess the greatest mucous envelope, and in those, as also in other Confervæ, we can observe that this mucous substance becomes thicker with increasing age, and that in the young plants it is entirely wanting. According to this, the view which Mohl has advanced on the nature of intercellular substance can hardly be extended to this substance.

In the more compound Algæ this mucous matter, according to Mohl's observation, is not only disposed on the surface of the entire plant, but also between the individual cells, which had already been observed by Eysenhard and Agardh; and since this homogeneous substance entirely fills the intervening space of the cells, the intercellular passages are totally wanting in these vegetables. In the thallus of Ferns the intercellular substance forms a less remarkable constituent part than in the Algæ. In this case it is the cells of the outer layer becoming transparent in water, which are connected by this substance, so that here also no intercellular passages remain. In the more perfect plants it is not so easy to demonstrate the presence of intercellular substance, as in these not only do the cells lie in closer contact with each other, but since also intercellular passages run between their parenchymatous cells. Mohl however alleges that notwithstanding these difficulties, we may still succeed in many cases in meeting with this mass interfused between the cells, in greater or smaller quantities, even in perfect plants, so that there will be scarcely any plant in which we shall not be able plainly to demonstrate this substance in one organ or other. Mohl then enumerates a number of instances in which the intercellular substance is found in mosses, ferns, in the wood of the Coniferæ and of the Dicotyledones. The intercellular substance shows itself more plainly in the protended thick-sided cells which often occur in the bark of the stem, or under the epidermis in the petiole of the leaf, than between the woody cells. If we examine this cellular mass in the stem of *Sambucus nigra* in diagonal sections, the cellular cavities appear at first sight to be very irregularly distributed in a completely homogeneous glass-like transparent substance; by a more accurate examination it is however evident that this substance is not totally homogeneous, but that it separates into cellular membranes and into intercellular substance. The lines of demarcation are said to be very fine and easily overlooked.

Our views on this point are very different from those of Mohl. If we take very thin sections, and observe them with a magnifying power of 1000 or 1800, with achromatic glasses, no lines can be perceived distinguishing the exterior surface of the cellular membrane from the intercellular substance; but it can be plainly seen that there takes place a gradual transition from the substance of the membrane of the cell into the so-called intercellular substance. I will also mention an observation which most clearly proves that the intercellular substance of Mohl is no peculiar separately constituted substance, which is as it were diffused between the cells, but that it belongs to the cellular partitions, and is secreted by them when a more close connection of such cells is to take place. If, for instance, we observe in diagonal sections the solid cellular layers which clothe the leaf petiole of *Beta Cicla* (the red variety is best for this purpose), we find that the so-called intercellular substance occurs in great quantities between the cellular layers; we however can distinguish even with weaker magnifying powers that to each one of the surrounding cellular membranes belongs an appropriate portion of that intermediate substance. I could mention several other instances where the case is precisely similar; and consequently his view of the nature of the intercellular substance in vegetables would have to be altered. Mohl also applies his view of the intercellular substance to the epidermis of plants, as he considers the cuticula with its dependents as this substance in which the cells are sunk.

Subsequently to the appearance of this memoir of Mohl, Valentin * also published a series of observations in which this intercellular substance was demonstrated more or less evidently; and these examples will be multiplied by every observer. Valentin concludes from his observations that all intercellular substance is found only between ligneous forma-

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* On the Structure of Vegetable Membrane, especially in the secondary ligneous layers; in his *Repertorium für Anat. u. Physiol.*, vol. i. p. 96. Berlin, 1836.

tions; never, on the contrary, between simple sacs in any perceptible quantities; the cause of which lies in the very existence of the intercellular substance. Valentin's observations also show, that the intercellular substance does not exist at the beginning, but occurs first after the commencement of lignification, and must therefore certainly be distinguished from the substance which is found around and between the sacs of the lower Cryptogamia. We have however previously mentioned, that this substance is also wanting in germinating Confervæ. Since the intercellular substance, observes Valentin, appears first after the process of lignification*, it cannot be regarded as an organic mucus which holds the cells together. It is no less a secondary layer without the primitive sac than the ligneous lamellæ are within the same. It occurs only where a considerable number of ligneous lamellæ exist. If we may use an old technical expression, the intercellular substance occurs in all cases where thick-sided cells are closely connected with one another, few or no intercellular passages remaining.

Closely connected with this subject are the observations which have been made on the structure of cellular membrane. Mohl in the above-mentioned memoir has given an accurate description of the stripy structure of the partitions of the cells of the liber of *Nerium Oleander*, *Vinca minor*, and various other plants belonging to the families *Apocyneæ* and *Asclepiadeæ*. These cells show in a diagonal section, as also in a horizontal one, that their sides consist of a great number of superincumbent membranes. The cells of the liber of *Vinca* throw more light on the subject; Mohl describes them as broad, abruptly and greatly contracted at their ends, sides not very thick, and composed of several layers. In the wider parts the membrane was covered with spiral, steep, ascending lines, and so that a part of these lines wound round to the right, the other to the left, and in this manner the membrane was divided into small rhomboidal portions. Mohl previously supposes, that the lines are wound in the one layer to the right, in the other to the left, and that the layers which compose these cellular membranes are not homogeneous, but possess a fibrous texture.

"Are we now to deduce from this aspect of the tubes of the liber of the plants above cited," says Mohl, "the view already entertained by Grew, that the cellular membrane is woven together of fibres? I do not think so. As far as we can observe of these extremely delicate formations, discernible only with good

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* The author, differing from other phytotomists, understands by lignification nothing more than the thickening of the sides of the cells by the deposition of new layers.

instruments and under favourable light, the substance of these apparent fibres appears to be exactly the same as that which fills out the intermediate spaces; and this fibrous appearance seems to indicate not the existence of real separate fibres, but rather a small difference in the thickness of the cellular membrane: perhaps a different arrangement of the molecules at various points, perhaps a small difference in the thickness of the membrane causes a different refraction of light, precisely in the same way as fibres are visible in badly melted glass." Mohl also throws out the opinion that such a texture of the cellular membrane is very common, as several observations have seemed to prove to him.

Valentin (*l. c.* p. 89) repeated these observations of Mohl, and has completed them in many respects. He observed perfectly well in the cells of the liber of *Nerium odorum*, that the diagonal or rather horizontal stripes which these cells exhibit are found entirely outside, while the spirals which cross one another are found in different lamellæ bent over each other. And in each partition of the cells these spirals always take one and the same direction; and for that reason they must cross in opposite partitions. Valentin observes this structure of the tubes of the liber and woody cells in various other cases partly known, and partly not yet noticed, and arrives at the conclusion that they are all without exception lignifications, and that their partitions are never those of the primary cellular sac alone, but that they are also covered with ligneous lamellæ. And, as Valentin had not yet found these spiral lines in the more simple cells and septa (in which they however occur quite as beautiful, as I can prove in several cases,) he thinks he is able to consider these as the consequence of the process of lignification; the history of their development even is said to remove all doubt on this subject.

Valentin at the same time gives a history of the formation of the spiral stripes, which are certainly very difficult to observe in their formation. "In the centre of the tube of the liber a very fine granular substance is perceived, the granules of which possess for the most part a transversal arrangement. The corpuscles of this substance at first do not allow of our observing any definite arrangement. Subsequently they form diagonal lines, then spiral lines, in which, however, at the beginning it is still possible to distinguish the individual corpuscules, and which at last run on in an uninterrupted continuity."

Link* has examined the seed of the *Casuarinæ*, in refer-

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* *Philos. Bot.*, i. p. 186.

ence to the cells which are situated under the testa, and are regarded as a layer of spiral tubes which are easily unrolled. Link found under these another layer or membrane consisting of long parenchymatous cells, which are closed at one end, and contain fibres just beginning to be evident; but at the other end are spiral fibres which became true spiral vessels. On this ground Link has advanced the opinion that the cellular membrane changes with age into spiral fibres; this is represented in Plate III. fig. 3. of his work. I have on the contrary, in my recent work on Vegetable Physiology, endeavoured to confirm the opinion that the cellular membrane is composed of fibres having a spiral course.

In a second memoir by Valentin* the structure of the cellular membrane in reference to its lamellar structure, and in regard to the form of the dots (*Tüpfel*) is more closely examined.

The thickening of the cellular membrane by the deposition of new layers is called by Valentin the process of lignification, and in the first stage only of development of this process do the first deposited lamellæ lie close on the entire surface of the primary sac partition. Subsequently, however, at the end of the individual development of the porous cells and vessels, a circular gap is formed around the exterior limit of the porous canal (*Tüpfelkanal* dotted duct) between the first superposed layers of lignification, and the primary sac partition whose exterior periphery runs in a direction concentric with that of the porous canal, and which gradually becomes smaller from this point up to its circumference, until both membranes become once more fixed close to each other. The porous canal as well as the gap above cited constantly contained, like the interior of these ligneous cells or vessels, an aëriform substance. Valentin then gives a more accurate description of the great disks with double circles which the cells of the *Coniferæ* exhibit, and accompanies his explanation with some figures, from which it is evident that some error has occurred in these observations; for it is very easy to perceive in these formations a coincidence with the structure of other dots; while Valentin's representation is quite at variance with this. For according to it, next to the external layer of the cellular membrane is situated a great gap, which is said gradually to contract into a fine passage representing a dot, and to merge into the cavity of the cell; while, according to the observations of other botanists,

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* On the various forms of the porous canal in the porous cells and vessels.—Vide his *Repert., &c.*, p. 78—87.

the gap originates between the partitions of contiguous cells, and indeed from a local separation of the membranes, and the real dots, here showing themselves as the small and interior circles, which consist of a cavity on the interior partition of the cellular membrane raised towards the inner side.

Valentin himself confesses that the porous opening exhibits quite different forms, not only in different plants, but also at times in various parts of the same plant; but nevertheless he considers it necessary to give various denominations to the different parts. Thus he calls the space which characterises the formation of the gaps, and which is continued in the true porous opening, the *gap-funnel*; yet in the *Coniferæ*, in which Valentin has figured this gap-funnel so very great and so plainly, this space is not present. The opposite end through which the termination of the porous opening enters the cavity of the cell, he calls the *entrance-funnel*, and the more cylindrical part situated between these, the middle part.

He afterwards draws attention to the various forms of these individual parts of the dots in various plants. I have however not been able to find these forms so constant as they are described. At all events it is highly gratifying that Valentin has entered so minutely into details on this point; there is much yet to be observed with regard to this, especially in the dots of the spiral tubes. Valentin has also confirmed the fact that the position of the dots on the sides of the cells is a spiral one, a circumstance evidently connected, as I have previously shown, with the formation of the cellular membrane from spiral fibres; as the dots always occur between the convolutions of those fibres running in a spiral direction. The dotted ducts do not, according to Valentin, stand perpendicular on the external layer of the cellular partition (which is called the primary sac partition) but in a rather slanting direction from the interior to the exterior, towards the cellular partition.

Dr. Hope * read on the 21st of March 1836, a paper on the colours of plants, before the Royal Society of Edinburgh, which in its results bears the greatest similarity to the beautiful labours of Marquart which had appeared from eight to nine months previously. Hope in like manner shows that two different colouring matters occur in plants, one of which forms with acids the red colours and is called on that account *Erythrogene*, while the other gives with alkalis the yellow combinations of colours and is named *Xanthogene*. These two substances evidently represent the *Anthokyan* and *Anthoxanthin*

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* *L'Institut*, Feb. 15, 1837.

of Marquart. The observations of the latter on this subject are however much more accurate than those of Dr. Hope; Marquart showed, for instance, that the *Xanthogene* is produced from a blue extractive substance converted to red by an acid, &c. It also appears from the statement of Dr. Hope that he made no use at all of the microscope, which however was quite necessary. One consequence of this is the retaining of the term *Chromule*, which De Candolle had proposed for the colouring matter of plants; against the adoption of which however many reasons exist. Dr. Hope states that he found that the *Xanthogene* occurs independently of the *Chlorophyll* in all green leaves; that in white flowers (about thirty different ones were examined) *Xanthogene* only is contained, exactly as in the yellow flowers, where also no *Erythrogene* occurs. The explanations of this are to be found, I think, in the experiments of Marquart. Red flowers on the contrary exhibited *Erythrogene*, as also *Xanthogene*, as did blue, purple, and orange flowers, &c.

I believe, judging from my own observations, that the work of Marquart deserves the preference in every respect; also that his names, on account of priority even, should be retained. Marquart's memoir is not mentioned, although it might have been well known in England*.

Hünefeld has published † a very interesting paper on the blue colours of the flowers of plants; the subject is however treated more in a chemical view; therefore we can only call attention to it. Hünefeld ‡ also proposes the use of water acidulated with sulphuric acid, as a means of facilitating the microscopical examination of the coloured parts of vegetables.

F. Schulze § has made some observations on the *Amylum* of the potato, and has confirmed some of the most essential points of the results which Fritzsche obtained in his experiments on this subject. As such, I will mention the following: The composition of the *Amylum* globule from concentric layers deposited round a certain point, called the nucleus, and the changes which the *Amylum* globule undergoes in consequence of growth; its solution from the interior as well as at the surface. Schulze directs attention to the fact, that we are ac-

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* [It is to be regretted that the little encouragement given to the publication of notices of foreign discoveries in this country gives rise to frequent remarks of this kind.—Edit.]

† Additions to the chemistry of vegetable colours.—Erdmann and Schweigger-Seidel's *Journ. für Prakt. Chem.*, ix. p. 217—238. ‡ *Ibid.*, p. 238.

‡ On the Metamorphosis of Amylum. Poggendorff's *Ann.*, vol. xxxix. p. 489—493.

quainted with no substance by which we can artificially dissolve *Amylum* from without; and such a one must be produced in the cells of the potato in their growth.

Hartig's views[*] that "in the evergreen fir trees the digesting apparatus itself (the leaves are understood by this) is carried from one year to the other, and on the contrary, in the summer-green plants, the substance for the development," have met with commendation from various quarters; although repeated observations show that the data on which that view was founded are by no means correct. Wiegmann, sen.[†] separated the *Amylum* from the wood of various trees; to which I will add the remark, that the occurrence of *Amylum* in woods is rather an old observation. Wiegmann found, that the powder in the end of the stem and in the root of *Buxus sempervirens* was not coloured blue by iodine. Wiegmann was not able to examine the fir trees, but is fully persuaded that they would be quite destitute of starch; evidently, however, only because the hypothesis of Hartig is founded on this. I find on the contrary in young fir trees, in *Pinus* as well as in *Abies* and in *Larix* proportionally quite as much *Amylum* as in many deciduous trees.

Creuzburg[‡] has published some microscopical experiments on the globules of starch before and after vinous fermentation, the results of which are represented in a plate drawn by Corda.

Various discoveries have been published during last year on the occurrence of crystals in plants. Link very justly observes[§], that crystals in plants might be compared with stones and concretions in animals. They are so frequent that it seems unimportant to mention all such cases. Link also confirms the observation that the spicular crystals seem to occur more in the Monocotyledons, the conglomerate ones on the contrary more in the Dicotyledons. Link however observes, that these crystals occur not only in the cells, but also between them; an opinion to which I am able to oppose at present, direct observations. In the tissue of Agaves and of *Pontederia cordata* I thought that I had observed[||] with certainty the occurrence of some large crystals between the cells; but on separating these crystals by macerating the tissue and by using a higher magnifying power, I recently succeeded in ob-

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[*] Meyen's Year's Report for 1835, p. 37.

[†] *Flora* 1836, p. 24, &c.

[‡] Additional Notices on the vinous Fermentation of amylaceous Substances. Erdmann and Schweigger-Seidel's *Journ. f. Prakt. Chem.*, ix. p. 299, &c.

[§] *Element.*, p. 137.

[||] Meyen's Year's Report for 1835, p. 131.

serving that in these cases also the crystals, occurring singly, are surrounded with a cellular membrane.

Turpin* has made an interesting discovery with reference to the occurrence of acicular crystals in the tissue of the *Aroïdeæ*. It is true that long ago it was known that these crystals, as well as entire druses of small crystals, occurred in the cells of the *Aroïdeæ*; but in the leaves of *Caladium esculentum*, these spicular or needle-form crystals, which here, as in all other cases, always occur in the form of fascicles, are not only very long and of extreme tenuity, but the cells also in which they are found differ in many respects from the other cells of the leaves of this plant. These crystalliferous cells are those which Turpin has named *Biforines*; and, for reasons which will now be stated, the position of these long crystalliferous cells in the leaves of the plant above-mentioned was not perceived by Turpin; and it is this very circumstance which, in some respects, assists in explaining this discovery which Turpin has made with regard to them. These cells are several times larger than the surrounding ones of the diachyma of the leaves of *Caladium*, which are filled with green-coloured cellular sap-globules; and they are so placed that their middle parts only lie between the cells of the partitions which separate from each other the air-passages, with which these leaves immediately below the epidermis are filled; they protrude, therefore, with one end into one air-cavity, with the other end into the neighbouring air-cavity. The membrane forming these cells is considerably thicker than the surrounding green cells of diachyma; they also exhibit a brownish yellow colouring. If we now immerse these cells with their bundles of crystals in water, most of them open at both ends, and the crystals gradually more or less rapidly come out at the apertures, either through one aperture, generally, however, through both. Turpin has figured these apertures of the cells with excessive regularity, so that one would imagine that he perceived some entirely peculiar formation in these cells; however, even with the highest magnifying powers, I have never been able to observe these apertures trimmed thus regular, and, as it were, edged with broad borders; but the drawing which Turpin has given, (fig. 4. pl. iv.), as regards the structure of the ends of these cells, before their bursting, I find to be drawn quite after nature. The cause of the bursting of these crystalliferous cells exists in the hygrometrical condition of the substance which occurs with the crystals in these cells; it is a

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* *Observations sur les Biforines, organes nouveaux situés entre les vésicules du tissu cellulaire des feuilles dans un certain nombre d'espèces végétales appartenant à la famille des Aroidées.—Ann. des Sciences, Nat.* 1836. ii. p. 4, 27.

yellowish gum, which in the beginning fills the entire cell; subsequently however, it is generally deposited only round the bundle of crystals, giving it a yellow colour. I could observe nothing, however, of an intestinal canal-like organ, which these crystals are said to contain*, and which is extended at full length, as it were from aperture to aperture, in the interior of these cells; but they had the appearance common to other cells containing such bundles of acicular crystals; only in this case thicker and yellow partitions occur where these cells protrude into the air-cavities. Besides the yellow gummy substance, there often occurs a greater or less number of very small molecules in these cells, arranged laterally from the bundle of crystals; and these also pass through the apertures of the cell when it bursts by the entrance of water. This bursting of these cells undoubtedly belongs to the most interesting observations for which science is indebted to Turpin; but to load these cells with peculiar names would not be allowed on a general consideration of the subject. In the diagonal sides of the air passages in *Pontederia cordata*, the occurrence of cells with spicular crystals is quite of the same kind as those in the leaves of the *Aroïdeæ*; and we there also find similar single cells, containing a yellowish gum-resinous substance; and indeed sometimes with, but in general without crystals†.

We are also indebted to Unger for an extended notice‡ on the occurrence of carbonate of lime upon the surface of Saxifrage leaves. It has already been known for a long series of years, that the gray and white efflorescence which occurs on the upper surface of the leaves of various species of *Saxifraga* consists of carbonate of lime; this calcareous covering is especially found in great quantities on those very species of this genus the leaves of which have at their borders small basin-like depressions, as, for instance, *Saxifraga Aizoon*, *S. cæsia*, *intacta*, *oppositifolia*, &c. Unger explains this appearance of lime on the leaves of the *Saxifragæ* as an excretion; and those cavities which are filled with this excretion are regarded as the excretory organs. "The epidermis of the leaves," says Unger, "which otherwise consists of very thick-sided cells with stripes and elevated points, becomes more delicate at the place where they cover the secreting cavities; and the cellular tissue situated under these, a continuation of the fasciculi of vessels (?), is also rather extended in length, and composed of smaller cells which are never filled with vesicles of Chloro-

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* [.... "*einem darmartigen Organe, welches die Krystalle enthalten*," &c.]

† *Vide* the figures on this subject in Meyen's *Phytotomie*, tab. v.

‡ On the influence of the soil on the diffusion of Plants, &c. Vienna, 1836. p. 179.

phyll. The carbonate of lime is secreted through these cavities in greater quantities the richer the soil is in lime. We also, however, find the leaves of the above-mentioned species of *Saxifragæ* very thickly covered with lime, when they have become old, in a soil very rich in humus. It may also frequently be observed, that many other places on the upper surface of the leaves of those plants, besides the cavities, are covered with a thin crust of chalk; that the deposition therefore of this calcareous substance does not take place through the cavities only. Unger, it is true, says, "We should in this case be mistaken if we were to consider the calcareous excretion as a product of the entire epidermis. I myself, however, think that this is the case; and it is easily observable on many garden plants of this tribe. These excretory organs on the upper side of the leaves, are said to be represented by an immense number of pores on the under surface, as if the increased process of secretion on the one side had produced an antagonistic process on the other side, but different in quality." In fact, this deposit of lime upon the leaves of Saxifrages is an entirely peculiar phænomenon, and can be connected with a few others only; it cannot even be viewed as parallel with the incrustation of the *Charæ*, for in these the lime appears to be precipitated from the surrounding water, as the carbonic acid, which held it in solution, is imbibed by the plant. In the Saxifrages only an exuding of the calcareous fluid seems to take place, and this is strongest in those cavities where the cellular tissue is very delicate: the phænomenon may be classed with the deposition of lime in the air-cavities of the leaves of *Lathræa*, and with the occurrence of crystalline druses on the sides of the air-ducts in *Myriophyllum.* We also often find in other plants a secretion of a salt which is contained in the soil on the surface of the leaves, &c.*

[To be continued.]

LVIII. *On the Action of Cold Air in maintaining Heat. By* ROBERT ADDAMS, *Esq., Lecturer on Natural Philosophy, &c.*

To Richard Phillips, Esq.

DEAR SIR,

IN the last Number of the Philosophical Magazine, p. 407, there is an account of the "Action of Cold Air in *maintaining* Heat." I conclude, from the initials R. P. being annexed, that you are the author of that communication; I therefore suppose you will be the more immediately interested in

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* Who first called attention to the occurrence of lime, on the leaves of Saxifrages?

knowing what I have seen and done in regard to the heating effects of currents of cold air on iron; and consequently I beg permission to acquaint you with the particulars; and if you think they are of sufficient interest to occupy a place in the forthcoming Number of the Philosophical Magazine, perhaps you will oblige me by forwarding the same for publication.

I was at Sheffield in last December, and then a Mr. Linley, bellows-maker of that town, showed me the following curious xperiments: first, a rod of iron, about an inch in diameter, was heated at one end in a forge fire, up to a full white heat, then quickly withdrawn from the fire and exposed to a strong blast of cold air from a forge bellows; the iron immediately became so hot as to fuse, and the liquefied matter was blown off and burnt in the air with the scintillating appearance of iron wire burning in oxygen gas; and so continued to melt until a pound or more of the metal had been thus wasted.

Another mode of producing the same action consisted in heating a rod of iron, as before; but instead of a blast of air, it was tied to a cord, and by it whirled round in a vertical plane; thus by passing swiftly through the cold air it melted, and was thrown off in beautiful scintillations, appearing as luminous tangents to the circle in which the bar was moved.

I have since applied a heated bar of iron to the periphery of a revolving wheel, and by an including tin hoop or guard, it is thus made an interesting class experiment.

The cause of this augmentation of temperature is, I conceive, referable to the oxidation of the metal, which takes place freely under the conditions of the experiments here recorded. Then, as is well known, the formation of the oxide is accompanied with a great development of heat; and the cases before us are striking examples of the heating influence by chemical action predominating over the cooling effect of the air conjoined with the radiating force.

The success of these experiments chiefly depends upon having the iron at first of a sufficiently high temperature, and upon the velocity of the air from the bellows, or otherwise the velocity of the iron through the air. For the iron at a white heat is greedy of oxygen, which the air solidifies. Then the oxide thus formed requires to be blown or whirled off, in order that fresh surfaces of the *metal* may be exposed to the air.

When the blast is employed, we see the oxide fusing, and deep channels scooped out in the bar by the rushing air on that side where the current is directed.

I am, dear Sir, yours truly,
ROBERT ADDAMS.

Kensington, Oct. 16th, 1837.

LIX. *Notice of New Discoveries of Ehrenberg's respecting the* Bacillariæ.[*] *By* M. Wiegmann.

THE readers of this Journal will have seen from Meyen's Report on Botany, (Phil. Mag., present volume, p. 385—390.) that the microscopical animal forms of this family continue yet to be a subject of dispute, inasmuch as they are referred by several botanists to the vegetable kingdom. Meyen is also of this opinion, which seemed to be supported by the circumstance that these creatures had hitherto not been observed to take in any coloured nutritive substance, like the other Infusoria. Recently, however, Ehrenberg has succeeded in feeding various species belonging to the genera *Navicula*, *Gomphonema*, *Arthrodesmus* (*Scenodesmus* Meyen), *Closterium acerosum*, on which subject he read a paper, at the same time exhibiting specimens in proof of this, before the Society of Naturforschende Freunde at Berlin. Vesicular ventral sacs, perfectly similar to those of the other polygastrical Infusoria, were apparent. "I could count," says Wiegmann, "with the greatest distinctness from six to seven ventral sacs completely filled with blue colouring matter, in the clear middle part of a *Navicula gracilis* exhibited by Ehrenberg." With this discovery, therefore, the most perfect proof has been afforded of their coincidence with the Polygastrica, which, however, the peculiar gliding motion[†] which was perceptible in many of them had before sufficiently indicated; which motion may be plainly recognised in many of them, and cannot be compared either with the lively motions of the sporules of Algæ, or with the motions of the Oscillatoria. The foot-like papillæ which these animalcules protrude out of various apertures, and draw back, may also be remarked in many of them: for instance, in *Navicula* very easily, when the water is rather thick; we can also perceive with the greatest facility the above-mentioned apertures in the empty shields[‡] of the fossil Infusoria.

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[*] From Wiegmann's *Archiv*, 1837, part iv., p. 377. Translated by Mr. W. Francis.

[†] [I have also lately noticed, in a few species belonging to this family, the peculiar papillæ by which they creep along. I may here state that I have often seen various species of *Bacillaria* and *Navicula* not only push one another out of the field, but have remarked that they make room for each other, which certainly cannot be attributed to electricity, as Morren supposes, (Phil. Mag., p. 389.)—W. F.]

[‡] See Ehrenberg's paper in the Scientific Memoirs, vol. i. p. 405.

LX. *Meteorological Observations taken at Bermuda, in July, August, and September,* 1836; *and on Sept.* 21*st,* 1836, *in accordance with the suggestions of Sir John Herschel*[*].

Place of observation sixty feet above the level of the sea.

JULY. BAROMETER.

Hours of Observation.	Inches.	No. of Observations.	
8 A.M.	30·149	26	Mean height for the month, taken by collecting those of the 8 A.M. and 9 A.M., with those of the 3 and 4 P.M. = 30·1615. Corrected by capillary action, capacity, and reduced from 77½° to zero.
9 —	30·165	28	
Noon	30·135	6	
2 P.M.	30·164	11	
3 —	30·182	18	
4 —	30·151	22	
9 —	30·183	24	

Greatest, 30·376 inch. Lowest height 29·620 inch. Oscillation, ·756.

Mem. On the 25th the barometer was at 30·376, whence to 31st it gradually fell; at 1½ A.M. of this day it descended to 29·620. On the 30th and 31st a very heavy storm with rain.

Mean temperature of the month, 75·79°. Greatest heat, 80°; least 67°. Mean temperature of 30th and 31st, 79°. Maximum taken from the shade, free from current and reflection; minimum from a register thermometer out of door.

Mean dew point, 74°.

Windy from the western side of the compass twenty-four days, of which fifteen were S.W.—Wind from the 30th S.E., changing from the 31st to W.S.W.

Greatest heat from 3 to 4 p. m.—Blackened bulb exposed to the sun, mean of six clear days, taken at the greatest heat, 96°.

Bulb lightly covered with calcareous soil, 138°.

Rain in seven days. Lightning eight days, mostly distant and in the east. Thunder less frequent.

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[*] Communicated by Dr. Dalton, by whom they have been arranged from the original observations of Lieut.-Col. Emmett, R.E. We have since received observations for the entire year from July 1, 1836, to June 30, 1837, from Lieut.-Col. Emmett, which we shall insert in our next; but we retain the present article on account of its having been prepared by Dr. Dalton.

AUGUST. BAROMETER.

Hours of Observation.	Inches.	No. of Observations.		
8 A.M.	30·126	29	Mean height taken as before	30·119
9 —	30·131	28	Corrected as before, and reduced for 77¾° ...	30·042
Noon.	30·159	14		
2 P.M.	30·108	10		
3 —	30·112	20		
4 —	30·109	20		
9 —	30·135	25		

Greatest height, 30·388; Least, 29·860. Oscillation, ·478.

On the 18th, storm with rain. S.W. wind. Lightning. Mean temperature of the month, 76°·65. Greatest heat was on the 1st at 4 p.m., 80°·4; least heat, night of 13th and 14th, 70°.

Mean dew point, 72°·5; many observations at 2 p.m. Temperature of the sea, 82°.

Rain on twenty-two days; quantity, 9·24 inches. Lightning, eleven days, nine of which in the east. Thunder, four times.

Winds, twenty-two on the western side of the compass, thirteen of which were S.W.

Morning of the 21st very stormy, but the barometer did not fall below 30·114. Dew point at 9 a.m., 75°. Temperature, 79°.

SEPTEMBER. BAROMETER.

Hours of Observation.	Inches.	No. of Observations.		
8 A.M.	30·154	27	Mean height taken as before	30·144
9 —	30·166	27	Corrected as before, and reduced to zero......	30·070
Noon.	30·150	15	Greatest height......... (South-east.)	30·316
2 P.M.	30·148	18	Least .... (Strong N. East.)	29 936
3 —	30·129	18		
4 —	30·128	21		
9 —	30·154	23		
Mid.	30·164	10	Oscillation	·380

Mean temperature of the month, 76°·75. Greatest heat, 81°; least heat in two nights, 71°. Mean dew point, 69¼°.

Winds, twenty-one days from the eastern side of the compass, and but seven from the western.

Rain on five days; quantity, ·91 cubic inch.

Lightning six times; twice only with thunder.

Temperature of the sea, 80½°, and towards the close of the month, 78½°.

Summary of the Observations of the Quarter ending the 30th of September.

Barometer.	8 A.M.	9	Noon.	2 P.M.	3 P.M.	4 P.M.	9 P.M.	Midnight.
No. of Obs.	82	83	35	39	56	63	72	17
July. Aug. Sept.	30·165 30·126 30·154	30·165 30·131 30·166	30·135 30·159 30·150	30·164 30·108 30·141	30·180 30·112 30·129	30·151 30·109 30·128	30·183 30·135 30·154	30·117 30·104 30·161
Mean	30·143	30·154	30·148	30·138	30·141	30·129	30·157	30·127

Mean height corrected { 30·090, 30·042, 30·070 } 30·067 at sixty feet above the sea.

Greatest height	30·388
Least	29·620
Greatest Oscillation . . .	·768

Mean temperature, 76°·73, about 2° lower than 1835 according to reports. Rain in thirty-four days; quantity in August and September, 10·15 inches.

Windy fifty-three days from the western side of the compass. Greatest heat in July and part of August, about 4 p.m.; afterwards somewhat below that time.

Dew point generally highest about 3 p.m. to 4.

Barometer used, Neuman's portable, with iron cistern; the attached thermometer being in the mercury.

Thermometer made also by Neuman; one brought as a standard. Dew point taken by using a cooling mixture in a copper gilt cup. Winds taken at sunrise, noon, and sunset, from the signal stations; these very variable, changing often from 9 to 11 a.m.

Observations on the 21*st of September*, 1836, *with reference to the suggestion of Sir John Herschel.*

	Bar.	Attached Therm.	Unattached Therm.	Wet Bulb.	Dew Point.
20th Sept.		°	°		
18 hour	30·194	76·5	76·5		
20 —	30·196	idem	77	72½	
21 —	30·200	idem	77·5		
22 —	30·208	77	78½	73½	
Noon	30·206	77·5	79·5	id.	69°

	Bar.	Attached Therm.	Unattached Therm.	Wet Bulb.	Dew Point.
21st Sept.					
2 P.M.	30·182	78	80·5	74½	70°
4 —	30·174	id.	79·75	73½	
6 —	30·174	id.	77·71	72½	67½°
8 —	30·196	id.	77	72	
9 —	30·194	id.	76·75	72	
Midn.	30·184	77·5	70·25	66?	
None made until 6 A.M. (18), probably little variation.					
18 hours	30·182	77·5	76	71	
20 —	30·194	id.	77·5	73	
22 —	30·204	78	79·5	74	
Noon	30·194	78¼	80¼	id.	
22nd Sept.					
2 —	30·162	79	81	74¾	
4 —	30·150	79½	80·5	74½	
6 —	30·158	79	78	72½	

Wind 20th, 18 hours,	North, light, and clear.	Passing and other clouds, cirrhi and cumuli.
Ditto Noon,	North-west.	
Wind 21st, 6 hours,	Ditto, bright and clear.	
Ditto 18 —	North, ditto ditto.	
Ditto Noon,	North-west, hot sun.	
Wind 22nd, 6 hours,	North-east, bright and clear.	

No correction made for barometers. Heat of white bulb, 89°. Temperature of the sea, 79°. Temperature of air over the sea, 78°[*].

Aug. 9th, 1837.

LXI. *On the Palo de Vaca or Cow Tree of South America.*

By Edward Solly, Jun., *Esq.*

I AM indebted to the kindness of Dr. Lindley for specimens of the bark, sap, &c., of the *Galactodendrum utile*, or Cow tree of Humboldt, accompanied by a highly interesting letter from Sir R. K. Porter to the Hon. W. F. Strangways, and communicated by him to Dr. Lindley; from which the following are extracts.

Dated "*Caracas, June 8th*, 1837.

"Towards the close of last month, I started from hence across the mountains, in a north-west direction towards the coast; and a most fatiguing and not a little dangerous journey we had; for journeying out of the beaten paths (for good

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[*] An abstract of Lieutenant Nelom's observations on the geological constitution of Bermuda will be found in Lond. and Edinb. Phil. Mag., vol. v. p. 222; and some remarks on the same subject by Mr. Greenough, in vol. vii. p. 147.—Edit.

roads we have none), it becomes break-neck work. Fifty miles of travel brought us to an elevated valley called Catorori, about a couple of miles from a mountain-perched respectable town called Cariacco; at a small sugar estate in the above-named valley we halted, as from it some four miles higher up in these elevated regions, grew the object of our search. On inquiry we learnt that it would not be possible to go there otherwise than on foot; therefore at six the next morning we started, accompanied by a few Indians and coloured peasants. I need not trouble you with the difficulties we encountered ere we had achieved our long league of fatiguing ascent through a dense forest, obliged at almost every step to have the way hewn out for us, so thick and interlaced were the branches, jungle, and pendent bejucos. A couple of hours' toil placed us at the foot of one of the trees in question, which very far exceeded any idea I could have formed of it from all the descriptions given to me, or even from that of Humboldt. This marvellously colossal vegetable cow stood surrounded by thousands of other trees of different characters, few of them less wonderfully stupendous than itself. At about five feet from its roots it measured in circumference somewhat more than twenty feet. Its trunk rose from its enrooted base most majestically straight for full sixty feet (gradually decreasing in thickness), clear of the slightest interruption, either of leaf or branch. At this vast elevation the huge and powerful branches spread forth on all sides to an extent of perhaps twenty-five feet from the centre stem; thickly and luxuriantly clothed with immense leaves of a brilliant though sombre green, not unlike in colour, polish, and shape, to the laurel, but somewhat more pointed, each being from twelve to sixteen inches in length, and from three to four in width. This magnificent verdant portion of the tree I should estimate to be not less than forty additional feet, hence the whole taken together amounted to no less than 100 feet; but others a few hundred yards from us I found even exceeded this, both in height and thickness, by many feet.

"As soon as an arrow-shaped incision was made deep in the bark (even to the wood itself) the snowy stream burst forth in a most extraordinary manner; so unceasing was the current, that within a quarter of an hour we filled one bottle, and the like was effected in the same time at another tree. In colour and consistence at the time it was drawn, it differed in no degree from that of the cow; in taste, not less sweet and palatable: however, it left on the tongue a slight bitterness, and on the lips a disagreeable clamminess. * * *

"The bark is a little roughish, and in general hue of a pale yellowish olive green; beneath the outer skin is a crust of more than an inch and a half in thickness, of a deep chestnut colour, wherein seemed to be contained the lacteal fluid, for on detaching it from the body of the tree, the milk oozed through thousands of pores from the curved surface that had embraced the trunk. The wood itself is white, close-grained, and hard, resembling in every way the box wood of Europe.

"I could not learn the precise period when the *Palo de Vaca* flowers (if ever) or bears its fruit. Plants, (and many are growing all about the trees,) will not live by even transporting to Caracas. The temperature of the air at 8 o'clock a.m. whilst at the foot of the tree, was 70° of Fahrenheit's scale*."

The milk as I received it was evidently rather altered by time; it was of the colour and consistence of very rich cream, having a sour and rather unpleasant odour, and a nauseous and acescent taste, accompanied by a somewhat gritty feel on the tongue. When left undisturbed for some time in a close bottle, a slight separation took place, a small quantity of a pale yellow fluid sinking to the bottom. The specific gravity of the sap was 1·085 at 60° Fahrenheit.

Exposed to the air, the sap gradually contracted from the evaporation of water, &c., and finally left a grey, transparent, and very viscid mass. When a portion of the sap was poured into water, it did not spontaneously mix, but fell to the bottom as a bulky white precipitate; on agitation, however, it easily mixed with the water, though after some hours' standing a slight coagulum rose to the top. When boiled, the mixture was immediately coagulated; this was not effected either by acids or alkalies.

When the sap was distilled at a low heat, water holding in solution acetic acid passed over, and a grey transparent mass remained, similar to that left by the evaporation of the sap exposed to the air; when the heat was cautiously raised, the mass fused, became of a pale yellow colour resembling oil, and contained numerous dark flocks of an infusible substance floating in it; 100 grains of the sap yielded on the average 38·0 grains of solid residue. In order to determine the nature of this residue, it was repeatedly digested in cold æther,

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* This tree was first described by Alexander Humboldt in his Relat. Hist., t. ii. p. 106, 130. See also *Ann. de Chim. et de Phys.* t. vii. p. 182. It received the name Galactodendrum from Kunth, who imagined it to belong to a new genus; *Synopsis Plantarum*, t. iv. p. 198. I am only aware of two detailed chemical examinations of the sap, one by Boussingault and Rivero, *Ann. de Chim. et de Phys.*, t. xxiii. p. 219; the other by Dr. T. Thomson, Trans. of the Royal Society of Edin., 1829.—E. S. [See Lond. and Edinb. Phil. Mag., vol. vii., p. 501.—Edit.]

which, from previous experiments, I had ascertained would dissolve the waxy principle, or galactin. By this process 30·57 grains of galactin were dissolved, leaving a brown powder weighing 7·43 grains.

Cold water was poured on this residue, and dissolved 4·37 grains, yielding a solution precipitated by alcohol, becoming viscid when evaporated, and having hardly any taste; what little it had was derived from traces of saline matters: it was evidently gummy matter coloured slightly by extractive. The residue, weighing 3·06 grains, was boiled in alcohol; the solution gave a precipitate with perchloride of mercury and with galls; this agrees entirely with the characters of gluten. There yet remained a small quantity of a brown powder, which was easily soluble in a weak solution of caustic potassa; this proved to be vegetable albumen. It is very probable that in the sap, these two last-mentioned substances were held in solution by acetic acid, and that they were precipitated on its evaporation. From these experiments the sap appears to contain

1. Water and acetic acid	62·00
2. Galactin	30·57
3. Gum and saline matter, probably acetate of magnesia	4·37
4. Gluten and albumen	3·06
	100·00

When the æthereal solution of galactin was evaporated, and the residue carefully fused to expel all adhering æther, pure galactin was obtained; its properties were as follows. It was transparent, and of a pale yellow colour; tough, adhering strongly to the fingers, and drawing out between them into threads, having a shining pearly lustre; but at low temperatures it became hard and brittle. It was fusible between 120° and 130° Fahrenheit; when exposed to a heat of above 500°, it boiled and was entirely volatilized, but I rather believe a portion was decomposed, giving rise to the formation of a highly combustible fluid; it was combustible like wax, burning with a bright smoky flame. On the surface of boiling water it floated like an oil, and was not sensibly altered, but a portion was carried up in vapour with the steam; when, however, the galactin was not perfectly dry, or contained any moisture, it rendered the water turbid from the mechanical suspension of small particles throughout it. It was not saponifiable by potassa or by ammonia; when not dry, however, it frothed up, and seemed to combine with a boiling solution of potassa; on cooling, it separated unaltered. It was soluble in cold alcohol, more easily in cold æther, and still more so in either of them when hot; from its alcoholic solution it was precipitated in white flakes by water; this was a mechanical mixture of galactin and water. It dissolved in sulphuric acid, and when heated caused its decomposition.

The action of nitric acid on galactin was very remarkable; at common temperatures it did not immediately act on it, but when left for some days it converted it into an opake, yellow, friable mass; this same substance was obtained more rapidly when the acid was heated, effervescence ensued, and on dilution, a pale yellow precipitate fell. This substance resembled a resin: it was fusible, combustible, soluble in æther, brittle, and friable; it had a resinous odour, and was insoluble in hot or cold water: during this action no oxalic acid was formed.

Galactin resembles wax in being fusible, volatile, combustible, and insoluble in water, but it differs in the following effects of reagents.

Action of	On wax	On galactin.
Cold sulphuric acid	none	dissolves it.
Hot ditto	combines with	decomposed by.
Nitric acid	forms oxalic acid	forms no oxalic acid.
Alkalies	saponify	no action.
Cold alcohol	no action	dissolves it.
Cold æther	no action	dissolves it.

LXII. *New Demonstration of an original Proposition in the Theory of Numbers.* *By* W. G. HORNER, ESQ.

To Richard Phillips, Esq., F.R.S., L. & E., &c.

My dear Sir,

I WILL not trouble you with an array of corollaries and practical deductions, for which I hope to find a fitting place elsewhere, but trust that you will regard the succinct statement which I send as not unsuitable to the purposes of your Magazine. I am, yours very sincerely,

W. G. HORNER.

" If c is a prime number, and N any number not divisible by c, the quantity $N^{c-1}-1$ will be divisible by c, so that $\frac{N^{c-1}-1}{c}$ will be an integer."

This theorem, which is due to Fermet, is justly described by Legendre as " one of the most important in the theory of numbers." He assumes it as the basis upon which his own theorems respecting prime numbers are founded, and to which, of consequence, the disquisitions of Gauss are equally to be referred.

Euler's demonstration, which has been generally adopted by other writers, is exceedingly simple and convincing, but does not appear capable of being extended to meet the case of composite divisors, without a very complicated and operose process. Such a process is sketched in the concluding paragraph of Part IV. chap. ii. of Legendre's Théorie; but the writer does not appear to have actually gone through it, nor am I aware that any one else has.

A different demonstration, not less simple, was given by Mr. Ivory in Leybourn's Repository; and being engaged at the time in a research, which caused the want of a more enlarged theorem to be urgently felt, I was glad to perceive that the new demonstration was more tractable than Euler's, and might easily be made to include the general case. The result of my attempt is given in the Annals of Philosophy for February 1826.

The mode of demonstration, however, is little less artificial than that of Euler, and did not supply a ready transit to the distinct cases which occur in its practical application. But by closely observing the indications which arise in actual practice, I soon afterwards found myself in the track of a demonstration of the most satisfactory description; simple, convincing, and naturally adapting itself to all the requisite practical uses. It is this which I now wish to submit to mathematicians; and I trust that the Theorem itself, which is here proved, will not be regarded as a mere extension of Fermat's, but recognised as the primordial idea from which his discovery was a fragment fortuitously detached.

Theorem.—If N, P, are prime to each other and c indicates the number of integers less than P and prime to it, $\frac{N^c - 1}{P}$ is an integer.

Dem. 1.—Let us consider the general formula $\frac{N^u - R_u}{P} = I$ an integer; where R_u is the remainder left after dividing N^u by P: $-R_u$ is prime to P; for had they a common factor, it would divide $IP + R_u$, that is N^u; and so P and N^u would have a common factor; which is against the hypothesis (A). Hence R_u is an integer $\angle$ P, and prime to it.

2. As this class of integers is limited, it is obvious that in assigning to u the successive values 0, 1, 2, 3,……to an indefinite extent, the same values of R_u will repeatedly occur. Assume, therefore, $R_{u+z}=R_u$; then $\frac{N^u-R_u}{P}$ and $\frac{N^{u+z}-R_u}{P}$ being integers, their difference $\frac{N^u\,(N^z-1)}{P}$ is also integral. Therefore $\frac{N^z-1}{P}$ is integral; since P and N^u are incommensurable.

3. We will suppose d to represent the least value of z, which satisfies this condition. Then it will also represent the least interval of recurrence of the same remainder. For if the equation $R^u=R_{u+b}$ were possible with $b\angle d$, we should have $\frac{N^u-R_u}{P}$ and $\frac{N^{u+b}-R_u}{P}$ integral; and therefore their difference $\frac{N^u\,(N^b-1)}{P}$; and consequently $\frac{N^b-1}{P}$ integral, with $b\angle d$; which is against the hypothesis.

(B.) It follows from this that the series of remainders R_1, R_2, R_3,……($R_a=$)1 recur perpetually in the same order, and are all distinct from each other.

4. If $a=c$, the proof is complete; for by (A) it cannot exceed c. But if $a\angle c$, let q be any one of the remaining $c-a$ integers, which are less than P and prime to it; and let $\frac{q\quad{}^u-Q_u}{P}$ be integral. But $\frac{qN^u-qR_u}{P}$ is also integral. Wherefore their difference $\frac{qR_u-Q_u}{P}$ is integral; or qR_u is $=Q_u$, rejecting the P's.

5. Now the same reasoning as was employed in Step (1), will prove that Q_u is of the class (A). But it is not found in the series (B). For, imagine it to be $=R_z$; then $qR_u=R_z$; whence $\frac{N^z-qR_u}{P}$ is integral. From it deduct q times the integer $\frac{N^u-R_u}{P}$. The remainder is $\frac{N^u\,(N^{z-u}-q)}{P}$ integral; whence $\frac{N^{z-u}-q}{P}$ is integral; or $q=R_{z-u}$ a term of series (B).

Which is against the hypothesis (4).

6. As at step (3), it may be shown that $Q_u = Q_{u+b}$ is impossible, if $b \angle a$.

(C.) Hence the series of remainders Q_1, Q_2, Q_3,...... $(Q_a =)\ q$, recur perpetually in the same order, and are all distinct from each other, and from the terms of series (B).

7. Should any term of (A) yet remain, a continuation of this process (4) will exhaust them. For, that $\frac{c}{a}$ is integral may be ascertained by reflecting that every unanticipated assumption, such as q, entails a, terms such as series (C), all unanticipated.

8. But since $\frac{N^a - 1}{P}$ is integral, and $\frac{c}{a}\, m$ is also integral, $\frac{N^{am} - 1}{P}$ is integral; that is $\frac{N^c - 1}{P}$. Q.E.D.

*** Since this paper was received we were pained to learn that its distinguished author is no more. We have been favoured by a mathematical friend with the following notice of his decease and his character. "Mr. Horner died on the 21st of Sept. after a week of extreme suffering, arising from a complication of asthma and ossification of the heart, aged fifty-one. His health had been for many years in a very precarious state, and his sufferings (as in this class of disorders they almost always are) often very great; yet his equanimity and firmness of character were fully equal to cope with pain and bodily decay; and his end was such as a Christian's should be—calm, peaceful, happy.

"As a mathematician Mr. Horner was undoubtedly amongst the first men of our time. His method of continuous approximation to the roots of equations of all kinds, but which has hitherto been little applied except to algebraical equations, is probably the most important invention with which algebra has been enriched during the last two centuries. The Philosophical Magazine contains a considerable number of his papers on mathematical and physical subjects, which are of course well known to its readers, and which, therefore, we need not specify or eulogise. Some alterations in this paper were sent off only a few days before his lamented decease.

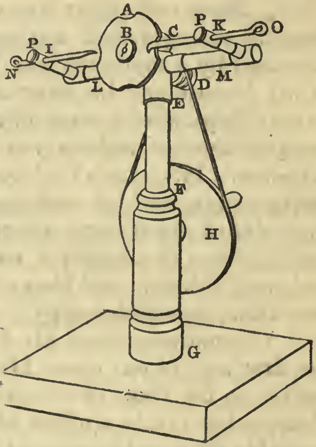
"Mr. Horner was not a mathematician only, he was a man of the most refined taste in literature, both classical and general. He has left behind him many papers on these subjects, on biblical criticism, and on other subjects, as well as on mathematics. His poetic compositions are full of taste and feeling; and his Latin compositions are remarkable for elegance and purity. In this respect, indeed, he combined qualities, powers and taste which are scarcely recorded to have been coexistent with his high mathematical faculties, in any other man."

LXIII. *Description of the Galvanic Shock-Multiplier. By the Rev.* N. S. HEINEKEN.*

THE drawing represents an apparatus for producing a rapid succession of shocks when used with the galvanic pile or battery, and may be called the Galvanic Shock-Multiplier. The idea was derived from the revolving armature of the magnetic battery; but I am not aware that anything of the same kind has been applied to the ordinary galvanic battery.

Description.

A is a thin wheel of copper having four or more circular indentations at equal distances in its circumference. It is fastened by a nut at B to a spindle which passes through the brass tube C, and has at its other end a pulley D. The tube is insulated by means of the glass pillar E F fixed into the wooden support F G, and by this attached to the base. H a wheel by turning which the pulley D and disc A revolve. I K two brass arms attached by mountings to glass tubes at L and M, and by them insulated and fixed also to C E. These two brass arms hold the copper wires N O by means of the screws P P. The wire N has its end formed into a slight spring; so also has O. That at N touches the copper disc A only where the circumference is entire; that at O always presses lightly on the face of the disc. The mode of operation of the apparatus will be sufficiently obvious. If a wire having a moist sponge be attached to one extremity while the other is connected with one of the poles of a battery, and the hand grasp the moist sponge, and if the other pole of the battery be connected by a wire with N, the other hand grasping



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* Communicated by the Author.

a second sponge, and connected with the wire O, a rapid succession of shocks will of course be experienced during the revolution of the disc A, in consequence of the interruptions which are occasioned in the circuit by the indentations in its circumference. The application of the instrument in various experiments, particularly those on the dead body, will be readily suggested.

I will not trespass further on your space than to state that the figure is in size one third of that of the instrument, that the copper disc and wires are amalgamated to ensure good contact, and that by a little alteration the apparatus may be employed for the rapid changing of the poles of a battery in electro-magnetic operations.

Sidmouth, Sept. 27, 1837.

LXIV. *Analytical Development of Fresnel's Optical Theory of Crystals.* By J. J. SYLVESTER, *Member of St. John's College, Cambridge.*

THE following is, I believe, the first successful attempt to obtain the full development of Fresnel's Theory of Crystals by direct geometrical methods. Hitherto little has been done beyond finding and investigating the properties of the wave surface, a subject certainly curious and interesting, but not of chief importance for ordinary practical purposes. Mr. Kelland, in a most valuable contribution to the Cambridge Philosophical Transactions *, has incidentally obtained the difference of the squares of the velocities of a plane front in terms of the angles made by it with the optic axes. I have obtained each of the velocities *separately*, and in a form precisely the same for biaxal as for uniaxal crystals.

I have also assigned in my last proposition the place of the lines of vibration in terms of the like quantities, and *that* in a shape remarkably convenient for determining the *plane* of polarization when the ray is given. For at first sight there appears to be some ambiguity in selecting *which* of the *two* lines of vibration is to be chosen when the front is known. If (p) be the perpendicular from the centre of the surface of elasticity let fall upon the front, $\iota_{,}\ \iota_{,,}$ the $<$es made by the front with the optic planes, $\varepsilon_{,}\ \varepsilon_{,,}$ the $<$es between its *due* line of vibration and the optic axes, I have shown that

$$\cos\varepsilon_{,} = \sqrt{\frac{b^2-p^2}{a^2-c^2}\cdot\frac{\sin\iota_{,}}{\sin\iota_{,,}}} \qquad \cos\varepsilon_{,,} = \sqrt{\frac{b^2-p^2}{a^2-c^2}\cdot\frac{\sin\iota_{,,}}{\sin\iota_{,}}}$$

so that all doubt is completely removed. The equation pre-

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* See Lond. and Edinb. Phil. Mag., vol. x. p. 336.—EDIT.

paratory to obtaining the wave surface is found in prop. 6 by common algebra, without any use of the properties of maxima and minima, and various other curious relations are discussed.

Without the most careful attention to preserve pure symmetry, the expressions could never have been reduced to their present simple forms.

Analytical reduction of Fresnel's Optical Theory of Crystals.

Index of Contents :

In proposition 1, a plane front within a crystal being given, the two lines of vibration are investigated.

In proposition 2 it is shown that the product of the cosines of the inclinations of one of the axes of elasticity to the two lines of vibration, is to the same for either other axis of elasticity in a constant ratio for the same crystal ; and the two lines of vibration are proved to be perpendicular to each other.

In proposition 3, a line of vibration being given, the front to which it belongs is determined ; and it is proved that there is only one such, and consequently any line of vibration has but one other line conjugate to it.

In proposition 4, certain relations are instituted between the positions of, and velocities due to, conjugate lines.

In proposition 5, the ∠es made by the front with the planes of elasticity are found in terms of the velocities only.

In proposition 6, the above is reversed.

In proposition 7, the position of the planes in which the two velocities are equal (viz. the optic planes) is determined.

In proposition 8, the position of a front in respect to the optic axes is expressed in terms of the velocities.

In proposition 9, the problem is reversed, and it is shown that if $v_{,}$ $v_{,,}$ be the two normal velocities with which any front can move perpendicular to itself, and $\iota_{,}$ $\iota_{,,}$ the $<$es which it makes with the optic planes,

$$\text{then } v_{,}^2 = a^2 \left(\sin \frac{\iota_{,} - \iota}{2}\right)^2 + c^2 \left(\cos \frac{\iota_{,} - \iota}{2}\right)^2$$

$$v_{,,}^2 = a^2 \left(\sin \frac{\iota_{,} + \iota}{2}\right)^2 + c^2 \left(\cos \frac{\iota_{,} + \iota}{2}\right)^2.$$

In the 10th the $<$es made by a line of vibration with the axes of elasticity is expressed in terms of the two velocities of the front to which it belongs.

In the 11th proposition the velocity due to any line of vibration is expressed in terms of the < es which it makes with the optic axes,

$$\text{viz. } v^2 - b^2 = \overline{a^2 - c^2} \,.\, \cos \varepsilon_{,} \,.\, \cos \varepsilon_{,,}.$$

In the 12th proposition $\varepsilon_{,}$ $\varepsilon_{,,}$ are separately expressed in terms of $\iota_{,}$ $\iota_{,,}$.

In the Appendix I have given the polar or rather radio-angular equation to the wave surface, from which the celebrated proposition of the ray flows as an immediate consequence.

Proposition 1.

Let $l x + m y + n z = 0 \ldots (a)$ be the $= n$ to a given front to determine the lines of vibration therein.

It is clear that if x, y, z be any point in one of these lines, the force acting on a particle placed there when resolved into the plane must tend to the centre. Consequently the line of force at $x\ y\ z$ must meet the perpendicular drawn upon the front from the origin. Now the $= n$ to this perpendicular is

$$\frac{X}{l} = \frac{Y}{m} = \frac{Z}{n} \quad \ldots\ldots \quad (1.)$$

And the forces acting at $x\ y\ z$ are $a^2 x$, $b^2 y$, $c^2 z$ parallel to $x\ y\ z$, — so that the $= n$ to the line of force is

$$\frac{X - x}{a^2 x} = \frac{Y - y}{b^2 y} = \frac{Z - z}{c^2 z} \quad . \quad (2.)$$

$$\text{from (2) we obtain} \begin{cases} b^2 y\, X - a^2 x\, Y = (b^2 - a^2)\, x\, y & (3.) \\ c^2 z\, Y - b^2 y\, Z = (c^2 - b^2)\, y\, z & (4.) \\ a^2 x\, Z - c^2 z\, X = (a^2 - c^2)\, z\, x & (5.) \end{cases}$$

Hence $(b^2 - a^2)\, x\, y\, n + (c^2 - b^2)\, y\, z\, l + (b^2 - c^2)\, z\, x\, m$

$$= b^2 y (l\, Z - n\, X) + c^2 z\, (m\, X - l\, Y) + a^2 x\, (n\, Y - m\, Z)$$

but by $b\, z =^{ns}$ (1) $l\, Z - n\, X = 0$ $m\, X - l\, Y = 0$ $n\, Y - m\, Z = 0$

$$\therefore (b^2 - a^2) \frac{n}{z} + (c^2 - b^2) \frac{l}{x} + (a^2 - c^2) \frac{m}{y} = 0 \quad . \quad (b)$$

Also we have $n\, z + l\, x + m\, y = 0 \quad , \quad \ldots\ldots \quad (a.)$

$$\therefore (b^2 - a^2)\, n^2 + (c^2 - b^2)\, l^2 + n\, l \,.\left(\overline{c^2 - b^2} \,.\, \frac{z}{x} + \overline{b^2 - a^2} \,.\, \frac{x}{z}\right)$$

$$= \overline{a^2 - c^2} \,.\, m^2$$

$$\therefore (c^2 - b^2) \left(\frac{z}{x}\right)^2 + \frac{1}{n\, l} \cdot \{\overline{c^2 - b^2} . l^2 + \overline{b^2 - a^2} . n^2 - \overline{a^2 - c^2} . m^2\} \frac{z}{x}$$

$$\ldots + \overline{b^2 - a^2} = 0$$

And in like manner interchanging b, y, m with c, z, n

$$(b^2-c^2)\left(\frac{y}{x}\right)^2 + \frac{1}{m\,l}\cdot\{\overline{b^2-c^2}\,.\,l^2 + \overline{c^2-a^2}\,.\,m^2 - \overline{a^2-b^2}\,.\,x^2\}\frac{y}{x}$$

$$+\overline{c^2 - a^2} = 0.$$

Hence if $\left(\frac{y_{\prime}}{x_{\prime}}\ \frac{z_{\prime}}{x_{\prime}}\right)\left(\frac{y_{\prime\prime}}{x_{\prime\prime}}\ \frac{z_{\prime\prime}}{x_{\prime\prime}}\right)$ be the two systems of values of $\frac{y}{x}$, $\frac{z}{x}$, then $\left(\frac{\mathrm{Y}}{\mathrm{X}} = \frac{y_{\prime}}{x_{\prime}}\frac{\mathrm{Z}}{\mathrm{X}} = \frac{z_{\prime}}{x_{\prime}}\right)\left(\frac{\mathrm{Y}}{\mathrm{X}} = \frac{y_{\prime\prime}}{x_{\prime\prime}}\frac{\mathrm{Z}}{\mathrm{X}} = \frac{z_{\prime\prime}}{x_{\prime\prime}}\right)$ are the two lines of vibration required.

PROPOSITION 2.

By last proposition it appears that

$$\frac{y_{\prime}y_{\prime\prime}}{x_{\prime}x_{\prime\prime}} = \frac{c^2 - a^2}{b^2 - c^2} \quad \ldots\ldots\ldots\ldots \quad (c.)$$

$$\text{and } \frac{z_{\prime}z_{\prime\prime}}{x_{\prime}x_{\prime\prime}} = \frac{b^2 - a^2}{c^2 - b^2} \quad \ldots\ldots\ldots\ldots \quad (d.)$$

$$\therefore \frac{y_{\prime}y_{\prime\prime} + z_{\prime}z_{\prime\prime}}{x_{\prime}x_{\prime\prime}} = \frac{c^2 - b^2}{b^2 - c^2} = -1$$

$$\therefore x\,x_{\prime\prime} + y_{\prime}y_{\prime\prime} + z_{\prime}z_{\prime\prime} = 0.$$

And $\therefore$ the two lines of vibration are perpendicular to each other.

N.B. $=^{\text{ns}}$ (c) and (d) must not be overlooked.

PROPOSITION 3.

A line of vibration is given $\left(\text{i. e. } \frac{y_{\prime}}{x_{\prime}}\ \frac{z_{\prime}}{x_{\prime}} \text{ are given}\right)$ and the position of the front is to be determined.

Let $l\,x + m\,y + n\,z = 0$ be the front required,

then $l\,x_{\prime} + m\,y_{\prime} + n\,z_{\prime} = 0$,

$$\text{and } (b^2 - c^2)\,\frac{l}{x_{\prime}} + (c^2 - b^2)\frac{m}{y_{\prime}} + \overline{a^2 - b^2}\,.\,\frac{n}{z}, = 0.$$

Eliminating (n) we get

$$b\,.\left(\overline{a^2 - b^2}\,.\,\frac{x_{\prime}}{z_{\prime}} - \overline{b^2 - c^2}\,.\,\frac{z_{\prime}}{n_{\prime}}\right)$$

$$+ m \cdot \left(\overline{a^2 - b^2} \cdot \frac{y_{\prime}}{z_{\prime}} - \overline{c^2 - b^2} \cdot \frac{z_{\prime}}{y_{\prime}}\right) = 0$$

$$\therefore \frac{l}{m} = \frac{x_{\prime}}{y_{\prime}} \cdot \frac{\overline{a^2 - b^2} \cdot y_{\prime}^2 - \overline{c^2 - a^2} \cdot z_{\prime}^2}{\overline{b^2 - c^2} \cdot z_{\prime}^2 - \overline{a^2 - b^2} \cdot x_{\prime}^2}$$

$$= \frac{x_{\prime}}{y_{\prime}} \cdot \frac{a^2 \cdot (x_{\prime}^2 + y_{\prime}^2 + z_{\prime}^2) - (a^2 x_{\prime}^2 + b^2 y_{\prime}^2 + c^2 z_{\prime}^2)}{b^2 \cdot (x_{\prime}^2 + y_{\prime}^2 + z_{\prime}^2) - (a^2 x_{\prime}^2 + b^2 y_{\prime}^2 + c^2 z_{\prime}^2)}$$

If now we make $x_{\prime}^2 + y_{\prime}^2 + z_{\prime}^2 = 1$

$$a^2 x_{\prime}^2 + b^2 y_{\prime}^2 + c^2 z_{\prime}^2 = v_{\prime}^2$$

and $\therefore \dfrac{l}{m} = \dfrac{x_{\prime}}{y_{\prime}} \cdot \dfrac{a^2 - v_{\prime}^2}{b^2 - v_{\prime}^2}$

and in like manner

$$\frac{l}{n} = \frac{x_{\prime}}{z_{\prime}} \cdot \frac{a^2 - v_{\prime}^2}{c^2 - v_{\prime}^2}$$

$\therefore \overline{a^2 - v_{\prime}^2}\, x_{\prime} \cdot x + \overline{b^2 - v_{\prime}^2} \cdot y_{\prime} \cdot y + \overline{c^2 - v_{\prime}}\, z_{\prime} \cdot z = 0$ is the $=^n$ required.

Proposition 4.

$\dfrac{l}{m}$, $\dfrac{l}{n}$ having each only one value, shows that only one front corresponds to the given line of vibration. Let $x_{\prime\prime}\, y_{\prime\prime}\, z_{\prime\prime}\, v_{\prime\prime}$ correspond to $x_{\prime}\ y_{\prime}\ z_{\prime}\ v_{\prime}$ for the conjugate line of vibration, then the $= n$ to the front may be expressed likewise by

$$\overline{a^2 - v_{\prime\prime}^2}\ x_{\prime\prime} x + \overline{b^2 - v_{\prime\prime}^2} \cdot y_{\prime\prime} y + \overline{c^2 - v_{\prime\prime}^2}\, z_{\prime\prime} \cdot z = 0,$$

so that $\dfrac{(a^2 - v_{\prime}^2)\, x_{\prime}}{(a^2 - v_{\prime\prime}^2)\, x_{\prime\prime}} = \dfrac{(b^2 - v_{\prime}^2)\, y'}{(b^2 - v_{\prime\prime}^2)\, y_{\prime\prime}} = \dfrac{(c^2 - v_{\prime}^2)\, z_{\prime}}{(c^2 - v_{\prime\prime}^2)\, z_{\prime\prime}}$.

Proposition 5.

To find ω, ϕ, ψ, the $<$es made by the front with the planes of elasticity in terms of $v_{\prime}\, v_{\prime\prime}$.

By the last proposition

$$(\cos \omega)^2 = \frac{(a^2 - v_{\prime}^2)^2\, x_{\prime}^2}{(a^2 - v_{\prime}^2)^2\, x_{\prime}^2 + (b^2 - v_{\prime}^2)^2\, y_{\prime}^2 + (c^2 - v_{\prime}^2)^2\, z_{\prime}^2}$$

$$= \frac{(a^2 - v_{\prime}^2)\,(a^2 - v_{\prime\prime}^2)\, x_{\prime}\, x_{\prime\prime}}{(a^2 - v_{\prime}^2)\,(a^2 - v_{\prime\prime}^2)\, x_{\prime} \cdot x_{\prime\prime} + (b^2 - v_{\prime}^2)(b^2 - v_{\prime\prime}^2)\, y_{\prime}\, y_{\prime\prime} + (c^2 - v_{\prime}^2)\,(c^2 - v_{\prime\prime}^2)\, z_{\prime}\, z_{\prime\prime}}$$

Now, by proposition (2), $\dfrac{x_{\prime}\, x_{\prime\prime}}{c^2 - b^2} = \dfrac{y_{\prime}\, y_{\prime\prime}}{a^2 - c^2} = \dfrac{z_{\prime}\, z_{\prime\prime}}{b^2 - a^2}$

$\therefore (\cos \omega^2)$

$$= \frac{(a^2 - v_{\prime}^2)\,(a^2 - v_{\prime\prime}^2)\,(c^2 - b^2)}{(a^2 - v_{\prime}^2)\,(a^2 - v_{\prime\prime}^2)(c^2 - b^2) + (b^2 - v_{\prime}^2)(b^2 - v_{\prime\prime}^2)(b^2 - c^2) + (c^2 - v_{\prime}^2)\,(c^2 - v_{\prime\prime}^2)(b^2 - a^2)}$$

$$= \frac{(a^2 - v_{,}^2)(a^2 - v_{,,}^2)(c^2 - b^2)}{a^4 . (c^2 - b^2) + b^4 . (a^2 - c^2) + c^4 (a^2 - b^2)}$$

$$= \frac{(a^2 - v_{,}^2)(a^2 - v_{,,}^2)}{(a^2 - b^2)(a^2 - c^2)}$$

Similarly,

$$(\cos \phi)^2 = \frac{(b^2 - v_{,}^2)(b^2 - v_{,,}^2)}{(b^2 - a^2)(b^2 - c^2)}$$

$$(\cos \psi)^2 = \frac{(c^2 - v_{,}^2)(c^2 - v_{,,}^2)}{(c^2 - a^2)(c^2 - b^2)}.$$

PROPOSITION 6.

To find $v_{,} v_{,,}$ in terms of ω, ϕ, ψ.

By the last proposition

$$\frac{(\cos \omega)^2}{a^2 - v_{,}^2} = \frac{a^2}{(a^2 - b^2)(a^2 - c^2)} - v_{,,}^2 \cdot \frac{1}{(a^2 - b^2)(a^2 - c^2)}$$

$$\frac{(\cos \phi)^2}{b^2 - v_{,}^2} = \frac{b^2}{(a^2 - b^2)(a^2 - c^2)} - v_{,,}^2 \cdot \frac{1}{(b^2 - a^2)(b^2 - c^2)}$$

$$\frac{(\cos \psi)^2}{c^2 - v_{,}^2} = \frac{c^2}{(a^2 - b^2)(a^2 - c^2)} - v_{,,}^2 \cdot \frac{1}{(c^2 - a^2)(c^2 - b^2)}$$

$$\therefore \frac{(\cos \omega)^2}{a^2 - v_{,}^2} + \frac{(\cos \phi)^2}{b^2 - v_{,}^2} + \frac{(\cos \psi)^2}{c^2 - v_{,}^2} = 0,$$

Just in the same way

$$\frac{(\cos \omega)^2}{a^2 - v_{,,}^2} + \frac{(\cos \phi)^2}{b^2 - v_{,,}^2} + \frac{(\cos \psi)^2}{c^2 - v_{,,}^2} = 0.$$

so that $v_{,}^2$, $v_{,,}^2$ are the two roots of the $= n$

$$\frac{(\cos \omega)^2}{a^2 - v^2} + \frac{(\cos \phi)^2}{b^2 - v^2} + \frac{(\cos \psi)^2}{c^2 - v^2} = 0.$$

Cor.—Hence the $= n$ to the wave surface may be obtained by making

$$(\cos \omega)\, x + (\cos \phi)\, y + (\cos \psi)\, z = v,$$

or if we please to apply Prop. (5), we may make

$$\sqrt{\frac{(a^2 - v_{,}^2)(a^2 - v_{,,}^2)}{(a^2 - b^2)(a^2 - c^2)}} \cdot x + \sqrt{\frac{(b^2 - v_{,}^2)(b^2 - v_{,,}^2)}{(b^2 - a^2)(b^2 - c^2)}} \cdot y$$

$$+ \sqrt{\frac{(c^2 - v_{,}^2)(c^2 - v_{,,}^2)}{(c^2 - a^2)(c^2 - b^2)}} \cdot z = v_{,},$$

or, if we please,

$$\sqrt{\frac{(a^2 a^2 - 1)(a^2 - v^2)}{(a^2 - b^2)(a^2 - c^2)}} \cdot x + \sqrt{\frac{(b^2 a^2 - 1)(b^2 - v^2)}{(b^2 - a^2)(b^2 - c^2)}} \cdot y$$

$$+ z \cdot \sqrt{\frac{(c^2 a^2 - 1)(c^2 - v^2)}{(c^2 - a^2)(c^2 - b^2)}} = 1,$$

and eliminating by differentiation (n) and (v) we obtain

$$\frac{a^2 x^2}{a^2 - (x^2 + y^2 + z^2)} + \frac{b^2 y^2}{b^2 - \overline{x^2 + y^2 + z^2}^2} + \frac{c^2 z^2}{c^2 - \overline{x^2 + y^2 + z^2}^2} = 1.$$

PROPOSITION 7.

To find when $v_{\prime} = v_{\prime\prime}$.

By Prop. 4,

$$\frac{x_{\prime}(v_{\prime}^2 - a^2)}{x_{\prime\prime}(v_{\prime\prime}^2 - a^2)} = \frac{y_{\prime}(v_{\prime}^2 - b^2)}{y_{\prime\prime}(v_{\prime\prime}^2 - b^2)} = \frac{z_{\prime}(v_{\prime}^2 - c^2)}{z_{\prime\prime}(v_{\prime\prime}^2 - c^2)} \quad . \quad . \quad . \ (\theta.)$$

Hence when $v_{\prime\prime} = v_{\prime\prime}$ we have, generally speaking,

$$\frac{x_{\prime}}{x_{\prime\prime}} = \frac{y_{\prime}}{y_{\prime\prime}} = \frac{z_{\prime}}{z_{\prime\prime}}.$$

Now $x_{\prime} x_{\prime\prime} + y_{\prime} y_{\prime\prime} + z_{\prime} z_{\prime\prime} = 0$

$\therefore\ x_{\prime}^2 + y_{\prime}^2 + z_{\prime}^2$ would $= 0$, which is absurd.

The only case therefore when $v_{\prime}$ can $= v_{\prime\prime}$ is when one of those terms of $=n$ (θ) becomes $\frac{0}{0}$: thus suppose $v_{\prime} = b$, then we have $\frac{x_{\prime}}{x_{\prime\prime}} = \frac{z_{\prime}}{z_{\prime\prime}} = \frac{0}{0}$, and we can no longer infer $\frac{x_{\prime}}{x_{\prime\prime}} = \frac{y_{\prime}}{y_{\prime\prime}}$.

Let now $(\omega_{\prime}\ \phi_{\prime}\ \psi_{\prime})$ $(\omega_{\prime\prime}\ \phi_{\prime\prime}\ \psi_{\prime\prime})$ be the two systems of values which ω, ϕ, ψ assume when $v_{\prime} = v_{\prime\prime} = b$, then applying the $=n$ of Prop. (5) we have

$$\cos\omega_{\prime} = \sqrt{\frac{a^2 - b^2}{a^2 - c^2}} \qquad \cos\omega_{\prime\prime} = \sqrt{\frac{a^2 - b^2}{a^2 - c^2}}$$

$$\cos\phi_{\prime} = 0 \qquad \cos\phi_{\prime\prime} = 0$$

$$\cos\psi_{\prime} = \sqrt{\frac{b^2 - c^2}{a^2 - c^2}} \qquad \cos\psi_{\prime\prime} = \sqrt{\frac{b^2 - c^2}{a^2 - c^2}},$$

so that (b) must correspond to the mean axis.

PROPOSITION 8.

$\iota_{\prime}\ \iota_{\prime\prime}$ being the $<$es made by the front with the optic planes to find $\iota_{\prime}\ \iota_{\prime\prime}$ in terms of $v_{\prime}\ v_{\prime\prime}$.

By analytical geometry:

$$\cos \iota_{\prime} = \cos \omega \,.\, \cos \omega_{\prime} + \cos \phi \,.\, \cos \phi_{\prime} + \cos \psi \,.\, \cos \psi_{\prime}$$

$$= \sqrt{\frac{(b_{\prime}^2 - a^2)(v_{\prime\prime}^2 - a^2)}{(a^2 - b^2)(a^2 - c^2)}} \,.\, \sqrt{\frac{a^2 - b^2}{a^2 - c^2}}$$

$$+ \sqrt{\frac{(v_{\prime}^2 - c^2)(v_{\prime\prime}^2 - c^2)}{(c^2 - a^2)(c^2 - b^2)}} \,.\, \sqrt{\frac{c^2 - b^2}{c^2 - a^2}}$$

$$= \frac{\sqrt{(v_{\prime}^2 - a^2)(v_{\prime\prime}^2 - a^2)} + \sqrt{(v_{\prime}^2 - c^2)(v_{\prime\prime}^2 - c^2)}}{(a^2 - c^2)},$$

and similarly

$$\cos \iota_{\prime\prime} = \cos \omega \,.\, \cos \omega_{\prime\prime} + \cos \phi \,.\, \cos \phi_{\prime\prime} + \cos \psi \,.\, \cos \psi_{\prime\prime}$$

$$= \frac{\sqrt{(v_{\prime}^2 - a^2)(v_{\prime\prime}^2 - a^2)} - \sqrt{(v_{\prime}^2 - c^2)(v^{\prime\prime 2} - c^2)}}{a^2 - c^2}.$$

PROPOSITION 9.

To find $v_{\prime}\, v_{\prime\prime}$ in terms of $c_{\prime}\, c_{\prime\prime}$.

By the last proposition

$$\cos \iota_{\prime} \,.\, \cos \iota_{\prime\prime} = \frac{(v_{\prime}^2 - a^2)(v_{\prime\prime}^2 - a^2)(v_{\prime}^2 - c^2)(v_{\prime\prime}^2 - c^2)}{(a^2 - c^2)^2}$$

$$= \frac{(a^4 - c^4) - \overline{a^2 - c^2} \,.\, \overline{v_{\prime}^2 + v_{\prime\prime}^2}}{(a^2 - c^2)^2}$$

$$= \frac{(a^2 + c^2) - \overline{v_{\prime}^2 + v_{\prime\prime}^2)}}{(a^2 - c^2}$$

$$\therefore v_{\prime}^2 + v_{\prime\prime}^2 = a^2 + c^2 - (a^2 - c^2) \cos \iota_{\prime} \cos \iota_{\prime\prime}.$$

Again,

$$(\sin \iota_{\prime})^2 \,.\, (\sin \iota_{\prime\prime})^2 = 1 - \cos \iota_{\prime}^2 - \cos \iota_{\prime\prime}^2 + (\cos \iota_{\prime})^2 (\cos \iota_{\prime\prime})^2$$

$$= 1 - 2 \,.\, \frac{(v_{\prime}^2 - a^2)(v_{\prime\prime}^2 - a^2) + (v_{\prime}^2 - c^2)(v_{\prime\prime}^2 - c^2)}{(a^2 - c^2)^2}$$

$$+ \frac{(a^2 + c^2)^2 - 2(a^2 + c^2)(v_{\prime}^2 + v_{\prime\prime}^2) + (v_{\prime}^2 + v_{\prime\prime}^2)^2}{(a^2 - c^2)^2}$$

$$= \frac{v_{\prime}^4 - 2 v_{\prime}^2 v_{\prime\prime}^2 + v_{\prime\prime}^4}{(a^2 - c^2)^2}$$

$$\therefore v_{\prime}^2 - v_{\prime\prime}^2 = (a^2 - c^2) \sin \iota_{\prime} \,.\, \sin \iota_{\prime\prime}$$

but $v_{\prime}^2 + v_{\prime\prime}^2 = (a^2 + c^2) - (a^2 - c^2) \cos \iota_{\prime} \cos \iota_{\prime\prime}$

$$\therefore v_{\prime}^2 = \frac{a^2 + c^2}{2} - \frac{a^2 - c^2}{2} \cos (\iota_{\prime} + \iota_{\prime\prime})$$

$$= a^2 \left(\sin \frac{c_{,} + c}{2}\right)^2 + c^2 . \left(\cos \frac{\iota_{,} + \iota_{,,}}{2}\right)^2$$

$$v_{,,}^2 = \frac{a^2 + c^2}{2} - \frac{a^2 - c^2}{2} \cos (\iota_{,} - \iota_{,,})$$

$$= a^2 \left(\sin \frac{c_{,} - c}{2}\right)^2 + c^2 \left(\cos \frac{\iota_{,} + \iota_{,,}}{2}\right)^2.$$

Thus for uniaxal crystals where $\iota_{,} + \iota = 80$

$$v_{,}^2 = a^2$$

$$v_{,,}^2 = a^2 . (\cos \iota)^2 + c^2 (\sin \iota)^2.$$

[To be continued.]

LXV. *Proceedings of Learned Societies.*

ZOOLOGICAL SOCIETY.

Dec. 13, 1836.* A Paper was read by William Ogilby, Esq., with a view of pointing out the characters to which the most importance should be attached in establishing generic distinctions among the *Ruminantia*.

Mr. Ogilby commences by observing that "It has been justly remarked by Professor Pallas, that if the generic characters of the *Ruminantia* were to be founded upon the modifications of dentition, in accordance with the rule so generally applicable to other groups of Mammals, the greater part of the order would necessarily be comprised in a single genus; since the number, form, and arrangement of the teeth being the same in all, except the *Camels* and *Llamas*, these organs consequently afford no grounds of definite or general distinction. Hence it is that naturalists have been obliged to resort to other principles to regulate the distribution of ruminating animals; and the form, curvature, and direction of the horns, selected for this purpose at a period when the extremely limited knowledge of species permitted the practical application of such arbitrary and artificial characters without any very glaring violation of natural affinities, still continue to be the only rule adopted by zoologists in this department of Mammalogy. The illustrious Illiger forms a solitary but honourable exception; he first introduced the consideration of the muzzle and lachrymal sinus into the definitions of the genera *Antilope*, *Capra*, and *Bos*; but his labours were disregarded by subsequent writers, or his principles applied only to the subdivision of the genus *Antilope*. It is obvious, however, that as the knowledge of new forms and species became more and more extensive, the prevailing gratuitous rule above mentioned, founded as it is upon purely arbitrary characters which have no necessary relation to the habits and œconomy, or even to the general external form, of the animals themselves, would eventually involve in confusion and inconsistency the different groups which were founded upon its application; and such has long been

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* The preceding papers read at this meeting have been noticed in the present volume, p. 196.

its acknowledged effect. The genus *Antilope*, in particular, has become a kind of zoological refuge for the destitute, and forms an incongruous assemblage of all the hollow-horned *Ruminants*, without distinction of form or character, which the mere shape of the horns excluded from the genera *Bos*, *Ovis*, and *Capra*; it has thus come to contain nearly four times as many species as all the rest of the hollow-horned *Ruminants* together; so diversified are its forms, and so incongruous its materials, that it presents not a single character which will either apply to all its species, or suffice to differentiate it from conterminous genera.

"To meet this obvious evil, MM. Lichtenstein, De Blainville, Desmarest, and Hamilton Smith have applied Illiger's principles to subdivide the artificial genus *Antilope* into something more nearly approaching to natural groups; the reform thus effected, however, was but partial in its operation; the root of the evil still remained untouched, for none of these eminent zoologists appears to have been sufficiently aware of the extremely arbitrary and artificial character of the principal group itself, which they contented themselves with breaking up into subgenera, nor of the actual importance and extensive application of the characters which they employed for that purpose. By mixing up these characters, moreover, with others of a secondary and less important nature, the benefit which might have been expected from their labours has been, in a great measure, neutralized; and even the subdivisions which they have introduced into the so-called genus *Antilope*, are less definite and comprehensive than they might otherwise have been made.

"The truth is, however, that the presence or absence of horns in one or both sexes; the substance and nature of these organs, whether solid or concave, permanent or deciduary; the form of the upper lip, whether thin and attenuated as in the goat, or terminating in a broad heavy naked muzzle as in the *Ox*; and the existence of lachrymal sinuses and interdigital pores, are the characters which really influence the habits and œconomy of ruminating animals, and upon which, consequently, their generic distinctions mainly depend. These, with the assistance, in a very few instances, of such accessory characters as the superorbital and maxillary glands, the number of teats, and the existence of inguinal pores, are sufficient in all cases to define and characterize the genera with the strictest reference to logical precision and zoological simplicity. It is not my intention to discuss the value of these characters, or to state the reasons which induced me to adopt them in preference to those more generally employed in this department of Mammalogy; these will form the subject of a future communication, and I shall content myself for the present with observing, that the presence or absence of horns in the females regulates, in a great measure, the social intercourse of the sexes; that upon the form of the lips and muzzle, the only organs of touch and prehension among the *Ruminantia*, depend the nature of the food and habitat, making the animal a *grazer* or a *browser*, as the case may be; and that the existence or nonexistence of interdigital glands, the use of which appears to be to lubricate the hoofs, has a very extensive influence upon the geographical distribution of the species;

confining them to the rich savannah and the moist forest, or enabling them to roam over the arid mountain, the parched karroo, and the burning desert.

"Having thus briefly explained the necessity of reforming the characters of the different groups of the Order *Ruminantia,* as they are at present constituted, and the nature and value of the principles which I propose to employ for that purpose, I shall at once proceed to their practical application, confidently anticipating that their employment will remove the most serious objections which exist against the present distribution of the order, and place our knowledge of these interesting animals, in point of scientific accuracy, precision, and affinity, on a par with the more generally cultivated departments of zoology.

Fam. I. CAMELIDÆ.

Pedes subbisulci, subtùs callosi, digitis apice solo distinctis; ungulæ succenturiatæ nullæ; *cornua* nulla; *dentes primores* suprà duo, infrà sex.

2 Genera.

1. CAMELUS, cujus characteres sunt: *Digiti* conjuncti, immobiles. *Rostrum* chilomate instructum, labro fisso. *Sinus lachrymales* nulli. *Fossæ interdigitales* nullæ. *Folliculi inguinales* nulli. *Mammæ* quatuor.

2. AUCHENIA: *Digiti* disjuncti, mobiles. *Rostrum* chilomate instructum, labro fisso. *Sinus lachrymales* nulli. *Fossæ interdigitales* nullæ. *Folliculi inguinales* nulli. *Mammæ* duæ.

"The *Camelidæ* form what Mr. MacLeay would call an aberrant group; they differ essentially from other Ruminants in the structure both of the organs of locomotion and of mastication, and their generic distinctions consequently depend upon characters which have no application to the remaining groups of the order. On the other hand, the principles of generic distribution which subsist among the rest of the *Ruminantia* appear to furnish negative characters only when applied to the *Camelidæ;* but though necessarily expressed negatively, the absence of lachrymal, inguinal, and interdigital sinuses forms, in reality, positive and substantial characters, and as such, as well as for the sake of uniformity, should be introduced into the definition of these, as well as of other genera, in which they unavoidably appear under a negative form.

Fam. II. CERVIDÆ.

Pedes bisulci; *cornua* solida, plerùmque decidua, in mare solo, aut in utroque sexu; *dentes primores* suprà nulli, infrà octo.

6 Genera.

1. CAMELOPARDALIS. *Cornua* in utroque sexu, perennia, simplicia, cute obducta. *Rhinaria* nulla. *Sinus lachrymales* nulli. *Fossæ interdigitales* parvæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Duo species sunt *C. Æthiopicus* et *C. Capensis.*

2. TARANDUS. *Cornua* in utroque sexu, subpalmata, decidua. *Rhinaria* nulla. *Sinus lachrymales* exigui. *Fossæ interdigitales* parvæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *Tarandus Rangifer* (*Cervus Tarandus*).

3. ALCES. *Cornua* in mare solo, palmata, decidua. *Rhinaria* nulla. *Sinus lachrymales* exigui. *Fossæ interdigitales* magnæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *Alces Machlis* (*Cervus Alces*).

4. CERVUS. *Cornua* in mare solo, ramosa, decidua. *Rhinaria* magna. *Sinus lachrymales* distincti, mobiles. *Fossæ interdigitales* magnæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typi sunt *C. Elaphus* et *C. Saumer* aut *Hippelaphus*, Cuv.

5. CAPREA. *Cornua* in mare solo, subramosa, decidua. *Rhinaria* distincta. *Sinus lachrymales* nulli. *Fossæ interdigitales* magnæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *C. Capreolus*.

6. PROX. *Cornua* in mare solo, subramosa, decidua. *Rhinaria* magna. *Sinus lachrymales* maximi, mobiles. *Sinus* duo supraorbitales ad basin cornuum, magni, mobiles. *Fossæ interdigitales* magnæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *Prox Moschatus* (*Cervus Muntjac*).

Fam. III. MOSCHIDÆ.

Pedes bisulci; *cornua* nulla; *dentes primores* suprà nulli, infrà octo. 2 Genera.

1. MOSCHUS. *Rhinaria* magna. *Sinus lachrymales* nulli. *Fossæ interdigitales* nullæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *Moschus Moschiferus*.

2. IXALUS? *Rhinaria* nulla. *Sinus lachrymales* exigui, distincti. *Fossæ interdigitales* nullæ. *Folliculi inguinales* exigui. *Mammæ* duæ. Typus est *Ixalus Probaton*, present vol. p. 124, or Proc. Zool. Soc., Part IV. page 119.

"The genus *Ixalus*, founded upon the observation of a single specimen, may eventually prove to belong to a different family; it differs little, indeed, from the true Antelopes: but even supposing it to be correctly placed among the *Moschidæ*, other forms are still wanting to fill up the chasms which evidently exist among the characters of that group. Two are more especially indicated, and our knowledge of the laws of organic combination and of the constituent parts of other groups, gives us every reason to believe in their actual existence, and to anticipate their discovery. They will be characterized nearly as follows, and will probably be found, one in the tropical forests of the Indian Archipelago, and the other on the elevated table lands of Mexico or South America.

HINNULUS. *Rhinaria* magna. *Sinus lachrymales* distincti. *Fossæ interdigitales* nullæ. *Folliculi inguinales* nulli. *Mammæ* quatuor.

CAPREOLUS. *Rhinaria* nulla. *Sinus lachrymales* nulli. *Fossæ interdigitales* parvæ? *Folliculi inguinales*? *Mammæ* duæ.

"It may appear a bold, perhaps a presumptuous undertaking, thus to predict the discovery of species, and define the characters of genera, of whose actual existence we have no positive knowledge; but, as already remarked, all the analogies of nature, whether derived from organic combination or from the constituent members of similar groups, are in favour of the supposition; and I may observe further, that the recent discovery of the genus *Ixalus*, if indeed it eventually prove to be a genus, of which I had long previously defined the characters, as I have here done for the presumed genera *Hinnulus* and *Capreolus*, strengthens my belief in the actual existence of these forms, and increases the probability of their future discovery.

Fam. IV. CAPRIDÆ.

Pedes bisulci; *cornua* cava, persistentia; *rhinaria* nulla; *dentes primores* suprà nulli, infrà octo.

7 Genera.

1. MAZAMA. *Cornua* in mare solo. *Sinus lachrymales* nulli. *Fossæ interdigitales* distinctæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *M. Furcifer* (*Antilope Furcifer*).

2. MADOQUA. *Cornua* in mare solo. *Sinus lachrymales* distincti. *Fossæ interdigitales* distinctæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *M. Saltiana* (*Ant. Saltiana* et *Hemprichii*).

3. ANTILOPE. *Cornua* in mare solo. *Sinus lachrymales* distincti, mobiles. *Fossæ interdigitales* maximæ. *Folliculi inguinales* maximi. *Mammæ* duæ. Typus est *A. Cervicapra*.

4. GAZELLA. *Cornua* in utroque sexu. *Sinus lachrymales* distincti, mobiles. *Fossæ interdigitales* maximæ. *Folliculi inguinales* maximi. *Mammæ* duæ. Typus est *Gazella Dorcas* (*Ant. Dorcas*).

5. OVIS. *Cornua* in utroque sexu. *Sinus lachrymales* exigui, immobiles. *Fossæ interdigitales* parvæ. *Folliculi inguinales* nulli. *Mammæ* duæ. Typus est *Ovis Aries*.

6. CAPRA. *Cornua* in utroque sexu. *Sinus lachrymales* nulli. *Fossæ interdigitales* parvæ. *Folliculi inguinales* nulli. *Mammæ* duæ. Typus est *Capra Hircus*. Ad hoc genus pertinent *Ovis Tragelaphus*, et *Antilope Lanigera* aut *Americana*, Auct.

7. OVIBOS. *Cornua* in utroque sexu. *Sinus lachrymales* nulli. *Fossæ interdigitales*? *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus *Ovibos Moschatus*.

Fam. V. BOVIDÆ.

Pedes bisulci; *cornua* cava, persistentia; *rhinaria* distincta, nuda; *dentes primores* suprà nulli, infrà octo.

9 Genera.

1. TRAGULUS. *Cornua* in utroque sexu. *Glandulæ maxillares* oblongæ. *Fossæ interdigitales* nullæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *T. Pygmæus* (*Ant. Pygmæa*).

2. SYLVICAPRA. *Cornua* in mare solo. *Glandulæ maxillares* oblongæ. *Fossæ interdigitales* parvæ. *Folliculi inguinales* distincti. *Mammæ* quatuor. Typus est *S. Mergens* (*Ant. Mergens*).

3. TRAGELAPHUS. *Cornua* in mare solo. *Sinus lachrymales* magni. *Fossæ interdigitales* distinctæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *T. Hippelaphus* (*Ant. Picta*); the *Neel-ghae*, and not the *Saumer Deer* of India, as I shall show elsewhere, is the animal described by Aristotle under the name of *Hippelaphus*.

4. CALLIOPE. *Cornua* in mare solo. *Sinus lachrymales* nulli. *Fossæ interdigitales* nullæ. *Folliculi inguinales* distincti. *Mammæ* quatuor. Typus est *Calliope Strepsiceros* (*Ant. Strepsiceros*).

5. KEMAS. *Cornua* in utroque sexu. *Sinus lachrymales* nulli.

Fossæ interdigitales magnæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *Kemas Ghoral* (*Ant. Goral*).

6. Capricornis. *Cornua* in utroque sexu. *Sinus lachrymales* magni. *Fossæ interdigitales* distinctæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *C. Thar* (*Ant. Thar*, Hodg.).

7. Bubalus. *Cornua* in utroque sexu. *Sinus lachrymales* exigui, distincti. *Fossæ interdigitales* magnæ. *Folliculi inguinales* nulli. *Mammæ* duæ. Typus est *Bubalus Mauritanicus* (*Ant. Bubalus*).

8. Oryx. *Cornua* in utroque sexu. *Sinus lachrymales* nulli. *Fossæ interdigitales* magnæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Species sunt *O. Capensis* (*Ant. Oryx*), *Leucoryx, Leucophæa*, &c.

9. Bos. *Cornua* in utroque sexu. *Sinus lachrymales* nulli. *Fossæ interdigitales* nullæ. *Folliculi inguinales* nulli. *Mammæ* quatuor. Typus est *Bos Taurus*.

"I have here confined myself strictly to generic characters; the synonyma and discrimination of species will form the subject of a future monograph; in the mean time, with the assistance of the Article Antelope in the Penny Cyclopædia, or, with the proper corrections of Col. Smith's Treatise on the Ruminants in the fourth volume of Griffith's Translation of the 'Règne Animal,' the student will have no difficulty in referring any particular species to its appropriate genus. He will thus be enabled to judge of the correctness or incorrectness of the affinities here indicated, and consequently to form a tolerable estimate of the value of the characters by which I propose to distinguish the genera of ruminating animals; and indeed it is principally from the wish to excite the attention of zoologists to more extensive observation than I myself possess, that I have been induced to publish the present analysis of my own investigations in this department of Mammalogy."

Mr. Gould exhibited numerous examples of the genus *Strix* (as at present restricted), from numerous parts of the globe, including three undescribed species from Australia, which he characterized as follows, the characters being given in No. 48 of the Society's Proceedings.

Strix castanops. Long. tot. 18 unc.; *rostri*, 2¼; *alæ*, 15; *caudæ*, 7; *tarsi*, 3½. *Hab*. In Terrâ Van Diemen. This is the largest known species of the restricted genus *Strix*, of which the common *Barn Owl* is a typical example. *Strix Cyclops*. One of the most beautiful species of the genus. *Strix delicatulus*. Long. tot. 14 unc.; *rostri*, 1¾; *alæ*, 11; *caudæ*, 4; *tarsi*, 2½. *Hab*. In Novâ Cambriâ Australi. This species in some respects very closely resembles the common *British Owl, St. flammea*; but it has a longer bill, and is considerably smaller.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: MEETING AT LIVERPOOL.

Sept. 11. Mr. Whewell rapidly sketched the principle on which his instrument registered the quantity of aërial current passing any place. He had exhibited the instrument in an unfinished state at the Dublin meeting, and in a more matured state of its existence at Bristol; it had since received some valuable improvements, which were suggested by the practical working of the machine. That he might not occupy the time of the Section too long, it would suffice at present to say, that in it a small set of wind-mill vanes, something like the ventilators placed in our windows, were presented to the wind by a common vane, let the direction of the wind blow how it might: the aërial current as it passed set these vanes into rapid motion, and a train of wheels and pinions reduced the motion, which was thence communicated to a pencil traversing vertically, and pressing against an upright cylinder, which formed the support of the instrument, and that 10,000 revolutions of the fly only caused the pencil to descend the one twentieth of an inch. The surface of the cylinder was japanned white, and the pencil as the vane wavered kept tracing a thick irregular line, like the shadings on the coast of a map: the middle of a line was readily ascertained, and it gave the mean direction of the wind actually exhibited before the eye by a diagram, while the length of the line was proportional to the velocity of the wind, and the length of time during which it blew in each direction; which therefore gave what he called the integral effects of the wind, or the total amount of the aërial current which had passed the place of observation in the direction of each point of the compass during the interval which had elapsed since the time of last recording the instrument. This, it was well known, was a subject of much importance in meteorological speculations, but has not been hitherto accomplished. It was indeed deemed of much consequence, to obtain even the mean direction of the wind at a given place, and the celebrated Kämtz, in his *Meteorologie*, has made a collection of several results of this kind; but, in the ordinary way of registering even the direction of the wind, which is, by stating the length of time it blows from a certain point of the compass, it is obvious that the velocity of the wind is altogether left out of account, and therefore the high wind or storm of one day, is placed on a par with the gentle breeze of the next, and therefore not an attempt can be made to infer the total quantity, or what he had ventured to term the integral effect of the wind. Mr. Whewell then exhibited large diagrams, giving the results of the observations recorded at the Cambridge observatory, under the care of Professor Challis, and at the house of the Cambridge Philosophical Society. The similarity of the curves showed a general coincidence, but some discrepancies were accounted for by the fact, that the dome of the Equatorial instruments sheltered the anemometers placed at the observatory on the north side, while that placed upon the house of the Philosophical Society was well situated for receiving the wind from every quarter. Anemometers on this principle had been also erected by Professor Forbes and Mr. Rankin, at Edinburgh, and by Mr. Snow Harris and Mr. Southwood, at Plymouth; but he was not at present prepared to state the results of the observations made with them, though he had little doubt they would be interesting and useful.

The President of the Section supposed it would rather suit the convenience of the Section to hear Mr. Osler give the description of his anemometer and rain-gauge, before they proceeded to make observations on the communication of Mr. Whewell: accordingly,—

Mr. Osler, of Birmingham, read an account of a new Registering Anemometer and Rain Gauge, now in use at the Philosophical Institution, at Birmingham, illustrated by diagrams, giving a condensed view of the observations recorded during the first eight months of the year 1837.

He observed that although the results obtained by this instrument are essentially different from those produced by the anemometer exhibited last year at Bristol, by Professor Whewell, he should have hesitated to introduce the one now submitted to the Section, had he not been kindly encouraged so to do by that gentleman himself. In this instrument the direction of the wind is obtained by means of the vane attached to the rod, or rather tube that carries it, and consequently causes the latter to move with itself. At the lower extremity of this tube is a small pinion working in a rack, which slides backwards and forwards as the wind moves the vane, and to this rack a pencil is attached, which marks the direction of the wind on a paper ruled with the cardinal points, and so adjusted as to progress at the rate of one inch per hour by means of a clock. The force is at the same time ascertained by a plate one foot square, placed at right angles to the vane, supported by two light bars running on friction rollers, and communicating with a spiral spring in such a way that the plate cannot be affected by the wind's pressure, without constantly acting on this spring, and communicating the quantum of its action by a light wire, passing down the centre of the tube to another pencil below which thus registers its degree of force. The rain is registered at the same time by its weight acting on a balance, which moves in proportion to the quantity falling, and has also a pencil attached to it recording the result. The receiver is so arranged as to discharge every quater of an inch that falls, when the pencil again stands at zero.

Mr. Whewell spoke highly of the construction of this anemometer, and he had no doubt but that a very slight modification of the mode of registering its indications would cause it to answer every purpose which he had lately described as desirable. In its present form, however, it was the force of the aërial current which it indicated, not the integral effect. He also highly commended the rain-gauge, and the method of showing to the eye in one diagram so many important meteorological phænomena.—Professor Lloyd stated, that there was a very simple method of causing the anemometer of Mr. Osler to give the integral effect of the wind, and that was to cut out the paper covered by the tracings of the pencil indicating the force of the wind, and to weigh it; for it was easy to perceive, that since the ordinates of the curved spaces covered by those tracings were proportional to the force, and, therefore, the velocity of the wind, and the abscissæ to the time, the areas represented the integrals, or the total amount of the aërial current.—Mr. Ettrick asked, whether some other method of supporting the cylinder which moved back and forward as the force of the wind varied, rather than friction rollers, would not be desirable—such, for instance, as bridle rods, or other means known to practical mechanics, and, he was sure, well known to Mr. Osler. Mr. Osler replied, that many methods of supporting this part of the apparatus had been tried and laid aside, as not answering; among the rest, bridle rods.

Professor Powell then made a communication 'On the Dispersion of Light.' The object of this communication is to state the progress of the inquiry into the subject of dispersion since the last meeting of the Association. On that occasion, the author laid before the Physical Section the results of his observations for determining the refractive indices of the standard rays for 28 media. These have been since published, with some preliminary remarks, as one of the series of memoirs of the Oxford Ashmolean Society. They are to be considered only as first approximations, and it would be very desirable to have many of them carefully repeated, as well as to extend the inquiry to other bodies. The author regrets that he has been unable, from particular circumstances, to carry on these researches during the past summer, but intends to take the first opportunity of resuming them. In particular, he was kindly favoured by Mr. Brooke with a specimen of some crystals of chromate of lead for examination, and accordingly put them into the hands of Mr. Dollond, who warmly entered into his views, and has used every endeavour to give them a prismatic form, but, unfortunately hitherto without success. It is only by such co-operation of those engaged in different departments of science that inquiries like the present can be successfully carried on, and the author is anxious to obtain specimens of any transparent media, which are capable of prismatic examination, and especially such as are of high dispersive power.

Meanwhile, he has been engaged in the comparison of observation and theory, especially among the more highly-dispersive bodies which he has examined. He has performed the calculations by the method of Sir W. R. Hamilton, and has found that for those media whose dispersion is not very great, the coincidences are sufficiently close; but, on proceeding to the more highly-dispersive bodies, especially oil of cassia, the discrepancies increase, and moreover preserve a certain regularity of character, which shows that they are not mere errors of observation: this would seem to warrant the expectation, that a further development of the formula might still give successful results. These investigations have been communicated to the Royal Society, and will appear in the next volume of its Transactions.

Since the period of this communication, however, the able and profound memoir of Mr. Kelland has appeared in the Cambridge Transactions[*]. This gentleman's theory is, in some measure a simplification of Cauchy's; the resulting formula, for the dispersion, though substantially the same, as developed in a different form, and readily capable of being applied to numerical computation. In some correspondence with Mr. Kelland, that gentleman favoured the au-

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[*] See Lond. and Edinb. Phil. Mag., vol. x., p. 336.

thor with a computation for the case of oil of cassia, in which the greatest discrepancies existed. By this method, those discrepancies have been made entirely to disappear; and thus *the most extreme case* at present known is brought under the dominion of the formula of dispersion. It is also to be observed, that Mr. Kelland's series is not rapidly converging; the neglected terms, therefore, *may*, if taken into account, give a still more accurate result. It will, therefore, now become of yet more extreme interest, to find some means of obtaining data for the more highly dispersive substances, such as chromate of lead, realgar, sulphur, &c. With regard to the theory, there may be much still wanting to render it entirely satisfactory. Its first principles have been discussed by several mathematicians, but especially in some papers read by Professor Lloyd to the Royal Irish Academy, embracing, in fact, the whole subject of the propagation of light in uncrystallized media.

Sir D. Brewster observed, that some other method than that of observing the fixed lines of Fraunhofer, must be resorted to, for substances of such imperfect crystalline forms as those examined in this communication,—as, for instance, the chromate of lead. The method he would recommend, would be, either to interpose nitrous gas or plates of mica, so as to form a net-work; a given number of the colours of the resulting rings being then counted, and attended to in the various observations, would be much better than Fraunhofer's lines, which, indeed, in this case, he contended, would be altogether incapable of being accurately observed.—*Athenæum*, Sept. 16.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

The anniversary meeting of this Society was held at Penzance on Monday, Oct. 16th, and it was never more fully attended.

The President, Gilbert Davies, Esq., opened the business of the Meeting by a brief and eloquent address, and presented to the Society his editon of Hals and Tonkins' Parochial History of Cornwall, from which he read many extracts relating to the rocks, minerals, and other products of the county.

The Secretary then read the Report of the Council, which, after noticing the demise of his late Majesty, the accession of the Queen, and the continued royal patronage and support of the Society, and proposing an Address to Her Majesty, proceeds as follows:—

"The labours of the Society during the past year have principally had reference to the organic remains which have been found in different parts of this county: for although their existence in one or two insulated spots was well known, no suspicion was entertained of their occurrence in so many localities, and in such abundance.

"This year has also witnessed the completion of an object which was one of the chief desiderata at the institution of this Society. The valuable researches of many of its members, and of Dr. Boase in particular, had given us a good general outline of the Geology of Cornwall, and accurate details of many parts of it; but the labours of Mr. De la Beche, under the direction of the Board of Ordnance, have at length brought to perfection a Geological Map of the County, executed with the accuracy for which that eminent geologist is so distinguished. This and a book of reference are now in a forward state, and they are to appear early in the ensuing spring.

"Although the Society cannot claim this useful and important work as its own labour, it does not the less heartily greet its appearance, and rejoice in its accomplishment.

"Mr. Henwood's Survey of the Mines is also completed, and the various particulars of it which have been from time to time brought before the Society, with Dr. Boase's Memoir on the Diluvium of Cornwall, and other communications, will appear in a fifth volume of Transactions, now about to be put to press, and which will be published in the course of the next year.

"The Donations to the Museum have been of great value and importance, particularly an extensive suite of the Silurian rocks of Murchison, by Mrs. Stackhouse, Acton; of organic remains of the elephant, rhinoceros, and other mammalia from the Sub-Himalayan mountains, by Lieut. Tremenheere, of the Bengal Engineers; and of the organic remains from the slates of our own county, by Mr. M'Lauchlan, of the Ordnance Survey; by Mr. Peach, officer in the Preventive Service, at Gorran; and by the Curator. For the reception of these and other donations, more accommodation is required, and the Council propose to obtain it by the enlargement of the present cases, and the addition of a new one.

"It being thought that the annual publication of papers read, or abstracts of them, would induce more extensive communications to the Society, the Council have desired the secretary to take the requisite steps; and this will be done for all such as may be presented in the ensuing year.

"They cannot close their Report without alluding to the loss which the Society has sustained by the removal of Dr. Boase, who for so many years discharged the duties of Secretary with so much honour to himself and advantage to the Society."

The *Treasurer* then read his report, which showed the finances of the Society to be flourishing; 173*l.* remaining in hand, after defraying all expences, exclusive of the present year's subscriptions.

The following Papers have been read since the last Report:—On the Utility of a School of Mines in Cornwall:—on the probable sources of its revenue:—and on the plan of management and of instruction in such an Establishment; by Henry S. Boase, M.D., F.R.S., F.G.S., &c., Hon. Mem. of the Society. On the Change of Level of the Land and of Sea, in Cornwall; by Joseph Carne, Esq., F.R.S., F.G.S., M.R.I.A., Treasurer of the Society. Note on the Seivaliks or N. W. Sub-Himalayan belt of Hills; by Capt. P. T. Cautley, Bengal Artillery, F.G.S., &c., Corresponding Member of the Society. On the Fossils which occur in some of the Slates near Gorran, and Fowey; by C. W. Peach, Esq., Associate of the Society. On the relations which exist between Elvan Courses and the central Granite; by Joseph Carne, Esq., F.R.S., &c., Treasurer. On the effects of a trap-dyke on the contiguous strata in a Colliery in Durham;

and on the Temperature of the Cornish Mines;* by Robert Were Fox., Esq., Vice-President of the Society. An account of the Quantity of Tin produced in Cornwall and Devon, in the year ending with the Midsummer Quarter, 1837; by Joseph Carne, Esq., F.R.S., &c., Treasurer. An account of the quantity of Copper produced in Great Britain and Ireland, in the year ending the 30th June, 1837; by Alfred Jenkin, Esq.

The following gentlemen have been elected in the present year:—

Ordinary Members.—Captain Rowland, Penzance; Capt. Davies, Penzance; R. E. A. Townsend, Esq., of London; Major Robyns; Richard Thomas, Esq., of Penzance; Dr. Willin, of Penzance; Lewis Stephens, Esq., of Tregenua Castle; and Nicholas Phillips, Esq., of Penzance.

Honorary Members.—Henry S. Boase, M.D., F.R.S., F.G.S., &c.; L. Cordier, of Paris, member of the Academy of Sciences; W. H. Fitton, M.D., F.R.S., F.G.S.; and R. J. Griffith, Esq., F.G.S., Pres. Geo. Soc. of Dublin.

Corresponding Members.—Capt. Proby T. Cautley, F.G.S., of the Bengal Artillery; E. W. Brayley, Jun., Esq., F.L.S., F.G.S., Librarian to the London Institution; Edward Moore, M.D., F.L.S., Secretary of the Plymouth Institution; Henry M'Lauchlan, Esq., F.G.S.; and Lieut. G. B. Tremenheere, Bengal Engineers.

Associates.—C. W. Peach, Esq., of Gorran; Samuel Martyn, Esq., of Lower St. Columb; Captains Absalom Francis, of Halkin, Flint; Thomas Richards, of Huel Vor; and Thomas Treweeke, of Halsetown.

Officers of Council for the ensuing year.

President.—Davies Gilbert, Esq., D.C.L., F.R.S., F.G.S. *Vice-Presidents.*—Wm. Bolliho, Esq.; Mich. Williams, Esq.; Right Hon. Sir R. Hussey Vivian, Bart., G.C.B., M.P., &c.; and W. Tyringham Praed, Esq. *Secretary.*—(pro tempore) and *Curator.*—W. J. Henwood, F.G.S. *Treasurer.*—Joseph Carne, Esq., F.R.S., F.G.S. &c. *Librarian.*—Richard Hocking, Esq. *Council.*—B. P. Baker, Esq.; D. B. Bedford, Esq.; Richard Davey, Esq., F.G.S.; J. S. Enys, Esq.; R. Were Fox, Esq.; Day Perry le Grice, Esq.; Richard Harvey, Esq.; John B. Pentreath, Esq.; C. W. Popham, Esq.; Rev. John Punnett; E. Hearle Rodd, Esq.; and Rev. Canon Rogers.

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* A brief but animated discussion between the author and Mr. Henwood followed the reading of this paper: the former insisting that water was the medium of transporting heat in the earth; the latter inquiring why, then, the lower and less permeable rock (granite) was at the same depth colder than slate, and why the water occurring in the slate, if coming from below, was not seen in the granite through which it must inevitably pass in its way upward?

LXVI. *Notices respecting New Books.*

UNDER the following head, which will be continued and extended in future Numbers, it is designed to give the titles, and sometimes short notices, of important foreign works, and of the most interesting papers which appear in foreign Journals and Transactions, especially those of Germany, Sweden, Holland, and Russia, as being probably least accessible to the generality of readers. Some of our respected correspondents have expressed a wish for such information; and the diffusion of it may further the objects which the publication of the SCIENTIFIC MEMOIRS is designed to promote, and may, perhaps, assist in making the best selection of papers for the future parts of that work, should the sale of the Volume already published encourage the Editor to continue it.—R. T.

Bibliographical Bulletin.

Skandinaviens Fiskar. (The Fishes of Scandinavia), by B. Fr. Fries and C. U. Ekström. Part 1. 4to. Stockholm, 1836.

It is a great pity that the authors, well known to zoologists, have not employed a more general language, Latin or French, instead of the Swedish with which the greater number of naturalists are unacquainted. The drawings are well executed and coloured with the greatest care from living specimens so that few can be found to equal them.

Neues System der Pflanzen-Physiologie. (A new System of Vegetable Physiology), by F. J. F. Meyen. Berlin, 1837. 8vo.

The first volume treats in the first book on the organs of assimilation and formation; in the second, on the organs of respiration and secretion; and concludes with a comparison of the types according to which the elementary organs in the stems of Monocotyledons, Dicotyledons and Acotyledons are arranged. The author not only gives the results of his own labours but carefully relates those of other botanists. The second volume is to appear next year.

Lehre von der Cohesion (Doctrine of Cohesion), by Prof. M. L. Frankenheim. Breslau, 8vo.

This work is divided into several parts, containing "Elasticity of Gases, Elasticity and Coherence of fluid and solid Bodies, and Crystallography," &c.

Poggendorff's Annalen, 1837. No. 5.

Contents.—On the Nature and formation of Coral islands and of the Coral banks in the Red Sea; by C. G. Ehrenberg. (This paper is highly interesting.)—On the changes of specific gravity which Sea-water undergoes from heat, by A. Erman.—Harris's Electrical experiments in rarefied air, by Dr. Riess, (This is a repetition and full confirmation of Mr. Harris's experiments, which are recorded in Phil. Transact. for 1834).—On the places of the Maxima and Minima of the wave-surface, according to the observations of Fresnel; by K. W. Knockenhauer.—Artificial formation of twin crystals which exhibit without any previous polarization epopoptical figures similar to those observed in Arragonite; by J. Müller.—Additional notice respecting experiments on vinous fermentation and putrefaction; by Th. Schwann.—Description of two new Lamps (for organic analysis and glass-blowing); by H. Hess.—On the Fusibility of Iridium; by Bunsen.—

A new method of dissolving Iridium; by L. R. Fellenberg.—On the determination of the expansion of crystallized bodies by Heat; by E. Mitscherlich. —Analysis of an antimony ore from Nasafjeld in Lapland; by M. C. J. Thaulow. (A notice of this will appear in the next Number of Phil. Mag.)

Wiegmann's *Archiv,* Part 3. Berlin. 1837.

Contents.—Description of two monstrous *Echini,* together with some general remarks on *Echinidæ*; by Dr. Philippi.—On *Gorgonia paradoxa*; by the same.—Contributions to the history of the genera *Campanularia* and *Syncoryne*; by Lowèn.—Ichthyological Appendices to the Fauna of Greenland; by Prof. Reinhard.—On the genera of Fossil Infusoria *Xanthidium* and *Peridinium*; by C. G. Ehrenberg.—On Fossil Infusoria; by the same.—Notice on the action of free Carbonic Acid on the nutriment of Vegetables; by Dr. M. J. Schleiden.—Diagnoses of some new species of Shells; by Anton.—Report on the Progress of Vegetable Physiology during the year 1836; by J. Meyen. (Translation now appearing in Phil. Mag.)

Neue Notizen von Froriep, vol. ii. contains;

No. 9. Effects of Galvanism on animal œconomy.—Climate of Scandinavia; by Forsell.—On the place which Dinotherium must occupy in the natural classification of Mammalia.—No. 10. Appendices to the natural history of Man; by Prof. J. van der Hoeven.—No. 14. New experiments on the action of the Torpedo; by Santi Linari.—Experiments on the Voice; by Bishop (from Phil. Mag.).—On Spermatozoa.—On the influence of aqueous vapour on all periods of Vegetation.

Journal für praktische Chemie, by O. L. Erdmann. Part 6. contains:

On the Silvering of Brass; by Dernen.—Sulphate of Zinc with 3½ M. G. of Water; by Anthon.—On the application of Metallic Sulphurets prepared in the moist way to chemical analysis; by the same.—Simple methods of determining Ammonia, both quantitative and qualitative, in chemical analysis; by the same.—Analysis of a Mineral water which had suddenly changed; by E. von Bibra.—On the Reduction of the Sulphuret of Arsenic.—Hints for the Improvement of the Quick Manufacture of Vinegar; by E. F. Anthon.—A new Method of preparing Iodic Acid; by Louis Thompson (from Phil. Mag.). On the Purple-red Colour produced by chloride of gold on animal fibre. On the æthereal Oil of Wine.—Enigmatical Phænomenon of a Mutation of Colours; by H. Ch. Creuzburg.

Comptes Rendus, No. 14. 1837. Oct. 2.

Contents.—On some fossil Teeth from Oran; by Duvernoy.—On the Geographical Distribution of Birds of Passage in South America; by A. d'Orbigny.—Recent Experiments on the Torpedo; by Matteucci.—Sulphate of Nitrogen; by Soubeiran.—On the Erratic Blocks of the Jura; by Agassiz.—Letter on the presence of Animalcules in various Secretions and Excretions of diseased patients; by Beauperthuy and Adet de Rouville. (Worthy of notice.) No. 15. Oct. 9.—Note on the Development of the Embryo in the cephalapodous Mollusca; by A. Dugès.—Researches in Animal Electricity; by Matteucci.—On some new Mammifera; by Jourdan.

LXVII. *Intelligence and Miscellaneous Articles.*

SOLUBILITY OF ARSENIOUS ACID.

Mr. Taylor, lecturer on chemistry at Guy's Hospital, has made numerous experiments on this subject, to the detail of which, as inserted in the following pages, he has prefaced the summary also annexed of the results obtained by other chemists.

" One thousand parts of temperate water dissolve, of their weight of arsenious acid, according to—

Despretz,	*La Grange,*	*Bucholz,*	*Guibourt,*	*Hahnemann,*
$\frac{1}{20}$	$\frac{1}{20}$	$\frac{1}{50}$	$\frac{1}{80}$	$\frac{1}{90}$

Spielmann,	*Ure,*	*Klaproth,*	*Fischer.*
$\frac{1}{96}$	$\frac{1}{333}$	$\frac{1}{400}$	$\frac{1}{1200}$

" The results of some of these experimentalists were probably obtained by boiling the water on arsenious acid, allowing the solution to cool, and then estimating the quantity dissolved; while those of others were probably deduced from the actual digestion of the poison in cold water. It is perhaps in this way that we may reconcile the enormous difference between the statements of Despretz and Fischer; the former making arsenic sixty times more soluble than the latter.

" One thousand parts of boiling water dissolve, of their weight of arsenious acid, according to

Guibourt,	*Bucholz,*	*Klaproth,*	*Ure,*	*La Grange,*	*De la Métherie,*
$\frac{1}{8}$	$\frac{1}{12}$	$\frac{1}{13}$	$\frac{1}{14}$	$\frac{1}{15}$	$\frac{1}{24}$

Vogel,	*Beaumé,*	*Navier,*	*Nasse,*
$\frac{1}{60}$	$\frac{1}{64}$	$\frac{1}{80}$	$\frac{1}{200}$

" The differences in this table may perhaps be explained, by supposing that a heat of 212° may have been applied for different periods of time; as also that specimens of arsenious acid, probably varying considerably in their degree of purity, may have been employed.

" It was the discovery of these very different results, respecting the solubility of arsenic, by men of well-known authority as chemists, that first induced me to endeavour to ascertain which statement was borne out by experiment.

" In relation to specific gravity, I found that of a mass of arsenious acid which had been kept four years, and was perfectly *opaque*—presenting, when fractured, a slightly crystalline structure—to be 3·529. Having procured a recently-prepared specimen, perfectly *transparent*, but of a slightly-yellowish tinge, I tried its specific gravity, and found it to be 3·798.

" Arsenious acid, it may be remarked, is soluble in water, oils, and alcohol. Water is its most common solvent: and it is, therefore, of its solubility in this menstruum that I shall first proceed to speak. The water employed in the experiments mentioned below, was the common water of the Hospital, which is the Thames water, filtered. It contains, comparatively, little foreign matter. A given measure of this water weighed 752·7 gr.; while the same measure of recently-distilled water weighed 752 gr. Its specific gravity will therefore be 1·00093. Distilled water was not employed in these experiments, since I had a medico-legal object in view: in no case of criminal poisoning is it likely that distilled water will be used by the suicide or murderer. In the course of many experiments, however, there did not appear to me to be the least appreciable difference in the solvent power of water over arsenious acid, whether distilled, or common river-water filtered, was employed.

" Exp. 1. Twenty grains of opaque arsenious acid, reduced to a fine powder, were placed in a clean glass vessel, and eight fluid ounces of *boiling water* were poured on. A portion of the powder collected into small lumps, which floated, and, even after violent agitation, adhered to the sides of the vessel; while another portion sank to the bottom. The vessel remained covered *seventy-two hours*; the contents being frequently agitated, to ensure perfect contact and admixture. The water was then carefully filtered, and the filter dried. The residuary undissolved powder weighed 10·46 gr. Therefore,

20—10·46=9·54 gr. dissolved by f ℥ viij. or (500 × 8) 4000 gr. water;
and 4000÷9·54=419; as also 9·54÷4=2·385 gr.

Hence,

1000 parts water at 212°, dissolved . . . . . 2·385 pts. or $\frac{1}{419}$.

"Exp. 2. A similar experiment was performed; and the residuary powder obtained on the filter weighed 9·27 gr. Therefore,

20—9·27=10·73, dissolved by f ℥ viij. or (500 × 8) 4000 gr. water;
and 4000÷10·73=372; as also, 10·73÷4=2·6825 gr.

Hence,

1000 parts water at 212° dissolved . . . . . 2·6825 gr. or $\frac{1}{372}$.

"Twenty-five grains of each of these solutions, filtered, were now evaporated to dryness, at a low temperature; and ·06 gr. were obtained as the mean weight of the residue of several successive evaporations of Exp. 1.; and ·07 as the mean for Exp. 2.; results which come as near to the proportions above ascertained as could be well expected, considering that distilled water was not employed.

"The mean of the Exps. 1 and 2 will be the following: 1000 gr. of boiling water, allowed to cool, and remain 72 hours (with frequent agitation), on 20 gr. arsenious acid, will dissolve 2·53 gr., or about $\frac{1}{395}$ of their weight.

"Exp. 3. Two ounces of water were kept *gently* boiling for *an hour*, the waste by evaporation being made up; and while boiling, finely powdered arsenious acid, in small quantities at a time, was gradually added, from a previously weighed quantity. No further portion was added until that which had been previously added was dissolved. The result was, that,

1000 gr. of water (f ℥ij.) dissolved . . . . 31·5 gr. or $\frac{1}{32}$.

This solution was placed aside for 72 hours; and at the end of that time it was found to have deposited in brown octohedral crystals, 14·5 gr.; and 31·5—14·5=17 gr. Hence,

1000 grains water (f ℥ij.) held dissolved, on perfect cooling, 17 grains, or $\frac{1}{58}$.

Twenty-five grains of the cold solution were slowly evaporated to dryness; and the mean of several evaporations gave ·41 gr. as a residue, which is a little below the proportion as above ascertained.

"Exp. 4. Two ounces of water were kept *violently* boiling for *an hour*, the waste by evaporation being made up; and arsenious acid was gradually added from a weighed quantity, as before. It was then found that

1000 gr. of water had dissolved . . . 46·3 or $\frac{1}{21}$.

From this solution there were deposited in crystals, after 72 hours, 21·6 gr. and 46·3—21·6=24·7 gr.

1000 gr. water, held dissolved, on perfect cooling, 24·7 gr. or $\frac{1}{40}$.

Twenty-five grains of the cold solution, evaporated, left ·55 gr., rather less than the proportion deduced from the weight of the undissolved residue.

"Exp. 5. In this experiment four ounces of water were kept *violently* boiling for *half an hour*; arsenious acid being added, as before, from a weighed quantity. 89 gr. were dissolved. Hence,

1000 gr. water dissolved $(89 \div 2)$ . . . 44·5 gr. or about $\frac{1}{22}$.

The mean of Exps. 3, 4, and 5, will be the following:

1000 gr. of boiling water dissolve . . . . 40·76 gr. or $\frac{1}{24}$.

The *rapidity* of boiling will make a considerable difference in the quantity dissolved, as will be seen on comparing Exps. 3 and 4. Indeed, water which boils violently will dissolve as much arsenic in half an hour as water kept gently boiling will dissolve in an hour. All other circumstances being equal, the *length of time* during which the boiling continues will assuredly make a difference in the quantity of the poison taken up.

"Exp. 6. Arsenious acid, in fine powder, was boiled for several hours to saturation, in two separate quantities of water. These solutions were, after filtration, kept apart; and allowed to stand for *six months* in well-stoppered bottles. A very abundant crop of octohedral crystals, lining the whole interior of the bottle, was deposited in each case. After this lapse of time, twenty-five grains of the filtered solution (A) were evaporated to dryness; and the solid residue weighed ·7 grain. Hence,

1000 gr. of the solution contained $40 \times {\cdot}7 = 28{\cdot}$ gr. or $\frac{1}{35}$.

Twenty-five grains of (B) left, as a solid residue, ·6 gr. Hence,

1000 gr. held dissolved $40 \times {\cdot}6 = 24$ gr. or nearly $\frac{1}{42}$.

The mean of these two experiments will be as follows:

1000 gr. of a saturated solution, after six months' standing, held dissolved 26 gr. or $\frac{1}{38}$.

"In closing these remarks on the solubility of arsenic in *boiling* water, I shall subjoin the results of some experiments on the recently-prepared, or transparent, arsenious acid: and I am the more desirous of doing this, since the statements of Guibourt, relative to the transparent being *less soluble* than the opaque variety, are not supported by them. In a medico-legal point of view, the question of a difference of solubility in these varieties of arsenious acid is not, perhaps, of much importance; since the pure transparent arsenic is with difficulty obtainable, and is rarely sold by druggists.

"Exp. 7. A perfectly *transparent* and recently-prepared mass of arsenious acid was finely pulverized; and a weighed quantity of the powder was gradually added to two fluid ounces of water, kept *violently* boiling for *an hour*, the waste by evaporation being made up. Forty-six grains were dissolved.

1000 gr. water dissolved . . . . 46 gr. or nearly $\frac{1}{21}$.

From this solution there were deposited in crystals, after 48 hours, 27·3 gr. and $46 - 27{\cdot}3 = 18{\cdot}7$.

1000 gr. water held dissolved, on perfect cooling . . . . 18·7 gr. or $\frac{1}{53}$.

"Exp. 8. In this experiment four ounces of water were kept boiling for *an hour*, and pulverized *transparent* arsenic was gradually added. There were dissolved 95·1 gr. Hence,

1000 gr. water dissolved (95·1÷2) . . . . 47·55, or $\frac{1}{21}$.

From this solution there were deposited, in crystals, after 48 hours, 68·3 gr. Hence, 95·1—68·3=26·8÷2=13·4 gr.

1000 grains of water, held dissolved, on perfect cooling, 13·4 grains, or $\frac{1}{71}$.

The results of these experiments show, that there is certainly *not always* the difference in the degree of solubility of these two varieties of arsenic, which M. Guibourt suspected; and which, upon his authority, is to be found stated in many chemical and medico-legal works. The quantity dissolved, of either variety, under similar circumstances, according to these experiments, may be regarded, for all practical purposes, as the same.

" In cases of criminal poisoning, it occasionally happens that *cold* or *temperate water* is used as the solvent for this poison, and a witness is expected to state the degree of its solvent powers. The consideration of this, led to the performance of the following additional experiments.

" Exp. 9. Eight fluid ounces of *temperate* water were poured upon twenty grains of the pulverized opaque arsenic, in a clean glass vessel. The powder immediately collected in lumps, which partly floated and partly remained at the bottom of the vessel. A slight film of powder formed by repulsion on the surface of the water. The vessel was covered over, and allowed to stand 72 hours, having been first well agitated. The liquid was filtered, and the filter carefully dried. The residual undissolved powder weighed 16 gr. And 20—16=4. Hence,

1000 gr. water (f℥ij.), dissolved . . . . 1 gr. or $\frac{1}{1000}$.

Twenty-five grains of the filtered solution, evaporated, left ·03 gr.; which is nearly equal to the proportion above determined from the undissolved residue.

" Exp. 10. Another experiment was performed, which only differed from the preceding in the circumstance of the vessel having been *frequently agitated*. The undissolved powder left on the filter, after drying, weighed 11·5 gr., and 20—11·5=8·5 gr. Hence,

1000 gr. water (f℥ij.), dissolved . . . . 8·5÷4=2·125 or $\frac{1}{470}$.

Twenty-five grains of the filtered solution were evaporated; and left not quite ·06 gr., a proportion rather larger than that above deduced.

" It follows, from these experiments, that very nearly the same quantity of arsenious acid is taken up by hot water allowed to cool, and cold water poured on this substance in powder; provided the vessel containing the cold water be frequently agitated. They also show the necessity for a continued application of heat, in order that the poison should be dissolved in any considerable quantity. It is a curious, and hitherto an unexplained fact, that water should retain so much more of this poison, as from ten to twenty times the quantity, when *perfectly cooled* from a boiling saturated solution, than it will take up at common temperatures without heat. It would seem to indicate, that heat must excite some permanently powerful affinity between the particles of arsenious acid and water, which did not previously exist."

Note. It remains to be explained why 1000 grains of a saturated and cold solution, as in Experiment 6, contained 26 grains of arsenious acid, whereas by Experiment 7, it appears that an equal quantity of a solution also saturated and cold contained only 18·7 grains.—R.P.

STEARIC ÆTHER AND STEARATE OF METHYLENE.

M. Lassaigne obtained the above-named compounds by treating stearic acid with a mixture of alcohol and sulphuric acid, or by a mixture of the same acid with pyroxilic spirit.

Stearic æther has the following properties: it is solid, white, and semi-transparent like wax; it is lighter than water; its odour is not very marked, but is slightly ethereal; it is tasteless, and does not act upon litmus paper. The fusibility of this compound is so great that it melts when pressed between moderately warm fingers, or when rubbed in the palm of the hand; it is insoluble in water, but soluble in alcohol, and more so when hot than cold. When treated with a hot solution of potash, it gradually decomposes, and there are reproduced stearic acid, which remains combined with the potash, and alcohol, which is disengaged with the vapour of water. It appears from analysis that this æther consists of

Stearic acid	87·91
Æther..................	12·09
	100·

or an atom of stearic acid combined with one of æther.

The stearate of methylene, prepared by heating stearic acid with a mixture of sulphuric acid and pyroxilic spirit, is solid, and lighter than water; it is a confusedly crystalline mass, which is yellowish and semi-transparent; its smell is very slight. It softens between the fingers, and soon fuses; its melting point is about 90° Fahr. It does not act upon litmus, is insoluble in water, and decomposed by hot alkaline solutions. This compound, with respect to the relation existing between its elements, seems to approach the oxalate of methylene and analogous compounds.—*L'Institut; Août*, 1837.

METEOROLOGICAL OBSERVATIONS FOR SEPTEMBER 1837.

Chiswick.—Sept. 1. Cloudy: thunder showers. 2. Cloudy and fine. 3. Showery. 4. Cloudy: clear and cold at night. 5. Hazy: cloudy and fine. 6. Foggy: very fine. 7. Fine. 8. Rain: sultry. 9, 10. Cloudy. 11. Fine. 12—14. Cloudy and fine. 15. Clear. 16—18. Overcast. 19, 20. Very fine. 21, 22. Foggy: very fine. 23, 24. Dry and windy. 25, 26. Clear and fine. 27, 28. Cloudy and fine. 29, 30. Foggy: very fine.

Boston.—Sept. 1. Fine. 2. Fine: rain P.M. 3. Cloudy: rain A.M. and P.M. 4. Cloudy. 5, 6. Fine. 7. Cloudy. 8. Fine. 9. Cloudy: heavy rain P.M. 10. Fine. 11. Fine: rain P.M. 12. Fine. 13. Cloudy: rain early A.M. 14. Cloudy: rain A.M. 15. Fine: rain P.M. 16. Cloudy: rain P.M. 17. Cloudy. 18. Cloudy: rain early A.M.: rain A.M. 19—21. Cloudy. 22—24. Fine. 25. Fine: rain A.M. 26. Fine. 27—29. Cloudy. 30. Fine.

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

Days of Month. 1837. Sept.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Max.	Self-registering. Min.	Max.	Min.								
F. 1.	29·368	29·400	29·361	28·90	56·3	62·3	50·7	62	46	55·5	W.	W.	calm	...	·02	...	53
S. 2.	29·444	29·590	29·423	29·03	55·4	62·0	51·2	62	47	59	N.	N.	calm	·091	·07	...	52
⊙ 3.	29·666	29·797	29·644	29·11	53·4	61·2	50·3	58	49	55	W.	N.	N.	·072	·10	·03	49
M. 4.	29·920	29·925	29·910	29·46	53·0	57·3	51·2	59	40	54	WNW.	N.	N.	·055	...	·24	50
T. 5.	29·864	30·003	29·851	29·40	56·6	59·7	49·3	60	39	56·5	E.	NE.	calm	...	·02	...	52
W. 6.	30·060	30·056	30·033	29·55	52·0	61·6	47·6	67	41	54	N.	SW.	calm	...	...	...	50
☽ Th. 7.	29·946	29·940	29·844	29·34	59·3	61·6	51·3	67	43	57·5	S.	S.	W.	...	·20	...	52
F. 8.	29·930	29·899	29·807	29·27	55·5	66·0	54·5	68	50	58	SE.	S.	W.	·158	...	...	54
S. 9.	29·872	29·865	29·754	29·32	60·8	62·2	55·5	70	51	55	SSE.	S.	calm	...	...	...	55
⊙ 10.	29·916	29·931	29·860	29·21	60·2	67·3	54·7	70	52	59	SSW.	SW.	W.	·055	·26	·53	56
M. 11.	29·822	29·814	29·726	29·12	62·5	68·0	56·2	69	45	61	SE. var.	S.	W.	...	...	...	59
T. 12.	29·748	29·747	29·384	29·10	54·4	67·0	52·0	66	47	52	SW.	SE.	W.	·277	·09	·33	55
W. 13.	29·154	29·162	29·071	28·54	59·8	61·6	52·2	65	48	59	S.	SW.	W.	·052	·09	·18	58
○ Th. 14.	29·354	29·553	29·331	28·72	56·0	64·0	53·0	61	44	52	NNW.	W.	NW.	·069	·02	...	53
F. 15.	29·634	29·871	29·616	29·02	55·5	59·5	49·8	62	40	53	NW.	NW.	W.	·055	...	·03	52
S. 16.	30·004	29·996	29·984	29·40	55·0	60·0	48·5	70	56	52	SE. var.	S.	calm	·011	...	·05	52
⊙ 17.	30·058	30·086	30·067	29·33	65·5	66·0	56·0	72	60	61	W. var.	SW.	calm	...	...	·05	58
M. 18.	30·052	30·032	30·016	29·24	64·6	69·7	61·5	67	47	63	SSW.	SW.	W.	...	·01	·10	60
T. 19.	30·170	30·144	30·091	29·48	63·4	68·3	58·8	72	61	60	SSE.	S.	calm	...	...	·09	60
W. 20.	30·028	30·005	29·911	29·36	66·3	68·5	61·2	72	48	61	F.	SE.	E.	...	...	...	61
☾ Th. 21.	29·934	30·012	29·913	29·31	59·2	68·4	54·3	69	49	58·5	NE.	SE.	E.	...	...	...	56
F. 22.	30·086	30·065	30·041	29·46	60·5	68·7	53·2	67	51	62	NE.	E.	E.	...	...	...	58
S. 23.	30·144	30·135	30·110	29·49	58·3	65·2	52·9	63	47	59	E.	E.	E.	...	...	...	55
⊙ 24.	30·242	30·292	30·220	29·65	56·7	61·2	51·8	61	37	57	NE.	E.	NE.	...	...	...	50
M. 25.	30·320	30·313	30·305	29·73	56·2	58·4	46·5	61	40	57	N.	NE.	E.	...	...	...	51
T. 26.	30·302	30·304	30·204	29·75	54·2	59·3	46·4	62	35	54	NE.	NE.	calm	...	·01	·07	50
W. 27.	30·116	30·124	30·077	29·58	51·2	60·2	44·4	60	49	55	N.	E.	calm	...	...	...	49
Th. 28.	30·076	30·083	30·059	29·55	53·4	60·2	44·4	63	35	53	N.	NE.	calm	...	...	...	50
● F. 29.	30·064	30·068	30·019	29·50	54·9	60·2	44·4	62	42	55	NE.	E.	calm	...	...	...	50
S. 30.	29·972	29·969	29·946	29·40	57·3	59·6	48·0	66	49	58	NE.	E.	E.	...	·02	...	51
	29·909	29·939	29·852	29·31	57·6	63·2	51·7	65·1	46·3	56·8				Sum ·895	0·91	1·70	53·7

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LXVIII. *On the Action of Chromic Acid upon Silver, and its Combinations with the Oxide of that Metal.* By R. WAZINGTON, *Esq.*[*]

WHEN liquid sulphuric acid is added to a solution of the bichromate of potass and a plate of silver immersed, or the mixed solutions poured into a silver capsule, an immediate chemical action takes place, and the whole surface of the metal becomes covered with a scarlet-coloured precipitate, which in a very short time assumes a crystalline structure, and is changed to a dark crimson; this salt is the bichromate of silver, as will be demonstrated by analysis in a subsequent part of this paper. The precipitate should be removed at intervals to facilitate the progress of the action, as it would otherwise form a coating over the surface of the silver and prevent the acid from acting with its full energy. During this operation the colour of the supernatant liquid becomes much deeper, until the orange red tint of the original solution has become of a dark mahogany hue, and it then passes gradually into a deep green. This solution yields by evaporation one or two salts that will be hereafter described, and a large quantity of very fine octohedral crystals of the double sulphate of the protoxide of chromium and potass, or chrome alum.

Now to account for these phænomena, it must be evident both from the nature of the materials employed, and also of the compounds arising from their action, that the silver must become oxidized (this being necessary before any combination can take place between it and the chromic acid) at the ex-

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[*] Communicated by the Author.

92X

pense of the chromic acid of the bichromate of potass, and that the chromic acid must therefore be divided into two parts, the first of which, yielding oxygen to the metallic silver, forms the oxide of that metal, which is directly seized by the second, or undecomposed part of the chromic acid, giving rise to the crimson crystalline salt or bichromate of silver; while at the same time the deoxidized chromic acid or protoxide of chromium of the first part, enters into combination with the sulphuric acid and the potass of the bichromate of potass, to form the chrome alum; the sulphuric acid taking no part in the decomposition, but acting merely as a necessary agent to induce the stronger affinities, and to unite with the protoxide of chromium and the potass as soon as liberated from their respective combinations.

For the perfect success of this experiment care should be taken that the plate or capsule of silver employed is rendered perfectly clean, and free from any coating of sulphuret or chloride of that metal. I have usually, after rubbing the surface with a little fine emery powder, washed it with solution of ammonia before bringing it into contact with the mixed solution, and have always found the action to commence immediately. The proportions that appear to afford the most complete results are one proportion of the bichromate of potass, or about 150 grains, dissolved in water, and three or four proportions of liquid sulphuric acid, or from 150 to 200 grains by weight, equal to from 76 to 102 grains by measure.

The bichromate of silver crystallizes in rhomboidal plates, having frequently two of the angles opposed to each other truncated; it has an acid reaction, is slightly soluble in water, yielding a rich amber yellow solution, which deposits very dark brown crystals by spontaneous evaporation; these however are crimson by transmitted light, and when rubbed down in a mortar give a crimson powder.

The bichromate of silver is also formed whenever an acid salt of silver is precipitated by the bichromate of potass, and even at times by the neutral chromate, as is the case with the sulphate of silver; and although the fact of a crystalline precipitate of chromate of silver being sometimes obtained is mentioned in many of the best works on chemistry, yet the composition, or a difference in composition from the brown-red precipitate of chromate of silver as it usually occurs, is never hinted at. In making this statement, however, I should except the *Traité des Essais par la voie sèche* of M. Berthier, and also a short notice by Mr. Teschemacher in the Phil. Mag. and Annals, N.S. vol. i. p. 345.

20 grains of the bichromate of silver obtained by the pro-

cess given in the former part of this paper and reduced to a fine powder, were acted upon by very dilute hydrochloric acid and boiled; this threw down chloride of silver, which when well aggregated by the boiling was thrown upon a double filter and carefully washed, and gave, after being dried and heated to incipient fusion, 13·1 grains, equivalent to 10·5 grains of the oxide of silver.

To the filtered solution a small quantity of alcohol was added, and the whole boiled until a perfect deoxidation of the chromic acid had taken place; the alcohol was then evaporated off, and the protoxide of chromium precipitated by a solution of caustic ammonia added in slight excess; this after being well washed, dried, and heated to full redness, weighed 7·2 grains, which are equivalent to 9·36 grains of chromic acid. We have therefore the following proportions for the composition of this salt, corresponding as closely as we can expect with the theoretical constitution of the bichromate of silver.

	By Experiment.	By Theory.	Atoms.	Atomic Weight.
Chromic acid	9·36	9·455	2	104
Oxide of silver	10·59	10·545	1	116
	19·95	20·000		220.

When the above salt is boiled in distilled water a part of it is dissolved, and separates in beautiful micaceous crystals of a very rich crimson colour as the solution cools, while at the same time another part of the salt is resolved into chromic acid and a very dark green crystalline chromate of silver, crimson however by transmitted light, and yielding by trituration a powder similar in colour to the precipitated chromate. This after being well washed and dried was submitted to exactly the same method of analysis as the bichromate of silver, (as were also the two salts next mentioned,) and gave from 2·1 grains.

	By Experiment.	By Theory.	Atoms.	Atomic Weight.
Chromic acid	·646	·65	1	52
Oxide of silver	1·440	1·45	1	116
	2·086	2·10		168

The precipitated chromate, obtained by adding a solution of the yellow chromate of potass to one of the nitrate of silver, yielded exactly the same results.

The bichromate of silver is readily soluble in ammonia, affording a clear pale yellow solution, which by exposure to the atmosphere has a dark green pellicle or crust formed on its surface, having a metallic lustre, and being of a rich claret colour by transmitted light, giving however the same kind of red-brown powder by trituration, and having exactly the composition of the two preceding salts. After this green crust has been separated, crystals of chromate of silver and ammonia of a pale yellow colour form (this salt has been carefully examined by Professor Mitscherlich), and lastly bichromate or chromate of ammonia separates.

Should it be wished to obtain the double chromate of silver and ammonia, care should be taken to exclude the air, as the ammonia rapidly passes off and destroys the object in view. I have found that when placed in a partial vacuum over quicklime, an atmosphere of ammonia is formed around the solution, while at the same time the lime is gradually absorbing the watery vapour.

In a future communication I hope to consider the action of chromic acid upon some of the other metallic bodies, as well as to recite an examination of a variety of double chromates formed during that action.

LXIX. *On the Variation of the Arbitrary Constants in Mechanical Problems.* *By* J. W. Lubbock, *Esq., F.R.S.*[*]

THE general theory of the variation of the arbitrary constants in mechanical problems, with reference to the differential coefficients of the disturbing function considered as a function of those coefficients, as hitherto exposed, appears susceptible of several simplifications.

Suppose the differential equations of any dynamical problem to be

$$\frac{d^2 x}{d t^2} + \frac{d V}{d x} + \frac{d R}{d x} = 0 \qquad x' = \frac{d x}{d t}$$

$$\frac{d^2 y}{d t^2} + \frac{d V}{d y} + \frac{d R}{d y} = 0 \qquad y' = \frac{d y}{d t}$$

$$\frac{d^2 z}{d t^2} + \frac{d V}{d z} + \frac{d R}{d z} = 0 \qquad z' = \frac{d z}{d t},$$

and that a, b, c, &c. are constants which enter into the accurate integrals of the equations

$$\frac{d^2 x}{d t^2} + \frac{d V}{d x} = 0, \qquad \frac{d^2 y}{d t^2} + \frac{d V}{d y} = 0, \qquad \frac{d^2 z}{d t^2} + \frac{d V}{d z} = 0.$$

This *general* theory of the variation of the arbitrary constants in mechanical problems may be said to consist in the following theorems;

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[*] Communicated by the Author.

$$\text{I. } \frac{dR}{da}\,dt = (b,a)\,db + (c,a)\,dc + (e,a)\,de + \&\text{c.}$$

$$\text{II. } da = [b,a]\frac{dR}{db}\,dt + [c,a]\frac{dR}{dc}\,dt + [e,a]\frac{dR}{de}\,dt + \&\text{c.}$$

with similar equations for the constants b, c, e, &c. (b, a), (c, a), (e, a), &c. $[b, a]$, $[c, a]$, $[e, a]$, &c. being the same as in the notation of M. Poisson, *Mémoires de l'Institut*, 1816.

III. That the quantities (b, a), (c, a), (e, a), &c. and also the quantities $[b, a]$, $[c, a]$, $[e, a]$, &c. are constant.

The well-known proof which M. Poisson has given of the expressions which constitute the IInd theorem appears as simple and direct as can be desired. See the *Théorie Analytique du Système du Monde*, tom. i. p. 222. I shall begin by proving the Ist theorem.

Considering R as a function of x, y, z, x', y', z',

$$\frac{dR}{da}\,dt = \frac{dR}{dx}\,\frac{dx}{da}\,dt + \frac{dR}{dy}\,\frac{dy}{da}\,dt + \frac{dR}{dz}\,\frac{dz}{da}\,dt$$
$$+ \frac{dR}{dx'}\,\frac{dx'}{da}\,dt + \frac{dR}{dy'}\,\frac{dy'}{da}\,dt + \frac{dR}{dz'}\,\frac{dz'}{da}\,dt.$$

But if, as in the planetary theory, &c., R does not contain x', y', z' explicitly,

$$\frac{dR}{dx'} = 0 \qquad \frac{dR}{dy'} = 0 \qquad \frac{dR}{dz'} = 0$$

$$\frac{dR}{da}\,dt = \frac{dR}{dx}\,\frac{dx}{da}\,dt + \frac{dR}{dy}\,\frac{dy}{da}\,dt + \frac{dR}{dz}\,\frac{dz}{da}\,dt \qquad (1.)$$

The following equations result from the usual conditions, namely, that the expressions $\frac{dx}{dt}, \frac{dy}{dt}, \frac{dz}{dt}$ continue *in form* the same in disturbed as in the undisturbed motion. See the *Mécanique Anal.*, tom. i. p. 330.

$$\frac{dx}{da}\,da + \frac{dx}{db}\,db + \frac{dx}{dc}\,dc + \frac{dx}{de}\,de + \&\text{c.} = 0 \qquad (2.)$$

$$\frac{dy}{da}\,da + \frac{dy}{db}\,db + \frac{dy}{dc}\,dc + \frac{dy}{de}\,de + \&\text{c.} = 0 \qquad (3.)$$

$$\frac{dz}{da}\,da + \frac{dz}{db}\,db + \frac{dz}{dc}\,dc + \frac{dz}{de}\,de + \&\text{c.} = 0 \qquad (4.)$$

Also,

$$\frac{dx'}{da}\,da + \frac{da'}{db}\,db + \frac{dx'}{dc}\,dc + \frac{dx'}{de}\,de + \&\text{c.} = -\frac{dR}{dx}\,dt \qquad (5.)$$

$$\frac{d\,y'}{d\,a}\,d\,a+\frac{d\,y'}{d\,b}\,d\,b+\frac{d\,y'}{d\,c}\,d\,c+\frac{d\,y'}{d\,e}\,d\,e+\&\text{c.}=-\frac{d\,R}{d\,y}\,d\,t \qquad (6.)$$

$$\frac{d\,z'}{d\,a}\,d\,a+\frac{d\,z'}{d\,b}\,d\,b+\frac{d\,z'}{d\,c}\,d\,c+\frac{d\,z}{d\,e}\,d\,e+\&\text{c.}=-\frac{d\,R}{d\,z}\,d\,t \qquad (7.)$$

Multiplying (2) by $\frac{d\,x'}{d\,a}$, (3) by $\frac{d\,y'}{d\,a}$, (4) by $\frac{d\,z'}{d\,a}$, and adding the equations so formed to equation 1, after having substituted in (1.) the values of $\frac{d\,R}{d\,x}$, $\frac{d\,R}{d\,y}$ and $\frac{d\,R}{d\,z}$ from (5), (6) and (7), I obtain the equation

$$\begin{aligned}
\frac{d\,R}{d\,a}\,d\,t=&\left\{\frac{d\,x}{d\,a}\,\frac{d\,x'}{d\,a}-\frac{d\,x'}{d\,a}\,\frac{d\,x}{d\,a}+\frac{d\,y}{d\,a}\,\frac{d\,y'}{d\,a}-\frac{d\,y'}{d\,a}\,\frac{d\,y}{d\,a}\right.\\
&\qquad\left.+\frac{d\,z}{d\,a}\,\frac{d\,z'}{d\,a}-\frac{d\,z'}{d\,a}\,\frac{d\,z}{d\,a}\right\}d\,a\\
&+\left\{\frac{d\,x}{d\,b}\,\frac{d\,x'}{d\,a}-\frac{d\,x'}{d\,b}\,\frac{d\,x}{d\,a}+\frac{d\,y}{d\,b}\,\frac{d\,y'}{d\,a}-\frac{d\,y'}{d\,b}\,\frac{d\,y}{d\,a}\right.\\
&\qquad\left.+\frac{d\,z}{d\,b}\,\frac{d\,z'}{d\,a}-\frac{d\,z'}{a\,b}\,\frac{d\,z}{d\,a}\right\}d\,b\\
&+\left\{\frac{d\,x}{d\,c}\,\frac{d\,x'}{d\,a}-\frac{d\,x'}{d\,c}\,\frac{d\,x}{d\,a}+\frac{d\,y}{d\,c}\,\frac{d\,y'}{d\,a}-\frac{d\,y'}{d\,c}\,\frac{d\,y}{d\,a}\right.\\
&\qquad\left.+\frac{d\,z}{d\,c}\,\frac{d\,z'}{d\,a}-\frac{d\,z'}{d\,c}\,\frac{d\,z}{d\,a}\right\}d\,c\\
&+\&\text{c.}\\
&=(b,a)\,d\,b+(c,a)\,d\,c+(e,a)\,d\,e+\&\text{c.} \qquad (\text{I.})
\end{aligned}$$

which is Lagrange's theorem. Again,

$$\begin{aligned}
\frac{d\,V}{d\,a}&=\frac{d\,V}{d\,x}\,\frac{d\,x}{d\,a}+\frac{d\,V}{d\,y}\,\frac{d\,y}{d\,a}+\frac{d\,V}{d\,z}\,\frac{d\,z}{d\,a}\\
&=-\frac{d\,x'}{d\,t}\,\frac{d\,x}{d\,a}-\frac{d\,y'}{d\,t}\,\frac{d\,y}{d\,a}-\frac{d\,z'}{d\,t}\,\frac{d\,z}{d\,a} \qquad (8.)
\end{aligned}$$

Similarly

$$\frac{d\,V}{d\,b}=-\frac{d\,x'}{d\,t}\,\frac{d\,x}{d\,b}-\frac{d\,y'}{d\,t}\,\frac{d\,y}{d\,b}-\frac{d\,z'}{d\,t}\,\frac{d\,z}{d\,b} \qquad (9.)$$

differentiating (8) with regard to b and (9) with regard to a, and subtracting one result from the other, M. Cauchy obtains the equation

$$\frac{d\,(b,a)}{d\,t}=0 \qquad (b,a)=\text{constant.}$$

See Liouville's *Journal*, Oct. 1837, p. 407.

Finally, if in the equations

$$d a - [b, a] \frac{d R}{d b} d t - [c, a] \frac{d R}{d c} d t + \&c. = 0,$$

the values of $\frac{d R}{d b} d t$, $\frac{d R}{d c} d t$, &c. are substituted from the equations which constitute the Ist theorem, viz.

$$\frac{d R}{d b} d t = (a, b) d a + (c, b) d c + (c, b) d e + \&c.$$

$$\frac{d R}{d c} d t = (a, c) d a + (b, c) d b + (e, c) d e + \&c.,$$

the resulting equation must be identically equal to zero; and equating to zero separately the coefficients of $d a$, $d b$, $d e$, &c. in this equation, I obtain the equations

$$1 = [b, a] (a, b) + [c, a] (a, c) + [a, c] (a, e) + \&c. \qquad (10.)$$

$$[c, a] (b, c) + [c, a] (b, e) + \&c. = 0 \qquad (11.)$$

$$[b, a] (c, b) + [c, a] (c, e) + \&c. = 0 \qquad (12.)$$

These equations are given by M. Cauchy in Liouville's *Journal de Mathématiques*, 1837, p. 409; but the proof which M. Cauchy has there offered is in my opinion by no means so simple as the foregoing. These equations serve to connect the quantities $[b, a]$, $[c, a]$, &c. with the quantities (b, a), (c, a), &c., so that by elimination the one may be deduced from the other, and they serve to show that these quantities $[b, a]$, $[c, a]$, &c. are also constant, and independent of the time.

The reasoning which is required to establish the principal theorems I, II and III, is the same in the case of only one variable x, and of one differential equation, with two arbitrary constants a and b, $\frac{d^2 x}{d t^2} + \frac{d V}{d x} + \frac{d R}{d x} = 0$ as in the more general case of three variables x, y, z, with six arbitrary constants.

I have written this short paper under the impression that the proof here given of Lagrange's theorem (Theorem I., p. 494,) and the proof given here of equations (10), (11), and (12), have not been presented before; but these methods are extremely simple that it is by no means unlikely that they have occurred to others who may also have sought to bring this important theory within a narrower compass and within the reach of more elementary considerations than those hitherto employed.

LXX. *Remarks on* M. Mossotti's *Theory of Physics, suggested by* Mr. Babbage's *notice of the same. By Mr.* Thomas Exley, *M.A.**

THERE is most certainly one theory of physics, and only one; to determine this has always been the aim of philosophers. Innumerable have been the theories proposed, and as often have they been refuted, or at least rendered doubtful, till Newton unfolded the laws of gravitation; his doctrine has stood every test, and cannot be overthrown: but his theory of gravitation does not embrace molecular action. Many have attempted the generalization without success; and this paper is intended to show that even the theory lately proposed by Mossotti†, or any similar theory, which continues the direction of the forces the same to the centres of atoms, cannot be true. This so far clears the way for my own, which the more I examine the more I am convinced is the true one; which also it is desirable to corroborate. Because my theory is in conformity with received principles, with the single difference of a change of direction of the force, near the centre, and because no phænomenon can be found in contradiction to its doctrines, it has everything in its favour which, previously to its adoption, any theory can have. But since Mossotti's theory has excited much attention, and he has endeavoured to support it by mathematical reasonings, I have considered it incumbent on me to investigate its merits, especially as it has been particularly noticed by so great a name as Mr. Babbage.

While reading note A in Mr. Babbage's Ninth Bridgewater Treatise, it was gratifying to observe, that a train of thoughts had occupied my own mind, while writing my New Theory of Physics eight years ago, very similar to that which runs through the note mentioned.

Mr. Babbage has given a clear and concise statement of Mossotti's theory, and several judicious observations on what a physical theory ought to comprehend; and as I wish to make some remarks on that theory, with notices respecting my own, I beg to present some corresponding sentences from that note and my own treatise, as the reasons of such remarks; first stating that Mossotti has, in behalf of his theory, introduced some profound mathematical investigations: but we must be aware that such abstruse processes are not to be regarded as

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* Communicated by the Author.

† A notice of M. Mossotti's views will be found in Lond. & Edinb. Phil. Mag., vol. x. p. 320; and a translation of his entire paper in Scientific Memoirs, vol. i. p. 448.—Edit.

completely conclusive, till established at least in some remarkable particular cases, as is allowed on all hands, on account of the hypotheses, substitutions, and omissions of small quantities in the progress of the reasonings.

The theory is this: M. Mossotti supposes "matter to consist of two sorts, each of which repels its own particles, directly as the mass, and inversely as the squares of their distances, while each attracts those of the other kind according to the same law." Babbage, p. 164.

Mossotti supposes the forces to be directed towards central solids, which he assumes to be small and spherical; he also considers one of the two sorts to be diffused through universal space, and this sort he calls æther, its elements Mr. Babbage calls atmospheric particles; and the other sort, of which common bodies are formed, Mossotti calls molecules.

The two following principles comprehend my theory; viz. 1st. Every atom of matter consists of an indefinite sphere of force, which varies inversely as the square of the distance from the centre; and this force acts *towards* the centre, and is called attraction, at all distances, except in a small concentric sphere, in which it acts *from* the centre, and is there called repulsion. 2nd. The differences and distinctions of atoms arise from differences in their absolute forces, and in the radii of their spheres of repulsion.

According to this theory central solids are unnecessary. It was observed by the venerable Dr. Dalton (August 23rd, 1836) in a discussion of the subject at the meeting of the British Association in Bristol, that he could not give up the idea of solid atoms: my reply was, the theory does not require it, provided we may have the solids as small as we please; in that case they can do neither good nor harm. A very learned and universally admired professor of Cambridge, who honoured my paper with some judicious remarks, previously to its being read on that occasion, objected that "it would be impossible to identify such a repulsive force with the attractive force, because the law of continuity would be violated." Now I humbly conceive we do not in general determine the identity of a force by its direction, but rather by the law of its action, which in these principles is continuous. Why may not this change be at once? It must appear that all who admit alternations of attraction and repulsion, must admit such violation, if it be one, of continuity; for the change of direction is equally so, whether it be from a finite to a finite, or from an infinitely small attraction to an infinitely small repulsion.

It is manifest that this theory provides for an infinity of sorts of matter, and that the centres of atoms can in no case coincide. Various sorts of atoms according to the above principles may be assumed, and calculations made to deduce the phænomena *à priori*; but in order that such phænomena may agree with those actually observed, we must be guided in such assumptions by the indications of nature: on this ground I have admitted three classes: 1st. Tenacious atoms, distinguished into sorts by moderate differences in their absolute forces and spheres of repulsion. 2nd. Electrical atoms, being such as have a much less absolute force, say a thousand times less than the least powerful of the first class. 3rd. Æthereal atoms, whose absolute forces may be several million times less than those of the 2nd class, and like the first consisting of several sorts. If we suppose a due quantity of each variety present, it is manifest that the atoms of the first class will attract to their spheres of repulsion dense atmospheres of electric and æthereal atoms, whose centres will be found within the spheres of repulsion of contiguous atoms: and such a body as the earth will retain an atmosphere of such matter if present extending several hundred miles, giving rise to such repulsions as are observed.

Mr. Babbage has presented Mossotti's theory under a modified form in note A: he says, "If matter be supposed to consist of two sorts of particles, or rather centres of force, of different orders of density, and if the particles of each sort repel their own particles according to some given law, but attract particles of the other kind according to another law, then if we conceive only one particle of the denser kind to exist, and an infinite number of the other kind, that single particle will become the centre of a system surrounded by all the others, which will form an atmosphere around it, denser nearer the central body."

Mr. Babbage observes, 1st. That one particle of the denser kind will form around it an atmosphere of the other kind, when such are present. 2nd. That a stream of the atmospheric particles being added will increase its magnitude, and produce undulations, which will continue till it is increased to a certain extent, when it will begin to radiate. 3rd. "If the whole space in which such a central particle is placed, is itself full of atmospheric particles, then their density will increase in approaching the central body." 4th. "If a stream of such particles were directed towards the centre, they might produce throughout the atmosphere vibrations, which would be transmitted from it in all directions." 5th. "If two such central particles, with their atmospheres, exist at a distance from each other, they will be drawn together by a force depending on the *difference* between the mutual repulsions." Pages 164, 165, 166.

The following, extracted from my New Theory, nearly correspond: 1st. The electric fluid and æthereal matter form atmospheres about the tenacious atoms. Prop. 10. cor. 5—10. 2nd. The centres of two tenacious atoms being fixed near each other, when æthereal matter is added to one of them, its atmosphere will be increased to a certain degree, and then transfers will begin to be made to the other, and vibrations will be produced. Prop. 14. cor. 4. 3rd. Two or more tenacious atoms being put into a space containing a large collection of æthereal atoms, will accumulate atmospheres. And when an atmosphere is increased, it will not only be more extended, but its density at the same distance will be augmented. Prop. 10, 11, and cor. 5. 4th. Vibrations will be produced in bodies perpendicular to their surfaces, when æthereal matter is added. And in the composition of bodies multitudes of æthereal particles will be projected with great force, (will radiate,) and because of their own minute force, they will move with very great velocity, as is known in caloric and light. Pages 18, 19, and 63. 5th. The actions of the æthereal atoms, although in some cases tending to preserve, will in many cases tend to separate the tenacious atoms. Prop. 15. cor. 1.

From these quotations it will be seen that I have considered my theory as possessing all the requisites above stated and they may be fairly deduced from them; but this cannot hold in Mossotti's theory. For let a be the sum of the forces in two molecules A and B, and let the effective quantity of matter in the atmosphere of A be equivalent to a quantity of the same matter placed at its centre, and the like of B's atmosphere; and let b represent the sum of these quantities of matter when A and B are at the distance m, and $p\ b$ when they are at the distance n; also at an unit's distance let the repulsion of A and B with the atmospheres which they have at the distance m, be d, and their attraction e; then with their atmospheres at the distance n, if the repulsion be $q\ d$ at the unit's distance, the attraction will be $q\ e$, because the forces at a given distance are as the quantities of matter; hence

$\frac{e}{m^2}(a+b)$ is the attraction, and $\frac{d}{m^2}(a+b)$ is the repulsion, at the distance m

and

$\frac{qe}{n^2}(a+b)$ is the attraction, and $\frac{qd}{n^2}(a+b)$ is the repulsion, at the distance n.

Hence at the distance m, the attraction is to the repulsion as e to d, and at the distance n, it is in the same ratio From this it follows that if at any distance the attraction is equal to the repulsion, or if it be greater or less than the repulsion, it will be so at all distances, and therefore Mossotti's theory cannot be true.

Also from the assumption which Mossotti has introduced into his calculations, viz. that the elasticity of the æther varies as the square of its density, the insufficiency of his principles may be inferred. For the density must be much greater near the molecules than in free space, because the molecules powerfully attract the æther, and retain very dense atmospheres: hence also the density of the æther near a large body of molecules must be much greater than in free space; consequently its elasticity in free space must be much less than near the large body, being as the square of the density. But by his first principles the elasticity in free space must be perfect, since all the atoms are within the sphere of each other's repulsion, and no attraction exists among themselves, so that whatever displacement is made by a force applied, the atoms will restore themselves, when the disturbing force is removed, by a force equal to that by which they were displaced: hence the elasticity is both perfect and imperfect at the same time, which is impossible.

It seems as if Mossotti had assumed the universality of his æther to please the undulationists; but they have no reason to thank him, because it would completely undo their system, which is obliged to allow the hypothesis, that the elasticity of the æthereal medium is more imperfect in bodies than in free space.

Mr. Babbage further states that " the other conditions to be satisfied are," 1st. " That the juxtaposition of such atoms must in some circumstances form a solid body." 2nd. " In other circumstances a fluid." 3rd. In other cases a gaseous body—" The solid must possess cohesion, tenacity, malleability, elasticity:" the other forms must possess capillarity, and susceptibility of compression, without becoming solid, as also elasticity. p. 166. He adds, 4th. The *central* atoms must admit of a more intimate approach, so that their atmospheres may unite, and form one atmosphere. " Binary compounds might then have atmospheres not quite spherical, thus possessing polarity." 5th. " Combinations of three or more atoms, as the central body of one atmosphere, might give great variety of attractive forces." 6th. " Two or more central atoms uniting, might either not be able to retain the same amount of atmosphere, or they might possibly be able to possess a larger quantity." p. 166. and 167.

Corresponding with these, it will be found in my treatise, 1st. That some bodies will have the solid form; prop. 19. 2nd. Others will have the liquid form; p. 20. 3rd. And others the gaseous form; p. 21. See also pages 65, 93, 103 and 105. 4th. That if two tenacious atoms with their atmospheres be placed very near each other, in certain cases both will become enveloped in one atmosphere, and these will have polarity; prop. 15. cor. 4. 5th. If three tenacious atoms with their atmospheres are placed sufficiently near, they become enveloped in one atmosphere. Particles composed of three or more atoms will possess polarity; prop. 16, and cor. 4. 6th. It will be found in the explanations of several phænomena, that the combinations of two or more atoms may retain more or less æthereal matter in their atmospheres than one of them separately; p. 96 to 100, and 140 to 160.

These several requisites of a good theory are with perfect ease derivable from my principles; but some of them are inconsistent with those of Mossotti, as is easily demonstrable. It may be said that on the electrical hypothesis of one fluid, as well as on that of two fluids, bodies will be repelled, or attracted, or held in a state of equilibrium, according to the distance; but it should be recollected that these hypotheses have the earth and its atmosphere as a foundation, or as a separate existing matter, and on this ground may be convenient as far as they go, although not physically true; but Mossotti's theory has no such support. If his conclusion from his mathematical investigations were correct, viz. that all distant molecules attract each other, we have shown they would always do so, and his theory could not be true; but we reject that conclusion, by proving that, according to his principles, not one of the forms of solids, liquids, or gases could exist.

First conceive that the atoms have no solid centres, then a molecule plunged into the æther would attract into its own centre just so many centres of the æthereal atoms as by their united forces would exactly equal the whole force of the molecule; therefore the attraction of the compound atom on any distant atom of either kind would be equal to its repulsion on the same distant atom: hence the result is neither attraction nor repulsion. Next let the small central spherical solid be restored, and the concentrated æther now symmetrically disposed about the solid as an atmosphere; this atmosphere will have the same effect on any distant atom as when placed in the centre (Newt. Prin. B. I. pr. 76.); but this atmosphere is that which the molecule would acquire when plunged into the æther, since, as is evident, its atmosphere would increase till its repulsion on any distant atom is equal to its attraction, and no more: therefore still its resultant action on any distant atom of either kind is null, and the same is true of any other molecule; hence, on these principles there could be neither cohesion nor elasticity, and the forms of bodies, as solids, liquids or gases could not exist.

Mr. Babbage, as above stated, has presented the theory in a modified form, in which the repulsions are regulated by some given law, and the attraction by some other given law. First, let there be no central solids, and take two molecules A and B, and let one of them, A, be plunged into the æther; it will, as in the former case, attract the centres of æthereal atoms into its centre, till just so many are concentrated as are equal in force to the molecule at its centre: but since the law of attraction is different from that of repulsion, the actions of the compound atom, on any distant atom, will be different. And now let the molecule B, with its concentrated æther, be placed at a distance from A, and let the molecule A attract the concentrated atoms of B with the force n, and let the concentrated atoms of A repel the same with the force m; therefore the attraction of the compound atom A on the concentrated atoms of B is $n-m$. Again, because of the equal forces at the centre, we have the attraction of the concentrated atoms of A on the molecule B equal to m; and the repulsion of the molecule A on that of B is n: therefore the attraction of the compound atom A on the molecule B is $m-n$: consequently the attraction of the compound atom A on the compound atom B is $n-m+m-n=0$, so that two distant atoms neither attract nor repel. If very small solids be supposed to occupy the centres, the same will follow; because whatever is the law of force, the effect on atoms whose distance is great in comparison of the diameter of the body, will be the same as if the atoms were collected in the centre of gravity of the body (Math. Prin. of Mech. Phil.); hence the effect in this case will be the same as before: therefore according to the theory thus modified, whatever may be the given laws of force, no action on distant atoms can result: the theory is therefore erroneous.

Having thus shown that Mossotti's theory is not the true one, it may be added, that I have frequently tried my own by such tests as occurred: it is possible that prejudice may have had some influence in preventing me from discovering my own errors, if such there be, in the principles. It would to me be a kindness if some philosopher would undertake to prove any defects which he may conceive militate against my theory: if he show it to be inadequate he should have my sincere thanks, however unexpected and unpleasant it might be to my views and feelings, as it would save me much expenditure of time and thought, of which, besides property, much has been devoted to this subject, with a view to the interests of philosophy, without other emolument; so that should the theory be proved insufficient it would conduce to my advantage and to that of others; while, on the other hand, if it be found that the interests of philosophy have been promoted by my labours, it will yield me a gratification and pleasure amply repaying my toils in the field of science.

Permit me to present, by way of conclusion, a brief view of what I consider to be done, and may, as it seems to me, be done, by this theory; first observing that all which remains in addition to its principles, in order to render every department of natural philosophy a mathematical science, is to obtain approximatively, 1st. A knowledge of the absolute forces of at least tenacious atoms; 2nd. The radii of the spheres of repulsion; 3rd. The quantities of each class and sort. The first is in a tolerable degree made out in the atomic weights of elements, which are obviously as the absolute forces, or masses, since these weights depend simply on the force of the earth's attraction at a given place. A rude approximation of the two others may be attained from phænomena, and the more accurately as science proceeds. Respecting these three particulars, the most likely assumptions may be admitted, and from them calculations instituted, and when these are compared with actual phænomena, such alterations may be made, in the constants assumed, as the results may indicate. It may here be observed that every phænomenon of physical astronomy is explicable on this theory, since as far as that elegant science is concerned, it agrees precisely with that of Newton.

But what greatly raises my expectation in favour of the truth of the theory is, that, 1st. The general properties of bodies are derivable from it, as extension, divisibility, mobility, vis inertiæ, &c. 2nd. The laws of motion or of vis inertiæ are necessary consequences. 3rd. The principle, which as the result of experiment forms the foundation of hydrostatics and pneumatics, viz. that pressure communicated to a mass of fluid in equilibrium is equally transmitted through the whole, may by it be easily proved. 4th. The three forms of bodies, as well as cohesion, capillary attraction, and the attraction of floating bodies, with elasticity, tenacity, &c., equally are the offsprings of these principles. 5th. The polarity of particles in certain circumstances, and consequently crystallization and chemical affinity, in like manner result from the same views. 6th. The same may be said of the important law of the diffusion of gases and vapours, of Mariotte's law, definite proportions, the theory of volumes, and the electrical condition of elements. 7th. The leading phænomena of electricity, galvanism, magnetism, electro-magnetism, and optics flow freely and clearly from the same fountain. I am deeply interested in the wish that some mathematical philosopher would take up the subject. Were I thirty, instead of almost seventy years of age, I would enter into a full investigation of this theory mathematically: but now my age forbids; it remains therefore only for me earnestly to recommend these labours, which I am sure will yield abundant fruit, to the consideration of the real lovers of science.

Bristol, Sept. 29th. 1837. THOMAS EXLEY.

LXXI. *On the Action of Ammonia on the Protochloride and Peroxide of Mercury. By* ROBERT KANE, *M.D., M.R.I.A., &c. &c.**

Of the Action of Ammonia on the Protochloride of Mercury.

§ 1.—*Action of Liquid Ammonia upon Calomel.*

THE decomposition resulting from the action of water of ammonia upon the protochloride of mercury, does not appear to have attracted particular attention, as all writers who speak at all upon the subject, mention ammonia, along with potash and soda, as decomposing calomel into black oxide of mercury. Hennell in particular, states expressly, that calomel decomposed by excess of ammonia, yields a black powder containing in 100 parts, 96 of mercury and four of oxygen. I was therefore rather surprised when experiment showed me that a reaction of a totally different nature takes place, giving rise to a compound possessed of very remarkable properties.

When water of ammonia is poured on calomel, whether sublimed or precipitated, the mass immediately becomes black, and the appearance is not altered by boiling the mixture for a long time. While yet wet the powder remains almost black, but it becomes much lighter on drying, so that when quite dry it is of a dark-gray. This powder is not altered by exposure to air, or to a moderate heat; a portion of it was exposed in a platinum crucible on a sand-bath for several hours to a temperature of 180° Fahrenheit, without being altered in weight or colour. When moistened it becomes nearly as dark as when first generated, but it again loses its black colour on being dried; boiled with water it does not appear altered in its composition. When this powder is heated in a tube sealed at one end, it first gives a trace of water, with much azote and ammonia; then there sublimes calomel mixed with metallic mercury, the decomposition being accompanied with that sort of effervescence which appears in the heating of so many of the substances under examination.

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* From the Trans. Roy. Irish Acad., vol. xvii. p. 441; being Sections II. and III. of the author's "Researches on the Action of Ammonia upon the Chlorides and Oxides of Mercury;" in continuation from p. 435 of our last number.

For the examination of this body, an order of analysis similar to that adopted for white precipitate was pursued.

A.—148·15 grains of precipitated calomel were boiled for some minutes with a great excess of water of ammonia, and the whole thrown on a filter. The black powder thus obtained weighed 141·92 grains corresponding to 95·79 grains from 100 of calomel.

The liquor that had been filtered off was acidulated by nitric acid and nitrate of silver added in excess; the chloride of silver precipitated was collected and dried: it weighed 44·44 grains corresponding to 30·0 from 100 of calomel; and the 30·0 grains of chloride of silver containing 7·401. But calomel consists in 100 parts of

Mercury	85·117
Chlorine	14·883

Therefore we have by this experiment, the black powder composed of

Mercury	85·117	and	88·85
Chlorine	7·482		7·76
Other matters . .	3·191		3·39
	95·790		100·00

No. 2.—153·36 grains of calomel were boiled with water of ammonia for a few minutes, and filtered. The dry dark-gray powder weighed 146·71 grains, corresponding to 95·66 per cent.

The liquor treated with nitrate of silver gave 44·03 of chloride of silver, corresponding to 28·71 of chloride per cent. and which contains 7·08 of chlorine.

Thus we obtain,

Mercury	85·117	or	88·98
Chlorine	7·803		8·15
Other matters .	2·740		2·87
	95·660		100·00

The mean of these experiments gives,

Mercury	88·91
Chlorine	7·95
Other matters . .	3·14
	100·00

B.—As the above method necessarily throws the chlorine and mercury estimate rather too high, the following experiment was made, in which the necessary loss produces an opposite effect:

101·37 of the powder were boiled with strong muriatic acid, and an acid solution of protochloride of tin added. The reduction of the quicksilver took place readily, and large well-formed globules appeared; the metal collected and carefully dried, weighed 89·39 grains, or 100 of the powder had given 88·18.

C.—51·42 of the gray powder were dissolved in dilute aqua regia, and a current of sulphuretted hydrogen in excess passed through the liquor. It was found, that owing to free chlorine, the sulphur precipitated invalidated the result. The whole was therefore mixed with nitric acid, and boiled until the sulphuret of mercury was completely decomposed; the liquor was then freed from the particles of pure sulphur and evaporated until all free nitric acid and chlorine were completely dissipated. Being then treated by sulphuretted hydrogen, it yielded a sulphuret, pure and jet black, which collected and dried, weighed 52·39 grains, consisting of

$$\left.\begin{array}{ll}\text{Sulphur} \ldots\ldots\ldots & 7{\cdot}19 \\ \text{Mercury} \ldots\ldots\ldots & 45{\cdot}20\end{array}\right\} 52{\cdot}39$$

The 51·42 grains therefore contained 45·20 of mercury
or 100·00 - - - 87·90.

In this experiment so much ammonia was lost by the treatment with nitric acid, that its quantity could not be determined.

D.—As in none of these former analyses had the ammonia constituent been determined, the following experiments were made for the purpose of ascertaining its precise quantity:

1st. 66·43 grains were boiled with an excess of solution of iodide of potassium, and the flask being connected with a bent tube dipping into dilute muriatic acid, the heat was kept up until all the ammonia and about half the water had passed over. The liquor was then evaporated to dryness, and yielded a residue of 6·96 grs. of sal-ammoniac, consisting of

$$\left.\begin{array}{ll}\text{Muriatic acid} \ldots & 4{\cdot}73 \\ \text{Ammonia} \ldots\ldots & 2{\cdot}33\end{array}\right\}$$

or 100 of powder gives 3·36 of ammonia.

The action of potash on the gray powder liberates ammonia likewise; but it was found so difficult to obtain complete decomposition that the method was abandoned. Another process tried, consisted in repeatedly distilling strong muriatic acid off the powder, in order to convert it into metallic mercury, corrosive sublimate, and sal-ammoniac, and thus obtain a quantitative result; but this method also was found of so imperfect action, that it could not be well applied.

Summing up the results of the analyses above recorded, we have for 100 parts of the powder:

Process.	Mercury.	Chlorine.	Ammonia.
A	88·91	7·95	
B	88·18		
C	87·90		
D			3·36.

Or the mean result is

Mercury	88·33
Chlorine	7·95
Ammonia	3·36
Loss, &c.	0·36
	100·00

It is evident that we have here a body precisely corresponding to white precipitate; the mercury, however, being in proto-combination. Water of ammonia acting on calomel, abstracts half the chlorine, which is replaced by a corresponding quantity of ammonia in some form of combination. We can accordingly construct two formulæ corresponding to white precipitate; in the first, half the mercury being conserved as protoxidized and combined with an atom of ammonia: in the second, that half of the mercury being directly united to amidogene. The former theory gives from the formula $(\mathrm{Ch}+\mathrm{Hg})+(\dot{\mathrm{H}}\mathrm{g}+\mathrm{NH}^3)$.

Mercury	87·00	100·00
Chlorine	7·59	
Oxygen	1·73	
Ammonia	3·68	

and 100 of calomel should yield 97·84 of product; whilst the second, from the formula $(\mathrm{Ch}+\mathrm{Hg})+(\mathrm{NH}^2+\mathrm{Hg})$ gives,

Mercury	88·72	100·00
Chlorine	7·74	
Amidogene	3·54	

and 100 of calomel should yield 95·95 of product, which is almost precisely the quantity obtained in experiment.

We here find the evidence in favour of the existence of amidogene in combination to be almost insuperable. I shall nevertheless retain all through this paper the two methods of expression, until by examining the compounds of the other metals, the differences may become so much larger, as to completely prevent their falling within possible limits of error of observation.

Of the Action of Ammonia upon Peroxide of Mercury.

The accurate examination of the action of ammonia upon peroxide of mercury is of very great importance, as the compound resulting, the ammoniuret of mercury, is one of a very remarkable class of bodies, viz. the fulminating compounds containing ammonia; and in addition, the experiments of Guibourt, the only chemist I believe who has made analyses of it, would appear to demonstrate in it, a relation between the number of atoms of ammonia and oxygen, which must influence the ammoniacal theories to a very great extent. These circumstances made me trace out the properties of this body with more exactness than should have been otherwise required.

I have not been able to prepare a substance possessing the external characters of the ammoniuret of mercury described by Fourcroy and Thenard. I have varied in every manner I could imagine, the method of obtaining it; but, although I got a substance constantly the same in its properties and composition, it differed much in appearance from that described by the French chemists. They state, that by digesting liquid ammonia on red oxide of mercury during eight or ten days, the oxide gradually covers itself with a yellowish-white powder which generally passes to a very fine white. I have never obtained it of a pure white, but always with a tinge of yellow, possessing an appearance and affording on analysis, results always the same. The constancy of its properties justifies me, I should think, in considering it as pure, notwithstanding its not exactly agreeing with their result. Unfortunately they did not publish any quantitative analysis of their product; the only one known to me is that in Guibourt's thesis.

In order to prepare ammoniuret of mercury, I precipitated a solution of sublimate by potash, and the precipitate having been well washed from all excess of alkali, was put into a bottle of water of ammonia and left for some days; its colour became much lighter, but never completely white. Other portions of recently precipitated peroxide were boiled in water of ammonia for a few minutes, until the colour ceased to undergo any change: the reaction was very much accelerated by heat. These different portions of product had all the same colour, and were indifferently, but without mixture, used in the following examination without any difference of properties becoming observable.

When this ammoniuret is heated, it gives off much ammonia and azote; a considerable quantity of water collects in the tube, and the matter remaining becomes dark-red, like peroxide; but if it be allowed to cool, it reassumes its whitish colour, and is evidently still unaltered ammoniuret. The reaction evidently does not consist in a separation of the ammoniuret into ammonia and peroxide; but, from the commencement to the termination, there are disengaged water, ammonia, azote, oxygen and metallic mercury. The ammoniuret, like many other mercurial compounds, is dark-red when hot, but of a whitish colour when cold. When a quantity of the ammoniuret is suddenly thrown on ignited coals it explodes very feebly, and far inferiorly to fulminating gold, with which its discoverers have compared it: it dissolves readily in nitric or muriatic acid.

To analyse this compound, processes of a simple nature were sufficient.

A.—72·07 grains of ammoniuret were dissolved in muriatic acid, and the liquor having been diluted was decomposed by sulphuretted hydrogen. The resulting sulphuret dried and weighed, amounted to 70·08 grains, consisting of

Sulphur	9·61
Mercury...............	60·47

The liquor and washings evaporated to dryness, gave sal-ammoniac, 9·21 grains, consisting of

Muriatic acid.........	6·28
Ammonia	2·93

Hence, supposing the mercury to exist as peroxide, we have as the result of the analysis:

Mercury...............	60·47	72·07
Oxygen	4·78	
Ammonia	2·93	
Water and loss	3·89	

or in one hundred parts—

Mercury...............	83·90
Oxygen	6·63
Ammonia	4·07
Water and loss	5·40

2.—The following analysis was made on a portion of ammoniuret prepared at a different time and in another manner than that used in the former experiment.

67·57 grains were dissolved in muriatic acid and decomposed by a stream of sulphuretted hydrogen. The precipitated sulphuret weighed 65·37 grains, consisting of

Sulphur	8·96	65·37
Mercury...............	56·41	

The liquor evaporated to dryness gave 8·15 grains of sal-ammoniac, consisting of

Muriatic acid.........	5·54
Ammonia	2·61

we have therefore the result—

Mercury	56·41	67·57
Oxygen	4·46	
Ammonia	2·61	
Water and loss	4·09	

or in 100 parts—

Mercury	83·48
Oxygen	6·59
Ammonia	3·86
Water and loss	6·07

a result almost identical with the former.

B.—52·22 grains were dissolved in muriatic acid and decomposed by chloride of tin. There were obtained 43·74 of mercury corresponding to 83·76 per cent.

C.—As the constancy of the amount of mercury and ammonia in the preceding results, proved completely that the loss did not arise from error, but probably from water present, the following experiment was made to ascertain whether water existed in such quantity: A small green glass retort was blown, with a pretty long neck; to it was attached a tube containing potash; and the ammoniuret in the retort having been decomposed by a red heat, its gaseous elements were allowed to escape; the mercury condensed in the neck of the retort and the water in the potash-tube; the result, though not absolutely true, is sufficiently accurate for the determination of the point required.

Weight of retort and material		75·38
Weight of retort		63·00
Ammoniuret used	12·38 grains	
Weight of retort and mercury-residue		73·35
Weight of retort		63·00
Mercury remaining	10·35	
Weight of potash-tube before		278·28
Weight of potash-tube after		278·95
Water absorbed	0·67	

We thus obtain as results—

Mercury	10·35	83·62
Water	·67	5·39
Gases and loss	1·36	10·99

But the gases consist of oxygen and ammonia, the former being such as to peroxidize the mercury; and assuming the remainder to be ammonia without loss, we have,

$$\left.\begin{array}{lr} \text{Mercury . . .} & 83{\cdot}62 \\ \text{Oxygen . . .} & 6{\cdot}60 \\ \text{Ammonia . .} & 4{\cdot}39 \\ \text{Water . . .} & 5{\cdot}39 \end{array}\right\} 100{\cdot}00$$

These results summed up, give—

	Process.	Mercury.	Oxygen.	Ammonia.	Water.
A,	No. 1.	83·90	6·63	4·07	5·40
—	No. 2.	83·48	6·59	3·86	6·07
B		83·76	6·60		
C		83·62	6·60	4·39	5·39

Giving a mean result of

$$\left.\begin{array}{lr} \text{Mercury . . . .} & 83{\cdot}68 \\ \text{Oxygen . . . .} & 6{\cdot}60 \\ \text{Ammonia . . . .} & 4{\cdot}10 \\ \text{Water . . . .} & 5{\cdot}62 \end{array}\right\} 100{\cdot}00$$

on abstracting the water, we have—

$$\left.\begin{array}{lr} \text{Mercury . . . .} & 88{\cdot}67 \\ \text{Oxygen . . . .} & 6{\cdot}99 \\ \text{Ammonia . . . .} & 4{\cdot}34 \end{array}\right\} 100{\cdot}00$$

The only analysis of this substance that I am aware of having been published, is that of Guibourt, already quoted, and he considers it to be a compound of oxide of mercury and ammonia in such proportion that the hydrogen of the ammonia could convert the oxygen of the oxide of mercury into water, consequently his formula is the following ($3\ddot{\mathrm{Hg}}+2\mathrm{NH}^3$) and the per centage result:

$$\left.\begin{array}{lr} \text{Mercury . . . .} & 88{\cdot}08 \\ \text{Oxygen . . . .} & 6{\cdot}95 \\ \text{Ammonia . . . .} & 4{\cdot}97 \end{array}\right\} 100{\cdot}00$$

with which my analyses may be considered as completely agreeing. In the abstracts of Guibourt's paper that I have seen, there is not any notice taken of the water present; but yet its constant value shows it to be a chemical ingredient, and we have its atomic proportion, thus—

$$\frac{2\,\mathrm{Hg}}{3\,\mathrm{H}}=\frac{405{\cdot}6}{27}=\frac{83{\cdot}68}{5{\cdot}57}\ \text{or nearly}\ \frac{83{\cdot}68}{5{\cdot}62}$$

The compound ($3\,\ddot{\mathrm{Hg}}+2\,\mathrm{NH}^3+4\,\dot{\mathrm{H}}$) gives us in per cent. composition, the following:

$$\left.\begin{array}{lr} \text{Mercury . . . .} & 83{\cdot}72 \\ \text{Oxygen . . . .} & 6{\cdot}60 \\ \text{Ammonia . . . .} & 4{\cdot}72 \\ \text{Water . . . .} & 4{\cdot}96 \end{array}\right\} 100$$

A result agreeing very closely with that of experiment.

Admitting that the azotic element is engaged in the combination as amidogene, and not as ammonia, the above formula converts itself into

$$(2\,\ddot{Hg} + (2\,NH^2 + Hg) + 6\,\dot{H})$$

a method of arrangement which we have already met with as an element of the yellow powder, formed by water on white precipitate.

LXXII. *On the Nature of Lampic Acid.* *By* ARTHUR CONNELL, ESQ., *F.R.S.E.*[*]

IT is well known that Professor Daniell, to whom we are indebted for a knowledge of the properties of this acid liquid, ultimately came to the conclusion that it is acetic acid containing some disoxygenating substance which bestows its property of reducing metallic oxides.

A few years ago I had occasion to examine this acid, as well as that resulting from the action of potash on alcohol, and that obtained by distilling a mixture of alcohol, peroxide of manganese, and sulphuric acid, and I came to the conclusion that they all contained formic acid besides acetic acid. The grounds on which the existence of formic acid in them was inferred, were that they all reduced the oxides and salts of mercury and silver *with effervescence*, and that they all were capable of yielding perfectly well characterised formates of lead and of magnesia[†].

About the same time M. Leopold Gmelin came by independent observation to the same conclusion as myself respecting the acid from alcohol, oxide of manganese and sulphuric acid[‡]; and as the manner in which I had examined the three liquids, the results which I had obtained, and the conclusions which I had drawn regarding them, were the same in all the three cases, I could not avoid regarding M. Gmelin's experiments as amounting to a verification of the view which I had given regarding all the three acid liquids.

In a late memoir, however, on the products of the oxidation of alcohol[§], M. Liebig once more asserted the peculiar nature of lampic acid by maintaining that it was "probable, not to say certain," that lampic acid is identical with a peculiar acid, which this distinguished chemist supposes is formed by the action of oxide of silver on aldehyde, and to which he has given the name of the aldehydic. M. Mitscherlich, in the

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[*] Communicated by the Author.

[†] Edinb. New Phil. Journal, vol. xiv. p. 231.

[‡] Poggend. *Annal.*, xxviii. 508.

[§] *Annales de Chim. et de Physique*, lix. 289.

last edition of his valuable treatise on chemistry, has adopted without comment this view of M. Liebig as to the identity of the properties of these two acids[*].

The object of my inquiry is not aldehydic acid. The chemical world will gladly receive further light respecting that acid; but I think it will not be difficult to show that it will not be safe in the mean time to receive lampic acid as its representative.

It would appear that M. Liebig did not make any experiments himself on lampic acid. He however drew from the experiments of Messrs. Daniell[†] and Phillips the following conclusions, which led him to suppose the identity of the two acids, and which it will be observed differ in several particulars from those which these able chemists had themselves drawn.

"1st. Lampic acid reduces the salts of mercury and silver *without effervescence.*

"2nd. During this reaction it is changed into acetic acid.

"3rd. Its atomic weight is the same, or very nearly the same, as that of acetic acid."

These conclusions shall be examined in their order, after describing the mode of preparation of the acid liquid.

The acid examined in my former researches on this subject was prepared by suspending a small piece of ignited spongy platinum by a fine platinum wire, over æther covered by a glass funnel, to condense the vapours formed. In the present case I substituted a coil of fine platinum wire for the piece of spongy platinum, but no difference was observed in the qualities of the acid obtained. The sulphuric æther was contained in a small evaporating basin which was placed in a large one. The coil of fine platinum wire was then suspended by a long wire of the same kind in a large inverted glass funnel; and after the coil had been ignited the funnel was placed in the larger evaporating basin so as to bring the coil a little way above the surface of the æther. An alembic was then suspended a little way above the narrow extremity of the funnel; and the acid vapour formed was condensed principally in the funnel, from which it fell back into the large evaporating basin, and partly in the alembic head[‡]. The small

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[*] *Lehrbuch,* i. 159. 3te Auf.

[†] [The principal results originally obtained by Mr. Daniell were stated in Phil. Mag., First Series, vol. liii. p. 64.—Edit.]

[‡] When the funnel is large, as from five to six inches' diameter, it should not be raised, so long as the operation continues, because an explosion is apt to ensue from the too great access of air, although without injury to the vessels. On the other hand, when it is smaller it is necessary to place some thin fragments of glass below its edge, to admit air.

quantity of residual liquid in the smaller evaporating basin was always thrown away as not consisting of condensed vapour. From 2 ounces of æther, which required about $2\frac{1}{4}$ hours for consumption, rather more than a dram of the concentrated acid liquid was usually obtained, and the vessels were washed out with another dram of distilled water, which was added to the acid liquid, in which state it was employed in the experiments detailed.

It is unnecessary to say that the liquid thus obtained possesses powerful acid qualities, reddening vegetable colours strongly and effervescing briskly with carbonates: and when we talk of the nature of lampic acid, my understanding of the expression is that we thereby mean the nature of the acid thus contained ready formed in the liquid so procured. That there are other products of an æthereal, oily, or resinous description contained in the liquid there is no doubt; but these are not acids; and it will be unnecessary to have recourse to the supposition that they produce or modify the reactions of the acid which they accompany, if it can be shown that that acid exerts *in its own nature* all the characteristic reactions of the liquid. The examination of these accompanying products did not come within the scope of my inquiry either at present or formerly.

I. To determine experimentally whether lampic acid reduces the salts of silver and mercury *without effervescence,* a little of the acid was placed in a tube with solution of protonitrate of mercury, whilst one end of a narrower tube was fitted into the larger by means of a cork, and the other extremity terminated in a small quantity of lime-water. On applying heat to the mixture there was brisk effervescence, precipitation of metallic mercury, and evolution of elastic fluid which speedily made the lime-water very muddy; and on adding an acid to the lime-water the muddiness disappeared with effervescence.

This experiment was repeated, substituting peroxide of mercury for the protonitrate. Copious effervescence ensued as before, and evolution of elastic fluid which made the lime-water muddy; and there was at first on partial cooling a precipitation of white saline matter, and on the further application of heat this disappeared with precipitation of metallic mercury.

When solution of nitrate of silver was heated with lampic acid until effervescence and precipitation of metallic silver as a dark brown powder ensued, and the action was maintained by the occasional application of heat, the lime-water into which the evolved gas was conducted became very muddy as before.

With oxide of silver and lampic acid there was, when heat was applied, the like effervescence and evolution of carbonic acid, which made lime-water muddy; and after the liquid had been evaporated away on the sand-bath, the residue was metallic silver.

In these circumstances we cannot hesitate to say that lampic acid reduces the salts and oxides of mercury and silver with effervescence and evolution of carbonic acid *.

This quality, it is unnecessary to state, is a property of formic acid. It was also formerly shown that perfectly well characterised formates of magnesia and of lead may be ob-

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* Although it is quite true that Professor Daniell does not state on all occasions that lampic acid reduces these salts *with* effervescence, yet it is equally certain that he nowhere says that it reduces them *without* effervescence; and M. Liebig appears to have overlooked one or two passages which seem to prove sufficiently that Mr. Daniell was perfectly aware that lampic acid reduces these salts *with* effervescence and evolution of carbonic acid. After stating (*Journ. Instit.*, vol. vi. p. 323) that lampate of mercury when heated was reduced with "violent effervescence," he adds, that wishing to know "the nature of this decomposition of the metallic oxides," he heated black oxide of manganese with lampic acid, and found that carbonic acid was given off, which precipitated lime water; an experiment which I have made with the like result.

A circumstance was observed in the course of my experiments which is connected with the existence of substances not of an acid nature in the liquid, and which I shall merely notice for the use of any one who may wish to make these substances the subject of a separate study. When pure lampic acid is heated to from 150° to 160° Fahr. an evolution of permanently elastic fluid commences, and if the heat is gradually increased as occasion requires, a quantity of permanently elastic fluid is evolved, amounting to nine or ten times the bulk of the liquid employed. When the gas so evolved is collected over mercury and washed either with pure water or lime water, about one sixth of it is absorbed, and the lime water does not become muddy. The part absorbed appears merely to be vapour either of the acid or of some æthereal product. The residual gas was found to be inflammable, and when analysed in the voltaic eudiometer proved to be hydrogen nearly quite pure. Conformably with this result it was found that when the gas evolved from a heated mixture of lampic acid and the salts or oxides of mercury or silver was collected over mercury, about one third only of its bulk, and sometimes somewhat less, was absorbed by lime-water with precipitation of carbonate of lime; and the residue on analysis proved to be hydrogen. On the other hand, if the acid was first heated till permanently elastic fluid no longer was evolved, and then mixed with protonitrate of mercury and again heated, the salt was reduced with effervescence and evolution of carbonic acid without any mixture of inflammable gas. In like manner if the acid was saturated with soda and evaporated to dryness, and the salt thus got was redissolved in water, and mixed with protonitrate of mercury and heated, the salt of mercury was reduced with evolution of carbonic acid without inflammable gas. The origin of this hydrogen was not further investigated, because its presence was evidently altogether unconnected with the properties of the acid contained in the liquid; these properties being the same whether the liquid had been previously heated or not.

tained from the liquid by the proper steps. The salt of magnesia is highly characteristic, and any one who will prepare it from lampic acid in the manner formerly pointed out, and compare it with crystallized formate of magnesia, will at once see the identity. The properties of the salt of lead are also decisive. When the acid liquid was treated in the cold with carbonate of lead and left some days, the sparingly soluble salt of lead gradually precipitated from the liquid. This salt was then dissolved in water by boiling, and the solution filtered while hot. On cooling characteristic shining spicular crystals of formate of lead precipitated. The whole salt produced by saturating the acid with lead was then treated with alcohol at the temperature of 100° Fahr. to take up any acetate of lead present; and a portion of the residual salt was heated with concentrated sulphuric acid. Carbonic acid was evolved in abundance, and was recognised by its usual properties of burning with a pale blue flame and being absorbed by heated potassium.

Of the presence of formic acid in the lampic liquid not a doubt could therefore exist. To show that it also contained acetic acid, the alcohol with which the lampic salt had been treated was examined, after a little saline matter which made it slightly muddy had subsided and been separated by filtration, when the alcohol was found to contain dissolved a small quantity of a salt of lead; and as the presence of resinous matter rendered it difficult to examine its properties, a newly prepared portion of it was treated with diluted sulphuric acid and distilled. The acid obtained was neutralized with carbonate of soda, and the solution after being heated to boiling mixed with a hot solution of protonitrate of mercury, when a copious deposit of shining scales of acetate of mercury took place, either immediately or on cooling, according to the state of concentration of the liquid*.

Although the existence of acetic acid in lampic acid was thus established, yet its quantity was small compared to that of the formic acid. It was found that the proportion of acetate of lead taken up by the alcohol to that of the undissolved formate was about 1 to 5; and as the latter salt was much more free from resinous matter than the former the real proportion of acetic acid was apparently still less than this.

II. We are now in a condition to judge of the accuracy of the conclusion that in reducing the salts of silver and mer-

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* If a hot solution of formate of soda is mixed in this way with a hot solution of protonitrate of mercury, an immediate precipitation of metallic mercury ensues with effervescence; and no white salt is deposited either immediately or on cooling.

cury lampic acid is changed into acetic acid. If lampic acid consists of formic acid with a little acetic acid, it will not require any argument to show that in reducing metallic salts it is not changed into the last of these acids. In concluding that it was so changed, M. Liebig referred to an experiment described by Mr. Daniell, that when it is heated with peroxide of mercury, a shining micaceous salt is deposited on cooling, which he considered to be acetate of mercury. This experiment I had repeatedly made, and described in my former notice; and my impression also then was that the precipitated salt was entirely acetate of mercury; but the conclusion which both Professor Daniell and myself had drawn from the experiment was, not that lampic acid had been changed into acetic acid, but that the acetic acid thus supposed to combine with protoxide of mercury existed ready formed in the liquid. From the researches which I have since made, I entertain no doubt that the salt thus precipitated is to a great extent formate of mercury. It undoubtedly contains in the form of acetate all the acetic acid which is present in the liquid; but the fact which was formerly overlooked, is that pure formic acid when heated with peroxide of mercury affords on cooling a silvery micaceous salt much resembling acetate of mercury. This fact is not new to chemists*; and it was further verified as follows. Crystallized formate of lead prepared with formic acid obtained by Dœbereiner's process from tartaric acid, was heated with alcohol to take up any acetate of lead which might by possibility have been present, and was then distilled with half its weight of sulphuric acid diluted with an equal weight of water. A portion of the formic acid thus obtained was gently heated with peroxide of mercury; effervescence ensued, and on cooling a micaceous shining mass like acetate of mercury was deposited. When this mass was further strongly heated, it was reduced to metallic mercury with effervescence. It was formerly stated that when the white salt which precipitates on cooling, after lampic acid has been moderately heated with peroxide of mercury, is further strongly heated, it is in like manner reduced to metallic mercury with effervescence.

To ascertain with still greater precision the nature of this salt as produced from lampic acid, a portion of it was distilled with sulphuric acid diluted with twice its bulk of water. The distilled liquid had a distinct smell of formic acid, and when heated with peroxide of mercury effervescence ensued, and a white micaceous deposit was formed on cooling; the salt examined was therefore principally formate of mercury. Had

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* See Gmelin's *Handbuch*, ii. 125. 3te Auf.

it been entirely acetate of mercury, the acid obtained from it would simply have dissolved the peroxide of mercury.

From all these considerations it is manifest that there are no grounds for supposing that lampic acid when it reduces the salts of silver or mercury is converted into acetic acid.

III. The last point relates to the atomic weight of this acid.

The observations which follow on this subject will, perhaps, appear to be superfluous to those who may be of opinion that lampic acid has been shown by sufficiently decisive characters to consist of known acids. The apparent puzzle on this point has arisen from the foreign substances contained in the acid liquid disguising the result obtained, as will be sufficiently evident from what follows. I made an analysis of lampate of barytes, and at first obtained a result identical with that of Professor Daniell. The salt was prepared by saturating lampic acid with carbonate of barytes, and was once or twice alternately dissolved in water and evaporated to dryness. At each successive evaporation it emitted pungent vapours, and continued a brownish and partially crystallized mass. It was then reduced to powder and dried *in vacuo* over sulphuric acid. 6·8 grains of the salt thus prepared were dissolved in water and precipitated by sulphate of soda. The sulphate of barytes after ignition weighed 6·25 grains, equivalent to 4·1017 of barytes. These data would give as the constituents of the salt

Acid	2·6983	39·68
Barytes ...	4·1017	60·32
	6·8	100·

and 629·4 as the atomic weight of lampic acid, that of acetic acid being 643·19 and that of formic acid 465·35; but the brown aspect of the salt was quite sufficient to show that it was not in a state of purity; the impurity of course having the effect of increasing the apparent atomic weight of the acid. Accordingly it was easy to separate from it a quantity of brown resinous matter without altering its nature. A quantity of the salt was kept on the sand-bath at the temperature of from 300° Fahr. to 320° Fahr. for three quarters of an hour. By this treatment it became dark brown, and on solution in water when cold, much dark brown resinous matter was deposited. The solution separated from this matter was still coloured and was evaporated to dryness, and the mass again kept at a similar temperature for several hours. When again dissolved in water, more resinous matter separated, and when again evaporated the mass was still brown.

It was then digested in alcohol for some time under the idea that more of the resinous matter would be taken up; but as the alcohol had dissolved a little of the salt itself and had not acquired a deep tint, it was evaporated to dryness and the residue returned to the original mass of the salt. The whole was again dissolved in water, filtered from a fresh quantity of resinous matter which separated on the application of heat, and finally evaporated to dryness. The salt thus obtained, and which still had a brown colour, was dried *in vacuo* over sulphuric acid: 12·34 grains of it thus dried yielded 11·78 grains of sulphate of barytes, which would give the constituents of the salt as

Acid............	4·609	37·36
Barytes	7·731	62·64
	12·340	100·

and the atomic weight of lampic acid as 570·7. Thus by the separation of these successive portions of resinous matter the apparent atomic weight of the acid had been reduced from 629·4 to 570·7; but it was evident that the salt still contained impurity both from its own brown colour and from that of the solution separated from the sulphate of barytes in the analysis; and it would have required a frequent repetition of the process to have approached the true combining proportion of the acid. This, however, the circumstances did not require should be done, it being sufficient to show that the atomic weight was not the same, nor nearly the same, as that of acetic acid; for little doubt could then remain that the real combining proportion was that which belongs to the mixture of known acids of which it is conceived that lampic acid has been ascertained, by its ordinary reactions, to consist.

Care was taken to establish that the treatment had not altered the nature of the barytic salt analysed. The soda salt obtained in the latter analysis, and containing the acid which had been previously combined with barytes, still reduced protonitrate of mercury with effervescence and evolution of carbonic acid on the application of heat; and by an examination of the resinous matter which had separated, it was ascertained that not more than $\frac{1}{116}$ of the barytes contained in the salt examined had been separated during the treatment to which it had been subjected, so that only a very minute quantity of the salt had been decomposed, if indeed the trace of barytes separated had not previously been combined with the impurity itself.

The conclusions to which all my experiments have led are the following:

1st. Lampic acid reduces the salts and oxides of mercury and silver with effervescence and evolution of carbonic acid gas; and it possesses this character in its own nature, and independently of any matters not of an acid nature by which it may be accompanied.

2nd. It is obvious from this character, and from the appearance and properties of its salts, that lampic acid is principally formic acid, but mixed with a small quantity of acetic acid.

In the memoir alluded to, M. Liebig further states, that by the action of potash on alcohol a small quantity of an organic acid is produced, the salts of which reduce with the aid of heat the salts of mercury and silver, without effervescence. I have here only to repeat what I had previously stated after a detailed examination of this acid, that according to my experiments it reduces the salts of mercury with effervescence; and that this character and the appearance and properties of its salts show that it is entirely analogous to lampic acid in its nature. In other words it is formic acid mixed with a little acetic acid.

In my examination of it, as in that of lampic acid, I was formerly led to over estimate the proportion of acetic acid contained in it, from supposing the salt deposited when it is heated with peroxide of mercury to be acetate of mercury, whereas I have now little doubt that it really is to a great extent formate of mercury. From this acid being produced only in small quantity it is usually examined in a very dilute state, and on this account the appearance of effervescence when heated with the salts of mercury occasionally escapes observation. It so happens that it was the previous examination of this acid which led me to perceive the nature of lampic acid, as afterwards experimentally ascertained.

Edinburgh, Sept. 22, 1837.

LXXIII. *Substance of a Communication on the Temperature of some Mines in Cornwall and Devonshire, made by* Robert Were Fox, *to the Royal Geological Society of Cornwall at their last Annual Meeting.*[*]

IN the following table I have given the results of observations on the temperature of mines which I have reported from time to time to the Cornwall Geological Society and other societies; and I have selected those experiments only which were made at or near the deepest parts of the mines

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[*] Communicated by the Author: see our last Number, p. 480.

enumerated, and in which the bulbs of the thermometers were either buried in the ground or plunged under water, and into streams gushing into the mines.

References to the letters.—C. Copper mine; T. Tin mine; L. Lead mine; S. Silver mine; K. "Killas;" G. Granite; *w.* Water; *r.* Rock or ground.

TABLE.

Mines not exceeding 100 fathoms in depth from the surface.

	Ore worked in Mines.	Rock.	Depth in fathoms.	Bulb of thermom. in	Temp.	Date of Observation.
South Huel Towan	C.	K.	45	*w.*	60°	1822.
Huel Wellington Eastern end of level	C.	K.	50	*w.*	58	1827.
Western do. do.		K.	50	*w.*	57	1827.
East Liscomb	C.		82	*w.*	64	1822.
Huel Unity Wood	T.&C.	K.	86	*w.*	64	1822.
Huel Unity	T.&C.	K.	90	*r.*	66	1820.
			403		369	

5 Mines, or 6 Stations: mean depth 67·1; mean temp. 61°·5.

Mean temp. of climate 50*

Excess .... 11·5 = 1° in 35 ft.

Mines from 101 to 200 fathoms in depth:

Beer Alston	L.&S.	K.	120	*w.*	66°·5	1822.
Huel Squire	C.	K.	120	*w.*	68	1820.
Chasewater Eastern end of deepest level	T.&C.	K.	128	*w.*	75	1827.
Western end of do.		K.	128	*w.*	68	1827.
Huel Trumpet	T.	G.	128	*w.*	65	1822.
Huel Vor	T.	K.	139	*w.*	69	1819.
Tingtang	C.	K.	140	*r.*	66	1820.
Treskerby	C.	G.	140	*w.*	76	1819.
Poldice, bottom of a shaft	T.&C.	K.	144	*w.*	78	1822.
Do. do. another			144	*w.*	80	1822.
Consolidated mines Bottom of a shaft	C.	K.	150	*w.*	76	1822.
Do. of another do.			150	*w.*	80	1822.
Huel Damsel	C.	G.	150	*r.*	70	1820.
Huel Alfred Eastern end of a level	C.	K.	155	*w.*	70	1827.
Do. do. Western end of a level			155	*w.*	67	1827.
Carried over			2091		1074·5	

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* The mean temperature of the climate in a large proportion of the mining districts of Cornwall and Devon is rather below 50° I believe.

	Ore worked in Mines.	Rock.	Depth in fathoms.	Bulb of therm. in	Temp.	Date of Observation.
Brought over			2091		1074°·5	
United Mines	C.	K.	170	*w.*	76	1819.
Huel Friendship	C.	..	170	*w.*	64·5	1822.
Poldice	C. & T.	K.	176	*w.*	99	1830.
Tingtang	C.	K.	178	*w.*	82	1830.
Cookskitchen	T.&C.	G.	190	*w.*	68	1815.
United mines	C.	K.	200	*r.*	88	1820.
Huel Abraham	C.	..	200	*w.*	78	1815.
Stray Park Eastern end of level	..	..	200	*w.*	72	1827.
Western do.	..	..	200	*w.*	74	1827.
			3775		1776	

19 Mines, or 24 Stations: mean depth 157·3; mean temp. 74.

Mean temp. of climate .. .. 50 feet.
Excess .. .. .. 24° = 1° in 39·3

If some of the foregoing results, which differ much from the mean temperature, were omitted, the ratio, instead of 1° in 39·3 feet, would be 1° in 43 feet, at least.

Mines from 201 to 300 fathoms in depth.

	Ore worked in Mines.	Rock.	Depth in fathoms.	Bulb of therm. in	Temp.	Date of Observation.
Huel Vor	T.	K.	209	*w.*	79°	1830.
Levant	T.&C.	G.	‡230	*r.*	80	1837.
Dolcoath	T.&C.					
Bottom of Shaft		G.	230	*w.*	82	1815.
Near the end of deepest level, thermometer ? feet deep in rock, during 19 months		G.	230	*r.*	76	1822.
Bottom of Shaft		G.	239	*w.*	82	1819.
Tresavean	C.	G.	254	*r.*	76	1837.
Consolidated mines	C.	K.				
3 feet deep in the rock in a cross level and 24 fathoms from the lode			‡290	*r.*	85·3	1837.
In the lode 3 feet deep			290	*r.*	92	1837.
			1972		652·3	

5 Mines, or 8 Stations: mean depth .. 246·5 81·5
Mean temp. of climate 50

Excess .. . 31°·5 = 1° in 47 ft.

or, omitting the last station, at 92° of temperature, the ratio would be 1° in 48 feet.

If, instead of making the calculation from the *surface* downwards, it had been commenced at 10 or 15 fathoms under it, where the temperature may be supposed to be nearly constant, and coincident with the mean temperature of the climate, it is evident that the diminishing ratio of the augmentation of heat as the depth increased would be rendered still more remarkable. On the other hand, the vicinity of metalliferous veins in most of the instances reported, may have given a rather higher mean temperature than would have been indicated by experiments confined to rocks at a distance from "lodes." Upon the whole, however, I believe that the results stated in the foregoing table are sufficiently near the truth, to prove that the ratio in which the temperature augments in descending into the earth, is greater in shallow mines than in deep ones. Moreover, I am persuaded that we have no means at present within our reach to enable us to arrive at satisfactory conclusions relative to the ratio in which the temperature of the earth increases at much greater depths than have yet been attained.

It clearly appears, from what has been stated, that the conducting power of rocks is not the immediate cause of the high temperature observed in mines; but the facts are quite consistent with the hypothesis, which I have long advocated, of heat being transferred from greater or less depths towards the surface, in consequence of the well known tendency of warm water to ascend through cooler portions of that fluid *; and the anomalous results obtained at equal depths in different places, often contiguous to each other, are, I conceive, mainly to be attributed to the greater or less facilities afforded by the rocks and veins for the circulation of the water.

Note.—The experiments in Levant and Consolidated Mines marked thus ‡ in the table, were made by burying the bulbs of long thermometers three feet deep in the rocks, and compared with other thermometers near them buried only an inch deep. Under these circumstances, the former indicated about 1°½ of temperature more than the latter, proving that the high temperature was not due to extraneous causes, but existed in the rock. Such results are, however, only to be expected in the deepest parts of mines. Indeed, those obtained in the upper levels of mines are generally unsatisfactory and inconclusive, as it respects the native heat of the earth at equal depths.

[The details of Mr. Fox's observations on the temperature of mines made from 1815 to 1823, will be found in Mr. Brayley's "Account" of observations and experiments on the subject in Phil. Mag. First Series vol. lxi. p. 348, lxii. p. 38; and in Phil. Mag. and Annals, N.S., vol. ix. p. 94, appears another paper by Mr. Fox. See also Phil. Mag., First Series, vol. xxxiii. p. 320, lxvii. p. 302; and Lond. and Edinb. Phil. Mag. vol. v. p. 446.—Edit.]

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* To this property of water, and its solvent power at great depths, where the temperature is very high, I believe that many mineral deposits in veins are to be referred. I have stated my views on this subject in the Royal Cornwall Polytechnic Society's 4th Report, pp. 108 and 109, sold by Trathan, Falmouth, and Simpkin and Marshall, Stationers' Court, London.

LXXIV. *On an alleged Demonstration of* Fresnel *relative to the Wave-surface in the Theory of Double Refraction. By* JOHN TOVEY, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I AM glad to find from Mr. Lubbock's paper in your last Number, p. 417, that he is devoting a portion of his mathematical talents to physical optics. His comparison of Fresnel's ideas with those which have been since developed by M. Cauchy and others, is, in the present state of the science, of great importance. I hope he will continue these investigations; and I beg that you will allow me, through the medium of your Journal, to solicit his consideration of that part of Fresnel's theory in which it is supposed to be proved that when the *axes of elasticity* are taken for the coordinate axes, the differential equations may be reduced to the form

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

Mr. Lubbock takes this point for granted, referring for a proof of it to the remarks of Fresnel in the *Mém. de l'Institut.* To these remarks I have no access; but I conceive that they can be no more than equivalent to the demonstration of C. J. in your last volume, p. 24, which only proves, that if one molecule alone of the system be displaced, it is always possible so to take the axes of the coordinates that the differential equations may be reduced to the form

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

Now this equation is inapplicable when the system is in a state of undulation; and, therefore, unless it can be proved that it may then be changed into the form made use of by Mr. Lubbock, Fresnel's demonstration must be abandoned.

I am, Gentlemen, yours, &c.

JOHN TOVEY.

Littlemoor, near Clitheroe,
Nov. 6, 1837.

LXXV. *A Report of the Progress of Vegetable Physiology during the Year* 1836. *By* J. MEYEN, *Professor of Botany in the University of Berlin.*[*]

[Continued from p. 446.]

On the Structure and Function of Spiral Tubes.

THAT the spiral tubes in vegetables serve to conduct the nutritive sap has once more been remarked by Link[†], in

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[*] From Wiegmann's *Archiv für Naturgeschichte*, 1837, Part 3. Translated by Mr. Wm. Francis.

[†] *Philos. Bot.*, p. 189.

a very distinct manner, and a great number of new data which he had already made known in earlier writings have been treated more at large. In reply to those botanists who are of opinion that the spiral tubes convey air, because they had observed that air plainly proceeded from them, Link remarks, that the intestinal canal of animals is also not always full, but frequently contains air.

Gaudichaud * also has again confirmed a phænomenon, which had previously been described by various travellers, and which speaks very determinately for the sap conduction of the spiral tubes. For if we cut one of these Liane plants possessing great spiral tubes, and at a time when the sap is ascending, a great quantity of sap flows out from the surfaces of the section; that this sap does really proceed from the apertures of the spiral tubes has been observed by myself, and also by many other persons. Gaudichaud made his experiments on *Cissus hydrophora*, a new species which grows in the environs of Rio de Janeiro. A Liane stem of 15—18 lines in diameter was cut right through; the surfaces of the section were moist, no water ran out, excepting a few drops which fell from the upper surface. A small portion of from 15 to 18 inches was cut off from the basis of the upper end, and placed in a vertical direction, and immediately clear water came out in great quantity; various sections from the lower end of the stem exhibited the same phænomenon. The flowing out of the sap however proceeded more slowly; it just trickled down from both ends, as soon as the severed portion of the end was held in a horizontal direction. A portion of 15 inches long by from 14 to 15 in diameter was cut off from another stem of the same plant; this gave two ounces of water. From a second piece of the same length from the upper end of the stem Gaudichaud obtained rather less water; and this diminution in the flow of water became the more considerable the further the severed end was situated from the base of the stem. On the day following that on which the stem had been cut, the surface of the section of the lower end, which was still standing in the ground, exhibited no efflux of sap: the whole end from 5 to 6 inches below the surface of the section was dry. Gaudichaud also takes this occasion to mention the causes of the ascent of the sap in general; he thinks it possible to divide the forces which cause this phænomenon of vegetable life into external and internal forces. To the external forces would belong atmospheric pressure, heat, solar light, &c. The internal forces would have to be subdivided into nutritive and secretive

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* *Observ. sur l'Ascension de la Sève dans une Liane, et Déscription de cette nouvelle espèce de Cissus.—Ann. des Scien. Nat.*

forces: to the first would belong the reception of saps and of gases, the combination of gases with one another, the metamorphosis of the gases into liquids, the change of fluids into solid substances; to the latter, on the other hand, would belong the exhaling of gases, liquids, &c.

A memoir by Girou de Buzareingues* has attracted especial notice; it treats exclusively of the organs of the motion of sap in plants. The results of this work are so at variance with those of all other vegetable physiologists that we may perhaps expect a full refutation of them; however, the restricted limits of this report do not allow us to give more than a general review of it. It will be very easy for all botanists who have especially occupied themselves for many years with vegetable anatomy, to convince themselves that the observations reported by Girou de Buzareingues on the organs of the motion of sap do not *all* agree with nature. The observations it is true were made with an excellent microscope of Amici; but we must not however ascribe the faults which had crept into this work to the instrument, for I, who am also in possession of a similar instrument, see the objects quite otherwise than Girou has described and figured them. The chief blame of the discordant results of these observations might be ascribed to the mode of observation; for it appears that Girou always pressed the objects between plates of glass, and observed them in a pressed condition. We cannot sufficiently warn naturalists against the application of such pressure in microscopical observations.

Girou commences his memoir with the expression that the sap in plants ascends from the roots to the leaves, and passes from these again to the roots; that it moves from the axis to the periphery, and from this to the axis; and that there is a gaseous fluid which accompanies this sap. For the effecting of this motion of the sap the plants employ cells and vessels, and these are intercellular vessels to and off-carrying vessels. The intercellular passages (*des conduits inter-utriculaires*) are separate vessels, which are said to cause the movement of the fluids and gases in all directions (even an explanatory drawing is given in fig. 16. pl. vii. !). To the adducent vessels belong the simple vessels (*des vaisseaux unis*), by which are probably meant the fibrous cells and the tubes of the liber; and, further, the spiral tubes or tracheæ: to the reducent vessels belong, on the other hand, the false spiral tubes.

The fibre which forms the spiral tube is said to be hollow;

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* *Mém. sur la Distribution et le Mouvement des Fluides dans les Plantes.—Ann. des Scienc. Nat.* 1836. i. p. 226—248.

and to convey sap; it is further stated to be wound round a delicate tube, and to be inclosed externally by a membrane, under which is the fluid; while the inner tube round which the spiral fibre runs is said to convey air only.

These are, properly speaking, the results of Girou's observations; he however in this memoir as well as in his preceding ones never specifies the name of the plants on which he made this or that observation, and on which the observation might easily be repeated. He also never takes notice of the observations of other botanists. Towards the end of the paper Girou (*l. c.* p. 245) comes to the conclusion, that a certain circulation exists in plants; the sap ascends by means of the intercellular passages through the whole plant; it is conveyed by the adducent vessels from the root to the leaves, where it undergoes an elaboration, and it then passes into the vessels which carry it off. The sap which is contained in the spiral fibre of these vessels may descend to the root, and there in the earth serve for the purpose of excretion; but the other sap, which runs between the two membranes of the downward conveying vessels, is said to flow through the side apertures into the intercellular passages, and there mix with the ascending sap. I am very sorry to say that I could convince myself of none of these positions!

We will examine more specially the position, that the spiral fibre is hollow, for although we endeavoured many years ago to show that this question was decided in a most definite manner, yet many of the most learned phytotomists have in these latter years contended for the presence of a cavity in the spiral fibre; not only Mirbel, but also Link in his recent work. The latter considers it to be hollow, on account of some (as it appears) swollen places, as also from its appearance at the points of ramification. Link*, however, does not lay much stress on this opinion.

Mohl† has also argued against the presence of a cavity in the spiral fibre which Mirbel had assigned to the fibre in the ringed tubes of the Oleander; he says: "If the section passes exactly through the axis of the vessel, and still better, if we succeed in obtaining a thin disc-like diagonal section of the spiral fibre, we can very plainly observe that the spiral fibre consists of two layers, as it were of a central column and of a sheath. There is therefore a difference between the spiral fibre and the fibres of the dotted cells; but there is also a similarity, since it is probable that the central column is the first-formed part of the fibre, and the sheath a later de-

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* *Elem. Philos. Bot.*, p. 159.

† On Vegetable Substance, p. 29.

position on the same ... ...; I however consider thus much as certain, that the spiral fibre is not hollow." All that is here said of the fibre of the spiral tubes I also assign to the spiral fibres which are evident in the interior of the common parenchymatous cells; for I consider these formations as identical. I have also in my recent work on vegetable physiology* enumerated many other reasons which prove in the most distinct manner *that the spiral fibre is always solid.* At times it is thickened by a deposition of new layers and exhibits at times an apparent membrification.

On Observations respecting the System of Circulation in Vegetables.

It has been the lot of the doctrine of the peculiar system of circulation in the more perfect plants in the course of the last year again to sustain considerable attacks.

Link† endeavours to demonstrate by observations that the resinous ducts of the *Coniferæ* should be enumerated, in one and the same class of formations, with the milk vessels of the *Euphorbiaceæ* and *Asclepiadeæ*; although not exactly similar to one another. In young germinating *Coniferæ* Link observed near the resinous passages a peculiar membrane; but he himself says that in the greater and older vessels of this kind they seem to disappear. I have hitherto not been able to convince myself of the presence of the peculiar membrane of the resinous ducts, and even the drawings of diagonal sections which Link‡ has given to these resinous ducts exhibit no trace of a peculiar epidermis. Much easier is it to observe the origin of these resinous ducts in the young shoots of *Coniferæ*; here at least we can say with certainty that these resinous passages, even in their young state, possess no peculiar membrane; nay, even the leaves of the *Coniferæ* (particularly the leaves of *Pinus sylvestris*) exhibit a layer of peculiar cells which form the resinous duct, but no distinct simple membrane. In the paper quoted, Link advances the opinion (p. 132) that the resinous sap which fills those resinous ducts in the *Coniferæ*, appears to be in motion, for the substance flows out in great quantity and for a long time when a branch is cut off. It would certainly be a great acquisition to vegetable physiology if we could more strictly prove this opinion; but this is scarcely possible, since the vegetable parts

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[* See Bibliographical Bulletin, p. 481 of the present volume.—Edit.]

† *Element. Phil. Bot.*, i. p. 196.

‡ *Anatomie d'une Branche de* Pinus Strobus.—*Ann. des Scien. Nat.*, 1836. i. p. 129. Pl. iii. fig. 1.—Also in his *Anat. Bot. Abbildungen*, Tab. vii. fig. 1 and 5.

which contain such vessels are a great deal too thick to be immediately observed without any dissecting. Such a motion of the resin would place the receptacles nearer to the true vital sap vessels; and I consider it as highly probable that they are of an importance much greater than we have hitherto dared to ascribe to them; for the resinous ducts in the *Coniferæ*, as well as the gummy ducts in the *Cycadeæ*, form a system, of itself entire, and perhaps continuous through the whole plant; and it is exactly in those plants where these resinous ducts occur that the vital sap-vessels are wanting. A great coincidence may also frequently be proved, between the saps of the gummy passages and the vital sap-vessels of various plants, in a chemical point of view.

Link remarks on the milk vessels of the *Euphorbiaceæ* and *Asclepiadeæ*, that they stand singly in the stem, are straight and simple, and appear ramified only in the young stems where they run out towards the leaves; they were also observed in shrubby *Euphorbiæ* with spreading branches; sometimes they keep on their course at some distance from the nerves. Link further says that they terminate with an obtuse point; they also exhibit no anastomoses, nay at times they appear to have transverse partitions, but only false ones. These observations, it is true, do not exactly coincide with those which I mentioned in the report for 1835, with a view to refute the objections of Treviranus. I yet hope to succeed in giving to many of them quite a different bearing. In no plant is it more easy to observe than in the leaves of *Hoya carnosa* that the ramified and very thick membranaceous vessels with obtuse ends traverse the diachyma; these vessels, however, are not milk-sap vessels, but they are ramified cells of the liber, or fibrous vessels of which hitherto no mention has been made in botanical writings. A structure so highly remarkable belongs to the fibrous vessels (fibrous cells) of the *Asclepiadeæ* and *Apocyneæ*, of which mention has already been made. But nowhere is the ramification and anastomosis of the vessels of the stem to be observed more evidently and frequently than in the stem of the old genus *Sarcostemma*; here we find the regular and manifold anastomosing tissue of the milk vessels deposited immediately before the layer of the cells of the liber, which exhibit in every respect one and the same structure with those ramified vessels in the leaves of the *Hoya*, with the exception that ramification is wanting in them. These observations evidently show in the most certain manner that Mirbel's statement* that the cells of the liber in *Nerium*, where the ap-

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* *Vide* our Year's Report for 1835.

pearance is quite similar, are to be considered as milk vessels, cannot be right. I have not yet been able to succeed in observing in *Ficus elastica* the closed ends of the milk vessels, neither could I see partitions in these vessels; but I have seen real anastomoses, even in *Chelidonium majus* and in many others.

That the sap in the milk vessels moves had already been previously confirmed by Link, and it has again been observed by him; and he most admirably remarks, that this motion is neither effected by the contraction of the vessels, nor by the motion of the molecules contained in the sap, since observation does not show it.

From various travellers* who have remained for some time in Columbia, several notices have appeared, from which it is probable that in the districts in question there occur many other kinds of trees which produce a milk similar to that of the celebrated Cow-tree †, respecting which von Humboldt has in the accounts of his Travels (chap. xvi. and xxvi.) given us such highly interesting communications.

Mornay has again diffused through the medium of the journals very interesting notices on *Euphorbia phosphorescens*, with the inflammable milk This shrub grows near St. Francisco in Alagoas in Brazil, in impenetrable thickets, covering, perhaps, more than 1000 square feet. According to the statement of the natives, it sets light to itself, throws out for a long time an immense column of dense smoke, and finally breaks out into bright flames ‡.

On the Secretory Organs of Vegetables.

L. Griesselich § has with great propriety addressed some admonitory hints to vegetable physiologists on the subject of our defective knowledge relative to the structure and importance of glands; he also observes that even what De Candolle has reported in his celebrated physiological works is unfortunately not adapted for throwing any light on this subject. He also cites various passages from those works which satisfactorily prove this. Griesselich's statements respecting this subject are, however, not founded, any more than De Candolle's, upon personal observations with the compound microscope; if, therefore, there is nothing new in the memoir, yet it has the merit of having directed attention to a subject so

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* Loudon's Gardener's Magazine, 1836. No. 71, p. 100.

† [See Sir R. K. Porter's description of this remarkable tree in the present volume of Phil. Mag., p. 452.—Edit.]

‡ [Mr. Mornay's first notice of this plant will be found in Phil. Mag., First Series, vol. xlviii., p. 423.—Edit.]

§ On the Glands on the Leaves of the *Labiatæ*, and on the odoriferous constituent parts occurring in them.—Botanical Papers, part i. Carlsruhe, 1836. 8vo.

much neglected. The Royal Society of Sciences of Göttingen, sensible also of the defective state of our knowledge of vegetable glands, has chosen this subject for a prize question, which I have endeavoured to answer*.

Griesselich names the oil-bearing glands which occur so frequently in (not *on*) the substance of the leaves of the *Labiatæ*, pores; a term not particularly to be praised, as it might cause a confusion of ideas, and, secondly, it must be placed after those which we already possess. Together with Guettard's denomination (*glandes vésiculaires*), the name, internal glands, has been used by many phytotomists; this is very proper, and therefore should be retained, for this is the only kind of compound glands which occur in the cellular tissue of plants. Griesselich considers these inner glands as mere receptacles of a secreted substance, a view which is refuted by the anatomical examination of them. What is said on the occurrence of internal glands in *Labiatæ* has already been mentioned by Guettard †; nay, the latter has written much more on this subject than will be found in the paper now before us; unfortunately, however, Guettard's memoir has remained almost unknown.

Labiatæ cultivated in gardens contain, according to Griesselich's observations, fewer internal glands than wild species; this however can only relate to a smaller production of the secreted oils; the glands are present in as large a number. Guettard had already remarked, that in many of these plants we could observe such glands in dried specimens which in a fresh state exhibited none.

Besides these internal glands, we find also external, but simple glands, in the *Labiatæ*, which I have mentioned in the Göttingen prize essay.

On the Reception of Sap, the Secretion and Nutrition of Vegetables.

Many very interesting experiments have been made on the nutrition of vegetables; and it is to be hoped that we may soon arrive at definite and generally received views on this subject also. Unger ‡ for one, has given a very complete enumeration and comparison of the experiments and views of botanists and chemists who have treated on the reception and the formation of the nutritive substance in vegetables. The question, in fact, is, whether the vital principle of the plant is of itself capable of forming the organic substances which serve

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* Meyen, On the Secretory Organs of Plants. Berlin, 1837. 4to. With 9 tab. of microscopical drawings.

† *Observations sur les Plantes.* Paris, 1757. 2 vol. 8vo.

‡ Influence of the Soil on the Diffusion of Plants. Vienna, 1836, p. 125, &c. [See our present Number, p. 564.—Edit.]

to nourish the plant; or whether these nutritive substances are taken up, at least in their elements, from without. Unger (*l. c.* p. 136) finally arrives at the conclusion, "that the process of vegetation is neither able to produce new elementary substances out of the substances presented to it, nor even to arrange those already present; from this, however, it immediately follows indirectly, that plants also must necessarily take up their inorganic substances, such as carbon, hydrogen, oxygen, and nitrogen, from the external world."

Jablonski * has once more endeavoured to prove by accurate experiments, that the inorganic substances which plants contain are received from without. In order to refute the well known experiments of Schrader, which were to prove from observations that the process of vegetation was able to form alkalies, earths and metals, Jablonski performed similar experiments, which afforded the following result. The flowers of sulphur, which were also employed in these observations, were purified before the experiments by digesting them in muriatic acid, and it was proved by this operation that a quantity of oxide of iron, silica and lime was mixed with the flowers of sulphur! In perfectly clean flowers of sulphur, the seeds of various plants were sown, but they attained only a very low state of development, even when they were watered with water containing carbonic acid. The Dicotyledones developed slowly their cotyledons, but the *plumula* exhibited no inclination to lengthen itself; and after from three to four weeks, all the plants were dead.

Jablonski then made the same experiments with flowers of sulphur purchased as pure from a druggist; these on being burnt left behind 4 per cent. of a carbonaceous mass which gave 1⅓ per cent. of ashes, of oxide of iron, lime and silica. Cabbage seeds which were sown in these flowers of sulphur soon germinated, and attained a height of 4 inches above the sulphur, till at last they died between the 7th and 10th week, without having increased within the three latter weeks in any perceptible degree. This last experiment terminated exactly in the same way as the experiments of Lassaignes, whose plants of buckwheat in cleansed sulphur put forth in fifteen days stems six centimetres high. Lassaignes at that time analysed the plants thus sown, and found their ashes to have exactly the same composition with a quantity of the seed equal to that from which the plants had grown.†

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* Contribution to the solution of the question, whether by the process of vegetation bodies chemically indecomposable can be formed?—Wiegmann's *Archiv*, 1836, p. 206—212.

[† The entire series of researches on this subject recited above bears importantly on that of Mr. Reade's paper on the solid materials of the ashes of plants, in our last Number, p. 413.—Edit.]

Jablonski draws from his experiments the conclusion that the plants continued to live only so long as the nutritive substances deposited in the albumen or in the cotyledons could go through the chemical process necessary to vegetable life; as soon however as their combinations had arrived at a relative chemical neutrality, death was inevitable, and carbonic acid and water did not appear to be adapted to the sustenance of the new product from the organic substances.

From these observations we come immediately to those which have been made on the reception of various substances by the roots of plants. Mr. G. Towers* has once more made some experiments in order to ascertain whether coloured fluids can be taken up by the roots in their natural state; but neither infusions of log-wood nor of Brazil wood were absorbed by the plant, and this served to confirm the observations of Link and other German botanists. Towers employed for these experiments plants of balsam; and soon after Unger† made similar experiments on *Lemna minor*, which he grew in tincture of cochineal, with and without an addition of alum, and in an infusion of log-wood, but he could never observe the reception of the coloured fluid. The *Bibliothèque Universelle de Génève*‡ gave an extract from Towers's experiments, and complains that he had paid no attention to the labours of preceding naturalists, who had proved that plants, even with their roots in the natural state, did take up coloured fluids. The observations of De Candolle, sen., are here mentioned, according to which coloured liquids had penetrated through the spongioles. However these imperfect notices of De Candolle are contrary to a vast number of negative observations which I, among others, have performed yearly. But already, long before the appearance of De Candolle's Physiology, H. Schultz of Berlin had made known that he had observed coloured liquid imbibed by a *Chara*. The observation is related in a very detailed manner; yet I have never been successful in repeating it, although I have made similar experiments with a great quantity of *Chara*. Setting aside this single case observed by Schultz in *Chara*, we are able to infer, from unexceptionable observations which we possess, that the colouring substance in the coloured fluids is not finely enough divided to pass through the cellular tissue of vegetables, and that hence it is not taken up by the plant when uninjured. On the other hand, it has been proved by various

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* Transact. of the Horticult. Soc. of London, Sec. Ser. vol. ii. Part I. p. 41.—*Bibliothèque Universelle de Génève*, No. 5, 1836.—*Ann. des Scien. Nat.*, 1836, ii. p. 228.—Froriep's *Notizen*, No. 1078, Sept. 1836.

† Influence of Soil on Diff. of Plants, p. 149.

‡ *Nouv. Ser.* vol. i. *Mai* 1836.

experiments that substances perfectly dissolved, as for instance solutions of salts, even if they are the most deadly poisons, pass through the cellular tissue of plants. Towers and Unger have also performed various experiments as to this point; the first watered balsams with a solution of *iron in muriatic acid*, and although this had entered the plant, yet even after sixteen days the plant had not at all suffered. It is well known that Link had previously performed similar experiments with *prussiate of potash* and *sulphate of iron*, and obtained the same results; and Treviranus has, quite without reason, called in question those results of Link's experiments; for I have also succeeded with many experiments of the same kind, in obtaining similar results.

Towers placed also several balsams with the roots cut off in the solution of iron, and found that they soon died in it, a result which was also well known from earlier experiments of German botanists. Towers concludes from his experiments that plants in their natural state can take up a substance without injury, which under other circumstances causes death: this conclusion is however too hasty, as Unger's more complete experiments have demonstrated; these will be given below.

Thos. And. Knight* endeavours to call in question the opinion that the spongioles of the root are the organs which imbibe the nutritive sap from the soil, and send it forward to the other parts of the plant: they were too imperfectly organized. Knight says that he had shown that the nutritive sap in trees ascends only through the young wood or alburnum, and since the spongioles of the root possess no woody fibre, it must evidently be other canals, &c. which take up the sap; besides, the young wood is formed very early, long before the stem and branches are developed. He is convinced that portions of alburnous fibre have been erroneously supposed in the spongioles of the root. (The author here probably alludes to the observations of De Candolle!) It is true we are still in want of an accurate demonstration of the connection of the spongioles of the root with those elementary organs which convey the sap taken up by them onwards; but that the spongioles of the root, where they are present, take up the nutritive sap in the same manner as the finest fibres of the root, is a fact that can no longer be called in question.

Unger (l. c. p. 147) grew several plants of *Lemna minor*

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* Upon the supposed absorbent powers of the cellular points, or spongioles, of the roots of trees, and other plants. Trans. of the Horticult. Soc. of London, Sec. Ser. vol. ii. p. 117, [or Lond. and Edinb. Phil. Mag., vol. x. p. 488.] I was obliged to make use of the French translation, the English original not having yet arrived at Berlin.

in four ounces of water in which three grains of sugar of lead were dissolved; so soon as on the eighth day they became paler, and here the decoloration began from the root. From the third day these plants were placed in pure water, but the poisoning was so complete that they began to die as early as the fifth day. Repeated experiments showed that even within twenty-four hours so considerable a quantity of the salt of lead had been imbibed that sulphuret of ammonia indicated the presence of the metal by the brown tint. It was thus proved that in *Lemna* not only the little roots imbibe but also the leaves, and indeed the under surface in as high a degree as the upper one. This phænomenon is, however, as I think, quite common, even in the most perfect terrestrial plants, in a higher degree, however, in the imperfectly organized aquatic plants, which consist solely of parenchyma, and in which this has been demonstrated by various experiments. Unger thinks rather that the foreign substances which are taken in pass through the cellular partitions than that they mix with the cellular sap; on the other hand, however, my own experiments made on *Lemna* with salts of iron, as also on plants of balsam and maize with prussiate of potash, show that the dissolved substance imbibed mingles with the cellular sap. But if we act with reagents on such cells, the coloured substances proceeding from the action are almost all precipitated upon the partitions of the cells and on the molecules of the cellular sap.

These experiments on the reception of dissolved foreign matters through the cellular tissue were performed by Unger chiefly in order to see whether a secretion of the imbibed substances again took place through the root. Various experiments decidedly proved that the *Lemna* plants secreted neither the metallic salt nor the sulphuret of ammonia which they had taken up, and I can state the same thing of the imbibed sulphate of iron and the prussiate of potash. Plants of *Lemna trisulca* which were charged with one of these substances, and plants which had taken up the other, were placed in a glass with clean water: they continued to grow for some days, but exhibited no reaction in the water.

It is well known that the experiments of Macaire and Daubeny* went to prove such a secretion of the foreign substances imbibed by means of the root, yet in all their experiments we are left in uncertainty as to whether the roots were uninjured; the contrary must even be supposed.

In all these experiments, especially when acrid substances such as vitriol are given to the plant to imbibe, they suffer

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[* A notice of Prof. Daubeny's experiments will be found in Lond. and Edinb. Phil. Mag., vol. iv., p. 52.—Edit.]

very much; if in the earth, the roots generally first die off, and then sometimes little air-roots become developed on the stem. This phænomenon seems to be very common when the roots are in a suffering state, or if they cannot properly develop themselves: thus Jablonski (*l.c.* p. 211) saw the stem of a cabbage plant which grew in a very imperfect state in purified flowers of sulphur, send forth several similar air-roots; and I have noticed it in balsams and in maize, &c. when the roots had been destroyed in the earth by insects.

Dutrochet* has again published several observations on the respiration of plants, which are of very high interest; it is, however, not every physiologist that might be inclined to agree with the conclusions which Dutrochet has deduced from his experiments. We will first give a general notice of the observations from which Dutrochet set out; for he is of opinion that the glands of the epidermis, as Amici is said to have demonstrated, tend to close their stomata as soon as they are brought into contact with water. However, I have not been able to confirm this alleged observation, and hence some doubts may be had respecting the conclusions founded on it. Dutrochet had previously published an observation to show that the air in the air-receptacles of *Nymphæa lutea* were richer in oxygen the nearer they were to the leaves; whence might be inferred that the oxygen was impelled from the leaves through all the respiratory organs. On the other hand, I will mention an observation which contradicts this; for if on a hot day we place a hardy specimen of *Calla æthiopica* partly under water, and cut some of the petioles of the leaf off just above the surface, we are able to observe that from the influence of the solar light, a great quantity of air continually flows from the divided air-receptacles; but this air appears also to be exceedingly abundant in oxygen, for ignited charcoal shines much the brighter in it.

Dutrochet placed a severed leaf of a *Nymphæa* under water and observed how it disengaged oxygen, by the action of solar light, only from the divided apertures of the air-ducts of the petiole; he remarked the same circumstance in severed leaves of *Hydrocharis Morsus ranæ*, *Potamogeton sericeum* and *Myriophyllum spicatum*. This latter plant lives entirely under water, and possesses no stomata. But if we allow the leaves of *Nymphæa* and *Hydrocharis* to float as in their natural state on the surface of the water, the expiration of oxygen ceases at the sected air-ducts of the stem. Does this disengagement of gas cease if the ends of the divided leaf-stalk are bent upwards? If on the contrary severed leaves of *Nymphæa* were

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* *Recherches sur la Respiration des Végétaux.—Institut*, 1836, p. 358.

exposed to the influence of the sun under water, the disengagement of the oxygen from the divided air-ducts soon ceased; it began however anew if the leaves were again placed in their natural position.

Dutrochet finally infers from his various experiments that plants at night absorb the oxygen from the air, and that this is only an auxiliary respiration, while the true process of respiration of vegetables consists in the disengagement and diffusion of the oxygen in the interior of the plant caused by the solar light.

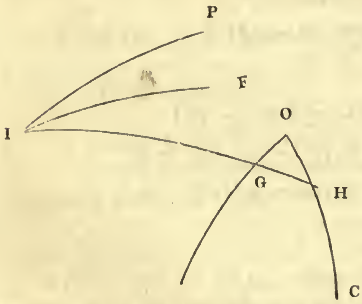
Morren*, who made several experiments in the botanical garden at Louvaine on the respiration of plants, observed on the 18th of May, during the great solar eclipse, that the respiration of the green parts of plants, that is the expiration of oxygen, entirely ceased at that time. We can observe something very similar to this on very warm summer days, when for instance the respiration of oxygen is very considerable from the action of solar light, and the sun all at once disappears under dark clouds; I have noticed several times how soon the disengagement of gas bubbles diminished, and at last ceased more or less completely.

[To be continued.]

LXXVI. *Analytical Development of Fresnel's Optical Theory of Crystals.* By J. J. Sylvester, *Member of St. John's College, Cambridge.*

[Continued from p. 469.]

Cor.—Hence we may reduce the discovery of the two fronts into which a plane front is refracted on entering a crystal to the following trigonometrical problem.



Let a sphere be described about any point in the line in which the air front intersects the plane of incedence. Let the great $\odot^{e}$ P I denote the latter plane, I F the former, O A, O C also great circles, the planes of single velocity. Suppose I G H to be one of the refracted fronts intersecting O A, O C in G and H, then

$$\frac{(a^2+c^2)-(a^2-c^2)\cos(G+H)}{2\ (\text{vel. in air})^2}=\frac{\sin\ (P\,I\,F)^2}{(\sin P\,I\,G\,H)^2}.$$

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* *L'Institut* 1836, p. 416.

The double sign will give rise to two positions of the refracted front I G H.

The propositions which follow are perhaps more curious than immediately useful.

PROPOSITION 10.

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To determine the po~~r~~tion of a line of vibration in terms of the two velocities of its corresponding front.

We have here to determine the quantities $\frac{y_{\prime}}{x_{\prime}}\ \frac{z_{\prime}}{x_{\prime}}$ (of Prop. 1) in terms of $v_{\prime}$, $v_{\prime\prime}$, or on putting $x_{\prime}^2 + y_{\prime}^2 + z_{\prime}^2 = 1$, $x_{\prime}\, y_{\prime}\, z_{\prime}$ are to be found in terms of $v_{\prime}$, $v_{\prime\prime}$

By Prop. (3.) $x_{\prime} : y_{\prime} : z :: \frac{l}{a^2 - v_{\prime}^2} : \frac{m}{b^2 - v_{\prime}^2} : \frac{n}{c^2 - v_{\prime}^2}$

and by Prop. (5.) $l^2 : m^2 : n^2 :: \overline{b^2 - c^2} \,.\, \overline{a^2 - v_{\prime}^2} \,.\, \overline{a^2 - v_{\prime\prime}^2}$

$$: \overline{c^2 - a^2} \,.\, \overline{b^2 - v_{\prime}^2} \,.\, \overline{b^2 - v_{\prime\prime}^2}$$

$$: \overline{a^2 - b^2} \,.\, \overline{c^2 - v_{\prime}^2} \,.\, \overline{c^2 - v_{\prime\prime}^2}$$

$$\therefore\ x_{\prime}^2 \quad : \quad y_{\prime}^2 \quad : \quad z_{\prime}^2$$

$$:: (b^2 - c^2)\frac{a^2 - v_{\prime\prime}^2}{a^2 - v_{\prime}^2}(c^2 - a^2)\frac{b^2 - v_{\prime\prime}^2}{b^2 - v_{\prime}^2} : (a^2 - b^2)\frac{c^2 - v_{\prime\prime}^2}{c^2 - v_{\prime}^2}$$

Let α, β, γ be the $<$es made by the given line of vibration with the elastic axes, then

$$(\cos\alpha)^2 = \frac{x_{\prime}^2}{x_{\prime}^2 + y_{\prime}^2 + z_{\prime}^2}$$

$$= (b^2 - c^2)(a^2 - v_{\prime\prime}^2)(b^2 - v_{\prime}^2)(c^2 - v_{\prime}^2)$$

divided by

$$\left\{\begin{array}{l} (b^2 - c^2)(a^2 - v_{\prime\prime}^2)(b^2 - v_{\prime}^2)(c^2 - v_{\prime}^2) \\ \quad + (c^2 - a^2)(b^2 - v_{\prime\prime}^2)(c^2 - v_{\prime}^2)(a^2 - v_{\prime}^2) \\ \quad + (a^2 - b^2)(c^2 - v_{\prime\prime}^2)(a^2 - v_{\prime}^2)(b^2 - v_{\prime}^2) \end{array}\right\}$$

and therefore

$$= \frac{(b^2 - c^2)(a^2 - v_{\prime\prime}^2)(b^2 - v_{\prime}^2)(c^2 - v_{\prime}^2)}{(v_{\prime}^2 - v_{\prime\prime}^2)(a^2 - b^2)\ (b^2 - c^2)(c^2 - a^2)}$$

(where it is to be observed that the reduction of the denominator is simply the effect of a vast heap of terms disappearing under the influence of contact with the magic circuit $\overline{a^2 - b^2}$,

$\overline{b^2 - c^2}, \overline{c^2 - a^2}$, a simpler instance of which was seen in proposition 5.)

In fact the coefficient of $v^4 . v^2$

$$= (b^2 - c^2) + (c^2 - a^2) + (a^2 - b^2)$$
$$= 0$$

that of $v_{\prime}^2 . v_{\prime\prime}^2 = (c^2 + b^2) . (c^2 - b^2)$

$$\begin{aligned} &+ (a^2 + c^2) . (a^2 - c^2) \\ &+ (b^2 + a^2) . (b^2 - a^2) \\ = &(c^4 - b^4) + (a^4 - c^4) + (b^4 - a^4) \\ = &0. \end{aligned}$$

The term in which neither $v_{\prime}$ nor $v_{\prime\prime}$ enters

$$= a^2 b^2 c^2 \{(b^2 - c^2) + (c^2 - a^2) + (a^2 - b^2)\}$$
$$= 0.$$

The coefficient of

$$- v_{\prime}^2 = a^2 . (b^4 - b^4) + b^2 . (c^4 - a^4) + c^2 . (a^4 - b^4)$$

and that of

$$v_{\prime\prime}^2 = b^2 c^2 . (c^2 - b^2) + c^2 a^2 . (a^2 - c^2) + a^2 b^2 . (b^2 - a^2)$$

each of which $= (a^2 - b^2) . (b^2 - c^2) . (c^2 - a^2)$

Hence,

$$(\cos \alpha)^2 = \frac{v_{\prime}^2 - b^2}{v_{\prime}^2 - v_{\prime\prime}^2} . \frac{(a^2 - v_{\prime\prime}^2)(c^2 - v_{\prime}^2)}{(a^2 - b^2)(a^2 - c^2)},$$

in like manner $(\cos \beta)^2 =$ &c.

and $$(\cos \gamma)^2 = \frac{v_{\prime}^2 - b^2}{v_{\prime}^2 - v_{\prime\prime}^2} . \frac{(c^2 - v_{\prime\prime}^2)(a^2 - v_{\prime}^2)}{(c^2 - b^2)(c^2 - a^2)}.$$

PROPOSITION 11.

$\varepsilon_{\prime}\, \varepsilon_{\prime\prime}$ being the $<$es between any line of vibration and the optic axes, required the velocity due to that line in terms of $\varepsilon_{\prime}\, \varepsilon_{\prime\prime}$.

By analytical geometry,

$$\cos \varepsilon_{\prime} = \cos \alpha . \cos \phi_{\prime} + \cos \gamma . \cos \psi_{\prime}$$
$$\cos \varepsilon_{\prime\prime} = \cos \alpha . \cos \phi_{\prime} - \cos \gamma . \cos \psi_{\prime}$$

$$\therefore \cos \varepsilon_{\prime} . \cos \varepsilon_{\prime\prime} = (\cos \alpha)^2 (\cos \phi_{\prime})^2 - (\cos \gamma)^2 (\cos \psi_{\prime})^2$$

$$= \frac{v_{\prime}^2 - b^2}{v_{\prime}^2 - v_{\prime\prime}^2} . \left\{ \frac{\overline{a^2 - v_{\prime\prime}^2} . \overline{c^2 - v_{\prime}^2} - \overline{c^2 - v_{\prime\prime}^2} . \overline{a^2 - v_{\prime}^2}}{(a^2 - c^2)^2} \right\}.$$

$$= \frac{v_{\prime}^2 - b^2}{v_{\prime}^2 - v_{\prime\prime}^2} . \frac{(a^2 - c^2)(v_{\prime\prime}^2 - v_{\prime}^2)}{(a^2 - c^2)^2}$$

$$= \frac{b^2 - v_{\prime}^2}{a^2 - c^2}.$$

Hence $v_{,}^2 = b^2 - \overline{a^2 - c^2} . \cos \varepsilon_{,} \cos \varepsilon_{,,}$,

and in like manner, for the *conjugate* line of vibration.

$$v_{,,}^2 = b^2 - (a^2 - c^2) \cos \varepsilon_{,}' . \cos \varepsilon_{,,}'.$$

PROPOSITION 12.

To find $\varepsilon_{,} \varepsilon_{,,}$ in terms of $\iota_{,} \iota_{,,}$

$$(\cos \varepsilon_{,})^2 + (\cos \varepsilon_{,,})^3$$

$$= 2 (\cos \alpha)^2 . (\cos \phi_{,})^2 + (2 \cos \gamma)^2 . (\cos \psi_{,})^2$$

$$= \frac{v_{,}^2 - b^2}{v_{,}^2 - v_{,,}^2} 2 \left\{ \frac{\overline{a^2 - v_{,,}^2} . \overline{c^2 - v_{,}^2} + \overline{c^2 - v_{,,}^2} . \overline{a^2 - v_{,}^2}}{(a^2 - c^2)^2} \right\}$$

but by Prop. (9)

$$v_{,}^2 = a^2 \left(\sin \frac{\iota_{,} - \iota}{2}\right)^2 + c^2 \left(\cos \frac{\iota_{,} - \iota}{2}\right)^2$$

$$v_{,,}^2 = a^2 \left(\sin \frac{\iota_{,} + \iota}{2}\right)^2 + c^2 \left(\cos \frac{\iota_{,} + \iota}{2}\right)^2$$

$$\therefore (\cos \varepsilon_{,})^2 + (\cos \varepsilon_{,,})^2 = \frac{b^2 - v_{,}^2}{(a^2 - c^2) \sin \iota_{,} . \sin \iota_{,,}}$$

multiplied by

$$\frac{2 \left\{ \overline{a^2 - c^2}^2 \left(\left(\cos \frac{\iota_{,} + \iota_{,,}}{2}\right)^2 \left(\sin \frac{\iota_{,} - \iota_{,,}}{2}\right)^2 + \left(\cos \frac{\iota_{,} - \iota_{,,}}{2}\right)^2 \left(\sin \frac{\iota_{,} - \iota_{,,}}{2}\right)^2 \right) \right\}}{(a^2 - c^2)^2}$$

$$= \frac{b^2 - v_{,}^2}{(a^2 - c^2) \sin \iota_{,} . \sin \iota_{,,}} \left\{ (\sin \iota_{,})^2 + (\sin \iota_{,,})^2 \right\}$$

and we have seen that

$$\cos \varepsilon_{,} \cos \varepsilon_{,,} = \frac{b^2 - v_{,}^2}{a^2 - c^2}$$

$$\therefore \cos \varepsilon_{,} + \cos \varepsilon_{,,} = \sqrt{\left(\frac{b^2 - v_{,}^2}{a^2 - c^2}\right)} . \frac{\sin \iota_{,} + \sin \iota_{,,}}{\sqrt{\sin \iota . \sin \iota_{,,}}}$$

$$\cos \varepsilon_{,} - \cos \varepsilon_{,,} = \sqrt{\frac{(b^2 - v_{,}^2)}{a^2 - c^2}} . \frac{\sin \iota - \sin \iota_{,,}}{\sqrt{\sin \iota . \sin \iota_{,,}}}$$

$$\therefore (\cos \varepsilon_{,}) = \sqrt{\left\{ \frac{b^2 - v_{,}^2}{a^2 - c^2} . \frac{\sin \iota_{,}}{\sin \iota_{,,}} \right\}}$$

$$(\cos \varepsilon_{,,}) = \sqrt{\left\{ \frac{b^2 - v_{,}^2}{a^2 - c^2} . \frac{\sin \iota_{,,}}{\sin \iota_{,}} \right\}}$$

and in like manner

$$(\cos\varepsilon_{\prime}') = \sqrt{\left\{\frac{b^2 - v_{\prime\prime}^2}{a^2 - c^2} \cdot \frac{\sin\iota_{\prime}}{\sin\iota_{\prime\prime}}\right\}}$$

$$(\cos\varepsilon_{\prime\prime}') = \sqrt{\left\{\frac{b^2 - v_{\prime\prime}^2}{a^2 - c^2} \cdot \frac{\sin\iota_{\prime}}{\sin\iota_{\prime\prime}}\right\}}$$

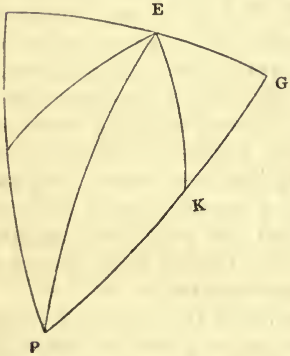
$v_{\prime}\, v_{\prime\prime}$ for the sake of neatness are left *unexpressed* in terms of $\iota_{\prime}\, \iota_{\prime\prime}$.

This is the simplest form by which the position of the lines of vibration can be denoted.

Corollary.

From the last proposition it appears that

$$\frac{\cos\varepsilon_{\prime}}{\cos\varepsilon_{\prime\prime}} = \frac{\sin\iota_{\prime}}{\sin\iota_{\prime\prime}}.$$



Hence we may construct geometrically for the two planes of polarization.

Let I K be the projections of the two optic axes on a sphere, E the projection of the normal to the front, P the projection of one line of vibration; then

$$\frac{\cos PK}{\cos PI} = \frac{\sin KE}{\sin IK}$$

Draw F E G the ⊙^e of which P is the pole, meeting P K, P I produced in G and F.

Then $\cos PK = \sin KG$, and $\cos PI = \sin IF$,

$$\therefore \frac{\sin KG}{\sin FI} = \frac{\sin KE}{\sin IE}$$

$$\therefore \frac{\sin KG}{\sin KE} = \frac{\sin IF}{\sin IE}$$

$$\therefore \sin KEG = \sin IEF$$

$\therefore KEG = IEF$ or $180 - IEF$. But $PEF = PEG$

$\therefore$ E P bisects either the $<$e I E K or the supplement to it.

These two portions of E P give the two planes of polarization. The construction is the same as that given in Mr. Airy's tracts, and originally proposed, I believe, by Mr. Maccullagh.

[To be continued.]

LXXVII. *Letter to* Richard Taylor, *Esq., one of the Editors of the Lond. and Edinb. Philosophical Magazine, occasioned by* M. Melloni's *paper on the Polarization of Heat in the* Annales de Chimie *for May* 1837. *By* JAMES D. FORBES, *Esq., Prof. of Nat. Phil. in the University of Edinburgh.*

MY DEAR SIR,

YOU may recollect last year at Bristol your having mentioned to me your intention of translating a paper of M. Melloni's in which my experiments on polarized heat were referred to, and having wished to know whether I chose that any remarks from me should accompany it in the "Scientific Memoirs*;" I replied that though I conceived that M. Melloni had not dealt quite fairly by me or my experiments, yet such was my abhorrence of scientific controversy that nothing short of necessity should ever cause me to enter into it. I still retain the same sentiments; I still think that the opinion of those whose opinion is alone worth having on such abstract and unfamiliar points, will be decided by an examination of the original memoirs in which the experiments are contained; to which memoirs I have given as extensive a circulation as I conveniently could. In their hands I very willingly leave my claim to the establishment of the chief facts of the polarization of heat as far as yet known; and it happens, fortunately for me, that the hostility with which my results were at first received by those who have since taken the trouble to examine and confirm them, prevents any, even the slightest, question respecting priority in any part of the inquiry.

I have, however, more than once taken the liberty of using your journal as a vehicle for combating any *definite* objection urged to the accuracy of my experiments. This is all that I mean to do in the present letter.

In the *Annales de Chimie* for May 1837 (but which has only very recently appeared) M. Melloni states the results at which he has arrived by the use of my methods of polarizing heat, which he seems to have applied with great skill and with his usual attention to minute accuracy. Whilst he amply confirms my results as to *phænomena* generally, he finds a marked difference in the quantitative *measures* made on the amount of heat from different sources polarized by a given pile of mica. He endeavours to explain the difference in our results by pointing out a source of error in mine. That this supposed source of error has no recognisable existence, but is one of those infinitesimal objections with which my experiments have been from the first assailed, I shall now endeavour to show.

M. Melloni supposes that the secondary heat radiated from

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* Scientific Memoirs, vol. i. p. 325.

the mica plates to the thermal pile affects my results; and that as more *dark* heat than *bright* heat is absorbed by mica, a greater share of the total effect is produced by this cause in the first case than in the second. Since this secondary effect is unchanged when the mica plates are perpendicular and parallel, there appears to be less heat polarized in the first case than in the second. In support of this opinion M. Melloni quotes one of my earliest and rudest experiments, in which the effect of absorbed heat was abundantly obvious, and which could form no certain basis of *quantitative* deductions.

M. Melloni does not appear to have observed that in my later *quantitative* experiments, (which he likewise quotes,) the supposed cause of error was eliminated by observing the dynamical effect, or the extreme arc of impulsion of the needle (a method which I borrowed from M. Melloni), and that (the screen which was removed in order to let the heat act upon the pile being *between the source of heat and the mica plates*,) the mica plates were only absorbing heat during the few seconds that the arc of impulsion was being observed. I have adopted M. Melloni's mode of ascertaining the inefficiency of the absorbed heat, (which is the same as I suggested at the very commencement of my experiments, for the satisfaction of those who on the very same grounds denied the polarization of heat altogether,) and I find the effect of absorption to be utterly inappreciable, whether for *dark* or *bright* heat.

It is clear that some other mode must be adopted to account for the discrepancy of our results. From M. Melloni's known accuracy and address in the use of the thermo-multiplier I can hardly doubt the correctness of the results he has obtained. I am also tolerably confident as to my own. Perhaps the difference may lie in his having used more plates, and at greater inclinations, to produce polarization; which of course will always tend to bring to uniformity of state rays of heat, however differently susceptible of polarization by a given plate at a given angle, which is all the distinction I ever meant to draw when I used for brevity the phrase "polarizable nature" of any kind of heat.

Though I cannot help saying that it seems to me that those who have attended to the memoirs hitherto published on this subject will find almost nothing new either in methods or results in M. Melloni's very long memoir now alluded to, still I feel very grateful to him for having, by delicate and judicious arrangements of the apparatus, confirmed the results already obtained, on a scale which may adapt them for class experiments, and which can leave no doubt respecting them, even on the minds of those least accustomed to weigh physical evidence.

As I refer to my own papers as the only answer which I mean to give to the other parts of M. Melloni's memoir, I will add that the *first* appeared in the Edinb. Trans., vol. xiii., and Lond. and Edinb. Phil. Mag., vol. vi.; the *second* (containing the more accurate measures) in the Edinb. Trans., same volume, but it has not been reprinted.

I am, my dear Sir, yours very faithfully,

Edinburgh, 15th Nov. 1837. JAMES D. FORBES.

LXXVIII. *On the peculiar Chemical Inactivity of Bismuth, with reference to the researches of* Dr. Andrews; *and on the action of Seawater on Iron, &c. By* Professor SCHŒNBEIN.*

THE short notice contained in the last number of the *Biblioth. Univers.* respecting Dr. Andrews's researches on the action of nitric acid upon bismuth,† has induced me to make some experiments on the same subject, and I now take the liberty to give you a short account of the results obtained from them. It certainly cannot be denied that there exists some analogy between the peculiar condition of iron and that of bismuth, but my impression at present is, that the cases are similar, but not identical. This opinion is founded upon the following facts: The chemical action of iron upon nitric acid, as is now well known, can be entirely stopped by a variety of ways, whilst according to my experiments it is impossible to obtain such a result with bismuth. I voltaically associated this metal with all the substances known to be capable of rendering iron completely inactive, but by so doing I could never succeed so far as to prevent bismuth from being chemically acted upon by nitric acid. It is true, by putting in contact the metallic body in question with platinum, the chemical action of nitric acid, sp. gr. 1·4, may be reduced to such a low degree of intensity that no visible disengagement of binoxide of nitrogen takes place, and the piece of bismuth (immersed in nitric acid) assumes a bright appearance.

But the oxidable metal being in this state is nevertheless uninterruptedly attacked by the acid fluid, as may be easily shown by having recourse to the galvanometer. There are besides some other facts, which put the continuance of chemical action in the circumstances mentioned beyond any doubt. I think I have first ascertained the remarkable fact, that iron can be rendered thoroughly inactive, not only towards the oxygen of nitric acid (of any degree of dilution), but also to

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* Extracted from a Letter to Dr. Faraday, and communicated by him.

† See the present Number, p. 554.—EDIT.

the oxygen disengaged (by the action of a voltaic current) out of aqueous solutions of any oxidized body or any oxyelectrolyte. You know that such a state of iron is called forth by making this metal act the part of the positive electrode of a pile and closing the circuit in a certain manner. Now if bismuth be placed in these very same circumstances, it does not seem to undergo any change whatever, for it is violently acted upon by nitric acid (of sp. gr. 1·4) and unites with the oxygen resulting from the electro-chemical decomposition of water or any other oxyelectrolyte. It is particularly the last-mentioned difference of bearing between the two metals which makes me suspect that the peculiar condition of iron is not produced by the same cause which occasions the inactivity of bismuth, that is to say, that the latter effect is not brought about by a current passing in a certain direction through bismuth. There is another fact which seems to speak in favour of this opinion: according to my experiments, peroxide of lead proves to be the most powerful of all substances which are capable of turning common iron into its peculiar state. Peroxide of lead, in whatever manner I tried to combine it with bismuth, did not appear to have any action upon the metal, for this substance was dissolved by nitric acid just in the same way as it was when put into the said fluid without any voltaic association. Now it is to be asked, in what manner does platinum weaken the chemical action of nitric acid upon bismuth? Are we to believe that in the case in question the former acts in a quite peculiar way, that it puts into play an agency of a nature as yet unknown and entirely different from current electricity? I am certainly not much inclined to draw any such inference from the fact alluded to, but at the same time I must confess, that for the present, at least, I am not able at all to account for the anomaly spoken of. Before passing from the subject of the peculiar condition of bismuth to another one, allow me to mention to you some more phænomena which bear upon the same matter, and which have, perhaps, not yet been observed by Mr. Andrews. After (by the agency of platinum) the violent action of nitric acid (sp. gr. 1·4) upon bismuth has been changed into a slow one, and both metals brought out of contact, bismuth loses its metallic lustre and assumes a blackish appearance; after a short time, however, the metal turns bright again by itself, and remains so until it is touched a second time by platinum. As long as the contact between both metals is maintained, certainly there is no change of the surface of bismuth to be observed, but no sooner have they ceased to touch each other than the bismuth begins to blacken again; it reassumes, however, after some lapse of time, its former lustre. This change of surface can be effected as often as you like. I have ascertained that bismuth covered with the said blackish coating is more energetically acted upon by nitric acid than it is when its surface appears to be bright. Now as platinum, by means of its contact with bismuth, causes a very considerable diminution of the energy of chemical action of the acid upon the latter metal, and makes the black film always and instantaneously disappear from it, the reproduction of this coating under the circumstances before mentioned is a fact very strange indeed, and altogether anomalous. Another fact also worthy of being stated is, that the black film can be produced either by moving the bright bismuth about within the acid or by causing the acid to be moved about the metal. I do not yet know what the black substance consists of, but whatever it may be, its production in the last-mentioned way is no doubt due to the removal of some stratum surrounding the bright metal and protecting the bismuth against the violent action of nitric acid. This supposed stratum consists perhaps of a solution of nitrate of bismuth mixed with some nitrous acid.

If bismuth being in its peculiar state or covered with the blackish film be tightly [strictly?] touched with a platinum wire within nitric acid of sp. gr. 1·4, a gaseous substance will be disengaged at the wire all the while contact is maintained between the metals. Having not yet made the experiment on a scale large enough to allow the collection of the gas, I do not know its nature. I have stated however the fact to you, because the development of a gaseous body under the circumstances alluded to must appear very odd, if we consider, that no gas whatsoever is disengaged at the negative electrode when nitric acid of some strength, for instance of sp. gr. 1·4, is subjected to the action of the current of a pile. Now in the case spoken of, the platinum wire does certainly act the part of the negative electrode. As every circumstance connected with the peculiar condition of readily oxidable metals appears to me to be of some importance, I will not omit to mention the fact, that inactive iron cannot be brought into contact with inactive bismuth without being thrown into chemical action. Iron, however voltaically associated with platinum, is proof to the exciting influence of the passive bismuth, and capable of destroying the often-mentioned black substance just in the same manner as platinum. Some few words more on the peculiar state of bismuth and I have done with this subject, with which I am afraid I have already occupied you too long. By immersing that metal for a few seconds into nitrous acid it is turned inactive, so that it can be put into nitric acid of sp. gr. 1·4 without being sensibly attacked by the latter.

The *Biblioth. Univers.* also alludes to a paper read at Liverpool by Mr. Hartley, on the Preservation of Iron against the action of sea water. The fact stated by that gentleman is on account of its anomaly highly interesting, and seems to enter into that class of electro-chemical phænomena which have been the subject of my researches these last two years. If you recollect a statement of mine made in a paper "On a peculiar Action of Iron," &c.,* you will be aware that the result obtained from Mr. Hartley's experiments does not quite agree with what I have found out to be a general fact. The statement alluded to runs as follows: In solutions containing, besides oxyelectrolytes, others of a different nature, for instance, hydracids, haloid salts, &c., no evolution of oxygen takes place (at the iron being the positive electrode of the pile) in whatever manner the circuit may be closed. Now if in Mr. Hartley's voltaic arrangement brass is to iron (in an electrical point of view) what platinum is to the latter or any other readily oxidable metal, according to my experiments we should suppose that iron, being voltaically associated with brass, would be chemically acted upon by sea water, that is to say, be oxidized and chloridized. You may easily ascertain the correctness of my statement by plunging an iron wire which is connected with the positive pole of a pile into an aqueous solution of chloride of sodium, closing thereby the circuit. You will observe that the iron is not turned inactive, but corroded, and effects are produced quite consonant to the well known electro-chemical laws. I made a couple of days ago some experiments with sea-water itself, and I found that iron was attacked by it when a current passed from the metal into the fluid. As you can easily imagine, the disagreement of Mr. Hartley's observations with mine, makes me exceedingly desirous of becoming acquainted as soon as possible with the particulars of that gentleman's researches. I hope the next number of the Philosophical Magazine will satisfy my curiosity on this point.†

Last summer, during a short stay at Stuttgard, I made in the laboratory of Professor Degen there, and in company with this able chemist, some experiments upon cobalt and nickel, to ascertain whether these metallic bodies are capable of being rendered inactive. Having but a very small quantity of these metals at our disposal, we were obliged to limit the number of our experiments to a very few, and to execute them on

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* Lond. and Edinb. Phil. Mag., vol. x. p. 267.

† We are not aware that any further statement of Mr. Hartley's results has yet appeared: that given in page 554 of the present Number is substantially the same with the notice cited above.—Edit.

a very small scale. The results obtained from them were, however, such as to convince us, that the peculiar condition cannot be excited either in cobalt or in nickel; at least not in the same way as it is done in iron. This fact seems to indicate that the peculiar voltaic state of the latter metal has nothing to do with its magnetic properties.

Believe me ever to be yours very truly,

To Dr. Faraday, &c. &c. C. T. SCHŒNBEIN.

LXXIX. *Notices respecting New Books.*

A Synopsis of the Family of Naïades. By Isaac Lea, Member of the American Philosophical Society. Large 8vo. with 1 coloured plate. Philadelphia, 1836.

In most families containing a great number of species the characters are generally found to shade so much into one another, that it is often almost impossible to draw the line of distinction. Such is the case with this interesting family of Mollusca, of which more than 344 species are enumerated. The author divides the family into two genera, *Margarita* and *Iridina*. The first he then subdivides into *Unio, Margaritana, Dipsas, Anodonta*, and *Pleiodon*, according to the existence and situation of the teeth. These subgenera are again divided into Symphynote and Non-Symphynote. Great attention has been paid to the Synonymy, which is enormous, and attended with many difficulties; and even this alone would render the work of great importance to every Conchologist. The numerous notes contribute not a little to its value, and evince the great labour and sound judgement of the author.

Bibliographical Bulletin.

Einfluss des Bodens auf die Vertheil d. Gew. (The influence of the soil on the diffusion of Vegetables demonstrated in the vegetation of the north-easterly part of the Tyrol). By Prof. F. Unger, Vienna, 1836. 8vo.

Our readers will already have seen this work noticed in Meyen's Report on Vegetable Physiology, p. 445, &c., where an extract from it is given: we, however, think it well worth while to direct attention to it, as it contains results which will probably be of vast importance in the physiology and geography of plants. The work is divided into three parts; geological, meteorological, and botanical. The author first considers the chorography, the situation, geological system, springs, and lakes in the neighbourhood of Kitzbühel. Then follows the petrography and geography of a great part of the Tyrol, which on account of the minute details, is of great value to the mineralogist. With the meteorological observations are given two tables on the condition of the barometer and one on the temperature. The botanical part is rich in comparisons of the vegetation with the localities, mountain and valley, and especially with the geological formations and conditions of temperature. Various new observations and experiments have been made on the structure of plants, their nutritive organs, and the substances which they take up. To these are added various maps, plates, and a catalogue of all the plants, cryptogamia and phanerogamia, occurring there (1773 species are enumerated, among which 818 cryptogamia). The work evinces great care and labour in the author.

Abbildungen neuer od. unvollständig bekannter Amphibien (Figures of new or imperfectly known Amphibia), sketched from living specimens, and accompanied with text by Dr. H. Schlegel, Curator of the Museum of Leyden. Part I. Dusseldorf, 1837.

In no branch of natural history are figures more necessary than in Amphibiology, and those of the present work from their excellence, being drawn and coloured after nature, merit approbation. Most of the numerous and beautiful drawings, executed in India by order of the Dutch government, by Reinwardt, Kuhl, van Hasselt, Boie, and Maclot, are to be given in this work. Great attention will be paid to the anatomical details and natural history of the serpents, a department hitherto rather neglected. The form of the work is that of Temminck's *Planches coloriées d'Oiseaux*, which it surpasses as to drawing and colouring, and it may be considered as completing the works of Lacepède, Russel, Neuwied, Bell, Wiegmann, and others. Each part contains ten plates, with text, at a moderate price.

Wiegmann's *Archiv*, Part 4. Berlin. 1837.

Contents.—A brief view of the history of the development of vegetable organization in the Phanerogamia; by Dr. M. J. Schleiden. (A translation of this interesting paper will shortly appear in this Journal).—On the generation of *Pteroptus Vespertilionis*; by Chr. L. Nitzsch.—A new genus of water-serpents described by J. Tschudi.—*Filaria?* in the brain of the fœtus of a Lizard; by Prof. Rathke.—Contributions to the knowledge of the *Trilobites*, with especial regard to their definite number of Members; by A. Quenstedt.—On the genus *Procyon*; by Prof. Wiegmann.—Fossil quadrumana.—Ehrenberg's new discoveries respecting the *Bacillariæ* (See p. 448 of our present volume).—Notice on the bite of the *Moco* (*Cavia rupestris*, Neuw., *Kerodon*, F. Cuvier); by Prof. Wiegmann.—Report on the progress of Zoology during 1836; by Prof. Wiegmann.

Poggendorff's Annalen, 1837. Nos. 6 and 7.

Contents.—On an apparatus for performing Volta's fundamental experiments; by T. Fechner.—On the expansion of dry air between 0° and 100°; by F. Rudberg.—On the oxidation of metals in atmospheric air; by A. v. Bonsdorff.—On the calculation of the forms belonging to the tessular system of crystals; by Fr. v. Kobell.—On the crystalline form of Phenakite; by E. Beyrich.—Description of a Mica-copper; by F. Benecke.—Analysis of the copper-mica; by F. Borchers.—Proportional combination of the oxide of silver and oxide of lead; by F. Wöhler.—On the formation of the oil of bitter almonds; by F. Wöhler and J. Liebig.—Proposal for the adoption of a new medicine instead of distilled laurel and bitter almonds water; by F. Wöhler and J. Liebig.—On *Amygdalic* acid and some of its salts; by F. L. Winkler.—Composition and constitution of *Amygdalic* acid; by J. Liebig.—Method of preparing the bicarbonate of Potash; by Wöhler.—On the decomposition of some of the æthereal oils extracted from various Cinnamons; by G. J. Mulder.—On Marcet's Xanthicoxide; by Liebig and Wöhler.—Additional notices respecting Arsenic and its combinations; by J. F. Simon.—Detection of quicksilver in the saliva voided after the use of the mercurial salivation; by L. Gmelin.—Solubility of mercurial vapour in water; by A. Wiggers.—On a new air-pump; by N. Löwenthal.—Instructions and tables for rendering easy the calculation of the specific gravity of gases from observed facts; by Poggendorff.—Experiments on the specific heat of gases and of the air at different pressures; by G. Suermann.—On the specific gravity of sea-water collected at different periods at the same places of the ocean; by J. Childer.—On the *Knee press*; by T.

Fechner.—Theory of the colours of thin plates; by Prof. Airy.—Two letters to A. v. Humboldt; by von Kreil: Observations on the magnetic deviation, inclination, and horizontal intensity at Milan in the years 1836 and 1837, with the description of a new Inclinatorium.—Meteorological observations made in 1836 at Baunsberg in East Prussia; by J. Feldt.—Results of the meteorological observations made at Karlsruhe in 1834 and 1835; by D. Otto, Eisenlohr.—On the relative masses of existing siliceous infusoria, and on a new infusorial conglomerate in the state of *polirschiefer*, found at Jastraba in Hungary; by Ehrenberg.

Neue Notizen von Froriep, vol. ii.

Contents.—No. 18. No ciliary organs in the Spermatazoa of the Salamander; by C. Th. Siebold.—No. 19. On the forces which regulate the internal constitution of bodies; by O. F. Mossotti, (For this remarkable paper see Scientific Memoirs, vol. i. p. 448. L. & E. Phil. Mag., vol. x. p. 320.)—On the phosphorescence of organic bodies.—No. 20. On the animalcules in the sap of plants.—No. 22. Contributions to the physiology of Fishes.—On the elevation of the ground in the neighbourhood of Naples, and the origin of the island Julia in the neighbourhood of Sicily.

Flora oder allgem. Botanische Zeitung. No. 7, &c. 1837.

Contents.—Remarks on some *Gramineæ*; by Prof. Tausch.—Remarks on *Erysimum lanceolatum* R. Brown; *E. ochroleucum* De Candolle, *rhæticum* De Can. and *pumilum*, Gaud.; by Guthnick.—On two North American species of the genus *Valerianella*; by R. J. Schuttleworth.—On *Rhododendron*; by Zuccarini.—On *Rhododendron*; by L. Reichenbach.—Observations on some *Saliceæ*.—Proceedings of the Royal Botanical Society of Regensburg. (This work contains many other papers which we have not considered to be of particular interest, as they relate entirely to Germany, such as the Floras of certain districts.)

Neues Jahrbuch für Mineralogie, &c.; by Leonhard and Bronn, 1837.

Contents.—No. 2. Excursion into the department of Isère; by Lortet.—On the age and organic remains of the tertiary formation of the Mainz basin; by Bronn.—No. 3. Geognostical and physical observations on the volcanos of the high land of Quito; by A. v. Humboldt.—On the sub-fossil remains of Puzzuoli near Naples, and on the island Ischia; by Prof. A. Phillipi.—Description of a new species of *Nerina* and a new fossil species of *Pecten*; by the same.—On the organic forms in the agate near Schlottwitz; by B. Cotta.—Notice on the genus *Aptychus*; by Voltz.

Archiv der Pharmacie; by R. Brandes, vol. ix. Part I. 1837.

Contents.—A voltaic battery of easy construction, and some remarks on Faraday's views on Electricity; by Dr. Geiseler.—Notice on Fusel oil; by C. Stickel.—On the natural springs of carbonic gas near Meinberg, with some general remarks on mineral waters; by R. and W. Brandes.—Poisoning by prussic acid cured with carbonate of ammonia; by Dr. Geoghegan.

Zeitschrift für Physik, &c.; by Baumgärtner and Holger. Parts II. and III. 1837.

Contents.—On the alkaline action of the mineral water of Püllna, of some calcareous fossils, bicarbonate of lime, and more especially on the mineral water of Prague; by Ad. Pleischl.—On the testing and division of areometer scales; by Aug. Neumann.—General view of the meteorological relations in 1836.—Observations on two invariable reversion-pendulums; by J. von Littrow.—On Legrand's observations on the oscillations which the scales of mercurial thermometers undergo; by W. Gintl.

Studi geologi sulla Toscana; by Savi.

Annales de Chimie et de Physique. June 1837.

Contents.—Action of sulphuric acid on oils; by E. Fremy.—On Oleic acid and Elaïdic acid; by A. Laurent.—Note on the cause of the red colouring of the salt-water basins; by Payen. (The author attributes this circumstance to the presence of a small *crustacean* known in England by the name of brine-worm, *Artemia salina?* of Leach, but in a paper of Dunal it is affirmed to be caused by the presence of some *Algæ*).—On the gases contained in the blood, oxygen, nitrogen, and carbonic acid; by G. Magnus. (From this memoir it appears that not only does the venous blood contain carbonic acid, oxygen, and nitrogen, but also the arterial blood, and that relatively to carbonic acid the arterial blood contains much more oxygen than the venous. This is a very interesting paper and will probably find a place in one of our future numbers).—Action of sulphuric acid on the hydruret of benzoyl; by A. Laurent.—Researches on the coercitive force and the polarity of magnets without cohesion; by Dr. Haldat.

LXXX. *Proceedings of Learned Societies.*

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: MEETING AT LIVERPOOL.

Section of Zoology and Botany, September 11.—Mr. Gray offered some remarks on the supposed production of insects by the experiments of Mr. Crosse, and referred to two experiments made by Mr. Children in a manner perfectly identical with those of the former. The solution of silica was obtained from Mr. Garden, of Oxford Street, and in one experiment it was sealed up, whilst in the other it was exposed to the air, but in neither case was there any appearance of insects. The insects had been very indefinitely described by Mr. Crosse, some as having six, and others eight legs. It was no proof that they could not have been produced from the water used in the experiment because it was boiled, as that would not be sufficient to destroy the eggs of the insects deposited therein.—The Rev. Mr. Hope remarked one peculiarity, that no one had given the insects a specific name, and that they merely appeared to belong to the commonest species of *Acari.*—The Chairman (Mr. W. S. Macleay) mentioned the circumstance, that the seeds and germs of animals and vegetables are earlier and more quickly developed in a current of electricity, and that in all probability, these favourable circumstances operated upon the eggs of the insects produced in question. It was well known that seeds would retain their vitality for an indefinite period of time, and there was no reason why any limit should be put to the vitality of the eggs of animals.—Mr. Gray stated that prussic acid had lately been used for the purpose of destroying insects at the British Museum, particularly those infesting a mummy. Some of the larvæ of the common Musca having been put into the acid, remained uninjured after two or three days exposure.—Professor Graham (Univ. Edinb.) remarked, that other plants and animals might be kept for an indefinite length of time, when the powers of life were either retained or suspended. He also alluded to some curious experiments recently made at Edinburgh, although first by Sir Astley Cooper in London, with respect to the circulation of blood through the brains of particular animals. If the circulation be suspended by pressure for half a minute, the animal becomes torpid, but after giving a few convulsive sobs recovers; whilst if it is suspended for a minute the animal irrecoverably dies.—The Chairman observed that he had often dried to powder the eggs of various insects, which having been put into water were hatched.

Mr. Golding Bird referred to the observations of Mr. Gray, with respect to the production of insects, as stated by Mr. Crosse in his experiments, which he had repeated on a large scale, but without any result, although he had continued them for some weeks, varying them in every possible form. He also explained that such could not have been produced from the silica, as this was precipitated from the mixture of the alkaline solution of silica and muriatic acid, the fluid passing through the filter being nothing but very dilute muriatic acid.

Sept. 12.—The President observed that he was about to read a letter upon a subject that had excited some interest in the scientific world, on account of the difference between two eminent naturalists,—Mr. Thompson and Professor Rathke, of Berlin. He remarked, that at first sight it appeared strange, that animals apparently so high in the scale of organization as the crabs and lobsters should undergo the same changes in the course of their development, as the class Insecta. Mr. Thompson had observed, that the young of the barnacle shell were deposited by the parent in a free state, and that they afterwards became fixed; and the same fact had been pointed out to him by that zealous naturalist Mr. Darwin. It was therefore not extraordinary Mr. Thompson should take the view he did of the development of Crustacea. The letter he had on this subject was from Capt. Du Cane, of Southampton, who, from his contiguity to the sea, had been led to investigate this subject. He had obtained the ova of what he supposed to be the common prawn: after keeping them in water a little time, they produced a number of minute diaphanous animals, altogether different from the full-grown prawn. These ova were obtained from a ditch open to the influx of the sea, but the water was only slightly brackish. Captain Ducane therefore proposed to call this animal the "Ditch Prawn." The letter was accompanied by a drawing, exhibiting the various changes which the Captain had observed to take place in these animals during the first three days after their leaving the ova, none of his specimens living a longer period.

The President observed, that the subject of the development of Crustacea was a most interesting subject. He questioned whether many of the received species of that class were anything more than one animal in its several stages of development.

Dr. Richardson inquired if it might not be possible for Capt. Du Cane to have mistaken some parasitic animals for the young of the prawn. The Rev. F. W. Hope was inclined to think the animal alluded to belonged to the shrimp, and not to the prawn family. On the Norfolk and Suffolk coast, the ditch shrimp was common. He believed crustaceous animals themselves were frequently parasitic.—The President replied, that the ova obtained by Capt. Du Cane were too abundant to be the produce of a parasitic animal, and the results too constant to lead to such a supposition.

Mr. Bowman read a paper from Mr. Gardner, 'On the internal structure of the wood of Palms.' The attention of Mr. Gardner, who is residing in Brazil, was directed to this subject by the remarks made by Professor Lindley, in his 'Introduction to Botany.' In order to test the truth of the theory of Mohl, he made several experiments on the palms in his district. He made a vertical section of a palm, four inches in circumference, and, by doing this, he could trace very plainly woody fibres proceeding from the base of the leaves to the centre of the stem, at an angle of 18 degrees; they then turned downwards and outwards to within a few lines of the external cortical part of the stem, running parallel with its axis. The distance between these two points was about two feet and a half. The fibres were traced quite distinctly up into the centre of the leaf. In answer to the questions proposed by Lindley in his work, the author stated :—1. That the wood of palms was always hard and compact outside, gradually getting softer towards the centre, the fibres of the upper leaves not descending to so great a depth as the lower. 2. The wood is much harder at the bottom than any other part of the stem, the inhabitants of tropical climates using only this part for economical purposes.

Professor Lindley observed, that this paper confirmed the views of the structure both of endogens and exogens, which had been increasingly embraced by botanists. In the first place, the views of Mohl on the structure of endogens were confirmed. There was, however, a slight difference between Mr. Gardner and Professor Mohl; the latter having stated that the woody fibres of endogens terminated *in* their cortical integument, whilst the former had traced them only within a few lines of this point. In the next place, the paper confirmed the theory of the formation of wood from the emanation of fibres from the leaves. Whatever might be the difference between the arrangement of the fibres of exogens and endogens, there could be no doubt that their origin was the same. Mr. Gardner had referred, in his paper, to the glandular disks on the woody fibre that were, at one time, thought to characterize the order Coniferæ. He would, however, draw the attention of the section to a fact that had lately been discovered, and not hitherto published, that these glandular disks existed on all the woody fibres of plants that yielded resinous matter. Brown first discovered them in the wood of Tasmania (*Winteraceæ*), and Griffiths had since demonstrated them in Spherostema (*Schizandreæ*).

Mr. Nevin detailed some experiments on vegetable physiology. The experiments were performed on elms forty years of age, in February, 1836.

1. The stem of the tree was denuded, in a circle, of its cortical integument alone, leaving the alburnum beneath uninjured. On the May following the denuded part was filled up by the exudation of bark and wood from the upper surface of the wound, and the tree had not suffered in growth.

2. The bark and *cambium* were removed in the same manner. In August 1837 this tree sickened, and there was no formation of wood or bark in the wounded part. Two developments however, took place

one above the other, from below; the former having the appearance of roots, the latter were branches with leaves.

3. The bark and two layers of alburnum were cut away. The tree was at the time unhealthy; it however put forth its leaves on that and the ensuing spring, but shortly after died. No sap was observed above or below the wounded part. Roots were developed from the upper, and branches from the lower part of the section.

4. The bark and six layers of alburnum were taken off. The tree became much less vigorous, but did not die, and otherwise presented the same appearance as the last.

5. The bark and twelve layers of alburnum were stripped. The consequences were again similar to the last two; the alburnum above and below the cut being dry, but an accidental cut that penetrated into the heart-wood exuded sap.

6. This was a repetition of the experiment of Palisot de Beauvais, by cutting away a circular ring of bark around a single branch. The branch continued to grow, and roots sprouted from the under surface of the isolated bark and branch.

7. In this the whole of the wood of the tree was cut away, except four pillars, composed of bark and sap-wood. In this case, the sap first appeared from above, descending by the pith, and then from the heart-wood, the alburnum being dry. In this case the sap must have passed up the alburnum, and horizontally through to the heart-wood.

Mr. Nevin inferred from these experiments—1. That the life of the tree does not depend on the liber or cambium. 2. A descent of sap takes place before the developement of leaves. 3. That new matter arises from below; which had not previously been allowed. He thought there were two distinct principles in the tree,—one, the ascending, or leaf principle; the other, the descending or root principle. Mr. Nevin had also performed some experiments on the conversion of roots into branches, and came to the conclusion, that buds or branches might be developed from any part of the root above its extreme end, from which point it was impossible for buds to be developed.

Professor Lindley remarked that these experiments confirmed entirely the theory of the structure of wood adopted by Du Petit Thouars. He did not think that the existence of any new principle could be inferred from the experiments. In the seventh experiment the horizontal circulation of the sap was proved, and confirmed the accuracy of Hall's experiment of cutting a tree nearly through on alternate sides, when the sap still ascended.

Section of Chemistry and Mineralogy, Sept. 12.—Mr. Hartley read a paper, 'On the Corroding of Iron by salt water.' The object of the paper was to show that brass protects both bar and cast-iron in a very perfect manner. The brass did not appear to have undergone any action, which, as stated by the President, (Mr. Faraday,) is rather opposed to received notions of electro-chemical action.

Dr. Andrews next read a paper, 'On some singular modifications of the ordinary action of nitric acid on certain Metals.' Bismuth in nitric acid of specific gravity 1·4, was rapidly acted upon, but this action immediately ceased when the bar was touched by platinum. On removing the platinum from the liquor, the bismuth will sometimes begin again to dissolve; at other times, its surface will become covered with a black crust, which is soon removed by the acid; but the metal, though now exhibiting a beautifully-polished surface, is no longer acted upon by the acid, or, at least, is dissolved only with extreme slowness. Thus, a slip of metal, which, in its ordinary state will require only a few seconds to complete its solution, will, when thus slightly modified, resist for many hours the action of the same acid.

Copper and tin present similar phænomena, but zinc, when treated in the same way, has its oxidation and solution not arrested, but merely retarded. Arsenic was found to present a singular anomaly when heated in nitric acid so as to give rise to effervescence: the contact of the platinum in the usual way did not produce any effect; whereas, when an acidulous solution of silver is used, platinum exercised its usual influence.

In the case of six metals, platinum checks the action of nitric acid, and three of them appear to be brought into a permanently peculiar state, opposed to chemical action. Platinum always separates any film of oxide as its initial function; but after its separation, it exercises a polarizing action, for example, it brings the other metal into a peculiar state, which enables it to resist chemical action.

On the conclusion of this paper, the President drew the attention of the Section to the analogy between the facts detailed by Dr. Andrews, and the preservation of iron by brass, as instanced in the communication of Mr. Hartley. In both cases, according to the known laws of electro-chemical action, effects, the very opposite of what are observed, should present themselves. The bismuth, copper, &c., should oxidize quickest when in contact with the platinum; and if, as would seem demonstrated by Mr. Hartley, brass protects wrought and cast-iron, the brass itself should be acted upon with increased rapidity. The solution of these anomalies, he conceived to be quite within the range of science in its present state, and he urged upon the members of the Section the necessity of studying the phænomena in question, as their explication would constitute a very valuable addition the existing state of our electrical knowledge*.

Section of Geology and Geography, Sept. 12.—Mr. Horner exhibited to the Section a drawing representing some of the Geological phænomena in the neighbourhood of Christiania, in Norway, and read a letter from Mr. Lyell in explanation. This part of Europe received some years ago an examination from M. von Buch, and the results were at that time presented by him to the scientific world in such a manner, as to excite much attention, and to stagger many in their attachment to the Wernerian Theory, then very prevalent. Von Buch had discovered granite overlying fossiliferous strata outstepping the bounds prescribed to it by Werner, so that his disciples were obliged to invent a creation of it at a later date, to explain the Norwegian phænomena. Mr. Lyell has lately examined this interesting locality, and has found the junction of the granite with the

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* See Prof. Schœnbein's paper in the present number, p. 545.

fossiliferous rocks well determined; also that it not only sends veins into them, but that it actually in some degree overlaps them, the strata rather turning up from it. The granite sometimes becomes syenitic, passing into trap porphyry; and it has altered the appearance of the adjacent rocks, making them truly *metamorphic.* These fossiliferous rocks rest upon gneiss, and may be referred to the Silurian system of Murchison. The granite in the same vicinity comes in contact with the gneiss, which rock has frequently indications of having been disturbed before the deposition on it of the Silurian rocks, and it bears on its surface marks of scoring and abrasion. The posterior formation of the granite is proved by its veins in the adjacent rocks, and by the fragments of gneiss in the Silurian strata. The entire phænomena are of the highest interest,—they must have been originally submarine, as the disturbances would have been vastly greater, had they not taken place under a pressure of perhaps some miles of ocean in depth, and perhaps also with other strata upon them: the gneiss, under such a pressure may have been only softened. Near Christiania, many dykes, evidently of volcanic origin, also occur; these are of syenite passing into greenstone, and are evidently fissures filled with injected matter.

Section of Mechanical Science, Sept. 12.—Mr. Fairbairne then read a report on the comparative strength and other properties of cast iron, manufactured by the hot and cold blast respectively.

At a previous meeting of the Association, Mr. Hodgkinson read a report on the comparative strength and other properties of iron manufactured by the hot and cold blast.—In the prosecution of inquiries since made, it was conceived desirable to subject the metals operated upon to more than one species of strain; to vary their forms, and, by a series of changes to elicit their peculiar as well as comparative properties. First, they have been drawn asunder by direct tension. Secondly, they have been crushed by direct compression, both in short and long specimens, (the results of which will be given in a paper read subsequently); and, Thirdly, they have been subjected to fracture by transverse strain, under various forms of section, and at various temperatures. Ten bars of hot and cold blast iron were also loaded with different weights from 112lb to near the breaking point, and left for many months to sustain the load, and to determine the length of time necessary to effect the fracture. The bars thus loaded, are still (with one exception) bearing the weight, having been suspended upwards of six months, and from what we can at present perceive, there is every chance of a long and protracted experiment. In making the experiment on transverse strain, a number of models of different sizes and forms were prepared, and the irons, both hot and cold blast, were run into the form of these models; but as there is usually a slight deviation in the size of the castings from that of the model, the dimensions of the bars were accurately measured at the place of fracture, and the results reduced by calculation to what they would have been if they had been cast the exact size of the model, assuming the strength of rectangular beams to be as the breadth and the square of the depth, and the ultimate deflection to be inversely as the depth, the length being constant. In comparing two irons, the greatest care was taken to subject them as nearly as possible to the same treatment.

A series of experiments was also made to determine the strength of hot and cold blast iron at various temperatures, from 32° (the freezing point) to the boiling point; for this purpose a cast-iron trough was employed, in which the bars to be broken were placed and covered with snow or water (which was kept at the proper temperature by a jet of steam) as the case required; the weights were then gradually laid on until fracture took place.

The strength of bars made red hot was also tried, and contrary to expectation, they retained their tenacity and power to resist the load to a considerable extent: the reduction of strength in a bar one inch square, in a range of temperature from 32° to that of redness was rather more than one-sixth, the deflection being upwards of 1½ inch in a bar 2 feet 3 inches long.

RESULTS.

Carron iron, No. 2. (Scotch.)

Mean ratio of transverse strength, assuming the cold blast iron at	1000 : 979·9
Mean ratio of power to resist impact	1000 : 1038·9

Whence in the transverse strength of Carron iron No. 2, using a variety of forms of section, the strength of the cold blast is to that of the hot blast, as 100 to 98, nearly.

Devon iron, No. 3.

Mean ratio of strength in sections of various forms (thirteen experiments)	1000 : 1409
Power to sustain impact	1000 : 2742

This is an exceedingly hard iron with a singular appearance, the centre or more granulated parts of the fracture being surrounded with a circle having the appearance of hardened steel.

Buffery, No. 1, Staffordshire iron, cold and hot blast.

Mean ratio of breaking weight	1000 : 925
Mean ratio of power to resist impact	1000 : 965

In the buffery iron, the hot blast manufacture is weaker whether we view it in its transverse strength, or its power to resist impact.

Coed Talon, No. 2, North Welsh iron.

Mean ratio of strength in a number of experiments	1000 : 1014
Mean ratio of power to resist impact	1000 : 1219

Modulus of elasticity in lbs. for a bar of one inch square.

Cold blast	14,680,000 13,947,000	14,313,500lbs.
Hot blast	15,810,000 12,835,000	14,322,500lbs.

Elsecar cold blast, No. 1, against Melton hot blast, No. 1, (Yorkshire iron.)

Mean ratio of strength	1000 : 809
Mean ratio of power to resist impact	1000 : 858

The modulus of elasticity in all the irons are computed; but only given in a few cases in the results.

Relative strength of hot and cold blast iron to resist a transverse strain at different degrees of temperature.

Cold blast 949·6 at 32°. Hot ditto 919·7, Mean.
Ratio of strength, 1,000 : 977·6.
Power to resist impact, 1,000 : 1,039.
Cold blast 748·1 at 191°. Hot ditto 823·6.

In these experiments it appeared that the cold blast lost in strength from 32° up to a blood-red perceptible in the dark, as 949·6 to 723·1; whereas in the hot blast the strength is not so much impaired, being as 917·7 at the freezing point, and 829·7 when perceptibly red in the dark.

In all former experiments on the transverse strain of cast iron it has been assumed, that the elasticity remained perfect up to one-third the breaking weight. In pursuing these experiments, discrepancies were noticed, and results widely different to those generally received were observed. It was found that one-seventh, and in some cases, one-eighth the breaking weight was sufficient to produce a permanent set. These facts induced an extended series of experiments, principally to determine what load was necessary to effect a permanent set; and, if such weight, continued for an indefinite time, would break the bar. It became a question of great importance to know, if a weight having once impaired the elasticity, would or would not, if continued, increase the deflection. The inquiry therefore, was—To what extent can cast iron be loaded without endangering its security? To solve this question ten bars of hot and cold blast, differently loaded, were placed upon a frame, to ascertain the amount of deflection at stated periods, and to determine what was necessary to break the bars with their respective loads.

	Inches.
In the cold blast, with a load of 280 lbs., the deflexion increased in 103 days from..........	1·025 to 1·033
Hot blast, ditto from....................	1·173 to 1·197
Cold blast, with a load of 336 lbs., increased in 105 days from..........................	1·344 to 1·366
Hot ditto, from	1·573 to 1·627
Cold, with a load of 392 lbs., increased the deflection in 108 days, from....................	1·786 to 1·843
Hot, ditto from....................	1·891 to 1·966

Cold blast, with a load of 448 lbs., continued to increase in deflection and ultimately broke, after sustaining the weight 35 days. All the bars from the hot blast broke in the act of loading them with the above weight, 448lb.

Mr. Fairbairne stated, that all the irons were made of the same materials, and under the same circumstances. The irons were of fifty sorts.

Mr. Cottam inquired as to the elastic forces. Dr. Young and Mr. Tredgold had found that the strength of the material would fail if loaded beyond its elastic force: he wished to know whether the loads had been more or less than 850 lbs. to the foot. Mr. Fairbairne stated that some of the loads were more, some less, and that a weight of 280 lbs. produced a permanent set of an inch square bar. The President remarked that the calculation as to elastic forces was scarcely to be confided in. Mr. Fairbairne in answer to another question, stated that the hot blast iron was the more flexible and better capable of bearing impact; but that all the results of impact had been taken from calculations founded on cold blast iron. Mr. Fairbairne stated that the crystalline appearance was finer in hot than in cold blast. There were no experiments made on the loss by remelting, and none on wrought iron,—all on cast iron. In reply to Mr. Cottam, he mentioned that all the Scotch irons had no cinder; the composition of the others they did not know. Great difficulty had been experienced on this point, because the different manufacturers were unwilling to give information.—Mr. Guest professed on his part the fullest readiness.—Some conversation took place with regard to the peculiarity of appearance in the broken bars. The President remarked, that when a rectangular bar of any substance is exposed either to fracture, or even to temporary deflexion, a similar appearance was found: this was known from the experiments on glass by polarized light. Mr. Fairbairne in assent, said the crystals were always more compact in the edge than the centre. Mr. Webster inquired whether the elastic weight was always less than one-third of the breaking weight. Mr. Fairbairne said, always—and afterwards replied to a question from Mr. Guest, that the Scotch hot blast iron showed a greater comparative strength as compared with cold blast, but that they had made no experiments on South Welsh iron. There was a perceptible permanent set from 280 lbs., the experiments being of from five to ten minutes in duration, and it being possible to judge the deflection to the 1000th part of an inch.—Mr. Webster said it had been found that the first set was owing to the breaking of the first crust; and that beyond the first permanent set up to the elastic limit, the deflexion increases exactly as the weight. Some further conversation ensued, in which Mr. Smith and others took part, when Mr. Guest suggested the propriety of further continuing these researches, to which the President agreed, and suggested a recommendation to that effect from the committee of the section to the General Committee. *Athenæum*, No. 516.

LXXXI. *Intelligence and Miscellaneous Articles.*

GASEOUS DIFFUSION.

THE following experiment has recently been made in the laboratory of Prof. Draper, of Hampden Sidney College, Virginia, and communicated to us by him; it is a good illustration of the endosmosis of gases, and is well adapted for a class-room experiment.

Take a two-ounce phial with a wide mouth, and having made a solution of soap in water of the consistency of a viscid lather, dip one of the fingers into it, and pass it over the mouth of the phial; this will leave a thin *plane* film stretched across it. Place over the phial thus arranged a jar of protoxide of nitrogen, in the course of a few seconds the horizontality of the film will be disturbed; it will become convex, and at the end of a minute and a half or two minutes, it will form the greater part of a sphere two inches in diameter, the colours of which are brilliant.

The extreme rapidity with which gases pass through films of water, has afforded in the hands of the same chemist a good method of determining the condition of equilibrium and law of the transit of gases. A series of papers communicated during the past and present years to the American Journal of Medical Science, and Journal of the Franklin Institute, contain a development of these laws. An illustration from these researches will give an idea of their result.

In an atmosphere containing 190 measures of nitrogen gas, a soap bubble was expanded containing 190 measures of protoxide of nitrogen. The bubble collapsed with great rapidity, and at the end of three minutes its contents were found to be 35 measures, the atmosphere around it being 341 measures. On analysis it was found that the constitution of the gas within and without the bubble was identical, one half being the protoxide and the other half being nitrogen.

By thus measuring and analysing the contents of bubbles and the atmospheres around them, the general law of equilibrium was proved to be, that motion only ceased when the chemical composition on both sides of the barrier had become alike.

CHLOROSULPHURETS OF LEAD, COPPER, BISMUTH, AND ZINC.

M. Reimsch, on passing sulphuretted hydrogen into a liquor containing lead and acidulated with muriatic acid, obtained a blood-red precipitate, which was collected on a filter, became of a red brown colour, and when heated on charcoal with soda gave a globule of lead. On leaving the precipitate a longer time in the liquor, and adding more sulphuretted hydrogen, or on continuing a current of this gas, the precipitate became black sulphuret. Free muriatic acid appeared to be requisite to the production of the red-coloured precipitate; for a neutral solution of chloride of lead, or salts of lead with excess of acid treated with sulphuretted hydrogen, always gave the usual black precipitate. M. Reimsch satisfied himself that the production of the red precipitate was always owing to nitric acid contained in the muriatic acid, and consequently to chlorine, and on using muriatic acid free from nitric acid he obtained a yellow precipitate. In order to produce the red precipitate, dissolve half a drachm of neutral acetate of lead in seven ounces of cold distilled water; add to this solution, constantly agitating, seven drachms of nitromuriatic acid which has some days been prepared, by mixing two parts of concentrated muriatic acid with one part of concentrated nitric acid; pass quickly through the solution a current of sulphuretted hydrogen, which is to be discontinued as soon as the reddish yellow deposit has become of a perfect cinnabar red. The yellow precipitate is obtained by adding to the above-described solution of lead, seven drachms of muriatic acid free from chlorine, taking care to agitate them well. The following experiments show the nature of these precipitates.

The Red Precipitate.—If this be boiled in water, it passes from the granular state to that of a bulky red-brown powder; this suffers no further change, and may be dried at 100° Fahr. without altering its colour. The filtered solution contains chloride of lead, and gives with nitrate of silver a copious precipitate of chloride of silver.

It dissolves perfectly in nitric acid with the disengagement of nitrous acid, and the separation of a little sulphur. Nitrate of silver added to this solution also gives a precipitate of chloride of silver.

If it be heated by the blow-pipe on charcoal, it fuses, and disengages sulphurous acid. Part of the lead is quickly reduced, whilst the chloride of lead is volatilized in a white smoke.

When the precipitate, separated from the chloride of lead by ebullition, is heated in a glass tube, it becomes black, disengages sulphur and sulphuretted hydrogen, and then fuses into a brown mass, which crystallizes on cooling.

Yellow precipitate.—This behaves in the same way as the red; except that when the chloride of lead is dissolved by boiling in water, it is converted into black sulphuret of lead, which when heated in a glass tube does not disengage either sulphur or sulphuretted hydrogen.

These results explain the difference which exists between these two precipitates. As by the action of the chlorine or of the aqua regia upon the sulphuretted hydrogen, hydrochloric acid is produced and sulphur set free, the latter combines with the sulphuret of lead, formed at the same time, to produce sulphuretted sulphuret of lead, analogous to that obtained by adding quintosulphuret of potassium to a solution of lead, and this body combining with the chloride of lead, gives the double compound of kermes colour, while the yellow precipitate is merely a compound of sulphuret and chloride of lead, which is decomposed by boiling in water.

According to the experiments of M. Reimsch, copper and bismuth appear to form similar combinations. A solution of copper, acidified with hydrochloric acid, gave with sulphuretted hydrogen a deep green blue precipitate, which when washed with water till free from uncombined muriatic acid, and dissolved in nitric acid, gave chloride of silver on adding the nitrate. The same effect takes place with precipitated bismuth; except that it cannot be obtained pure, on account of the difficulty of washing it.

Zinc, which is generally admitted not to be precipitable from its acid solutions by sulphuretted hydrogen, is however thrown down by it from a solution of the chloride rather strongly acidified by hydrochloric acid. The precipitate obtained is a compound of sulphuret and chloride of zinc. It dissolves in dilute sulphuric acid with a strong disengagement of sulphuretted hydrogen; but when subjected to ebullition, the solution yields an abundant precipitate of chloride of silver with the nitrate.—*Journal de Pharmacie, Mai*, 1837.

POLYGALIC ACID.

M. Quevenne prepares this substance by digesting powdered polygala in alcohol of specific gravity 0·85 till it is exhausted. The greater part of the spirit is to be distilled, and the syrupy residue is to be treated with æther to remove the fatty matters. On standing, a precipitate is formed in the liquid; this is to be separated by the filter and mixed with water: to the turbid solution thus obtained, a little alcohol is to be added; this facilitates the formation of a whitish precipitate, which is impure polygalic acid. The liquor is to remain at rest for several days in order that the precipitate may form perfectly; the supernatant liquor, which contains extractive matters and a little polygalic acid, is to be poured off. The polygalic acid is to be allowed to drain on the filter, and while moist is to be dissolved with the aid of heat in alcohol of specific gravity 0·837. The solution is to be boiled with animal charcoal purified by hydrochloric acid, and by cooling a fine white pulverulent precipitate is obtained, which is pure polygalic acid.

The properties of this acid are, that its smell resembles that of saponin, but is much weaker; its taste is acrid, and it is hot in the throat; in very small quantities it excites violent sneezing; it is soluble in water, the solution reddens litmus, and it froths much when shaken; when boiled with potash, the solution yields a gelatinous precipitate on the addition of hydrochloric acid; sulphuric acid poured into the solution when boiling also gives a gelatinous precipitate, and a rose colour appears on the sides of the capsule; nitric acid used in the same way does not yield a gelatinous precipitate, but on cooling it gives a yellow one; but neither acetic, citric, nor oxalic acid produces any precipitate. Tincture of galls gives a dirty white precipitate. Polygalic acid when saturated with an alkali gives no precipitates with metallic salts, except the acetates of lead and the protonitrate of mercury.

The mean of these experiments gave as the composition of this acid

Hydrogen........	7·529	
Carbon..........	55·704	
Oxygen	36·767	——100.

Journal de Pharmacie, June, 1837.

MODIFIED POLYGALIC ACID.

M. Quevenne gives this name provisionally to the substance obtained by the action of certain acids upon polygalic acid.

In order to prepare it four parts of this acid were mixed with about 30 times its weight of concentrated hydrochloric acid. After 24 hours' digestion, the gelatinous mass was diluted with water, and the whole was thrown upon a filter, and washed with distilled water till it ceased to act upon nitrate of silver. The precipitate thus washed and dried was a grey friable mass; it was boiled in alcohol of sp. gr. 0·817, which gradually dissolved it, leaving a small quantity of gray powder unacted upon. The filtered solution heated by a water-bath precipitated some white flocks on cooling. If a sufficient quantity of water be added, a gelatinous mass is precipitated, and the same effect is produced by the spontaneous evaporation of the spirit.

By evaporation to dryness the modified acid is obtained in yellowish white, brittle, irregular fragments; it has at first but little taste, but soon becomes decidedly bitter; when mixed with water it did not swell in 24 hours. It is slightly soluble in water. The solution does not redden litmus.

A portion of this acid was dissolved in alcohol of sp. gr. ·915. This solution had the following properties: it reddened litmus paper, and precipitated the following salts; the proto- and persulphate of iron, nitrate of silver, acetate and subacetate of lead, the chlorides of barium, lime, and platinum, but not that of magnesium; and sluphate of copper; the aqueous solution of tannin formed a light precipitate. The aqueous solution of the acid precipitates the same salts. The solution exposed to the air evaporated without giving any traces of crystallization. Another spirituous solution of this acid, saturated with potash, left by spontaneous evaporation a very bitter white mass, in which no trace of crystallization could be observed.

Concentrated sulphuric acid causes this acid to impart a weak violet tint to water.

This modified acid considerably resembles the oxalic acid of M. Frésny, [differing only in two of its properties; that of gelatinizing under peculiar circumstances, and of giving a bitter compound with potash. These differences are constant, by whatever mode the acid may have been prepared.—*Ibid.*

ARTIFICIAL PRODUCTION OF RUBIES.

A few months since M. Gaudin presented to the Academy of Sciences of Paris a note, in which he announced his having been able to produce rubies in considerable quantities by a process of which he has given merely a sketch.

In order to obtain these substances analogous to rubies, M. Gaudin uses a platinum blowpipe of a single piece, formed of two hollow concentric cylinders, communicating by one of the extremities, one with a reservoir of hydrogen, the other with a reservoir of oxygen; the two other extremities are pierced with convergent openings, so as to effect in a great degree the mixture of the gases.

It is well known that alumina is fusible with the oxygen and hydrogen blowpipe; but no one before M. Gaudin had endeavoured to melt this earth into globules several millimetres in size. Having submitted a piece of potash alum to the action of his blowpipe, he obtained a perfectly round and limpid globule The platinum tube being perforated and melted at several places, he obtained after the cooling, instead of a limpid spheroid, an opake elongated globule, and covered internally with crystals, which may be referred to the cube or to the rhombohedron. These crystals scratch rock crystal, topaz, garnet, and spinelle; with regard to hardness, therefore, they agree with the ordinary ruby. They appear to be composed solely of alumina, the potash volatilizing at the high temperature to which the alum is submitted.

Having obtained an apparatus stronger than the one first used, he made an experiment with some ammoniacal alum mixed with from 4 to 5 thousandths of chromate of potash; the whole being previously calcined, he gave it the form of a spherical cup, in order to obtain a maximum effect, by directing the flame to the concave part. In a few moments the inner surface of this cup was covered with globules of a beautiful ruby-red colour, slightly translucid, and some of which exhibited the form and cleavage of the ruby.

M. Malaguti, who had occasion to analyse these globules, found them to be composed of 97 parts alumina, one of oxide of chrome, and two parts of silica and lime; which composition is analogous to that of the ruby.—*Comptes Rendus*, August, 1837, p. 325.

THEORY OF ORGANIC COMBINATIONS.

M. A. Laurent states that his theory of organic combinations had given him the means of predicting beforehand, the existence of certain compounds which have since been obtained; and that he has also been able, knowing the composition of a body, to arrive without doubt at the process by means of which it may be obtained. In proof of the latter, he cites the preparation of œnanthic acid, and that of the æther of this acid*, which he had been able to predict without any other data than the composition of the acid, and without any other guide than the laws laid down in the theory which he had submitted to the judgement of the Academy.—*Comptes Rendus*, August, 1837, p. 212.

A NEW ORGANIC ACID.

M. Peligot has read to the Academy of Sciences some observations on cane sugar, and on a new acid derived from the action of alkalies on sugar of starch.

It is well known that there exist two distinct varieties of sugar: one of them is common sugar, extracted from the cane, beet-root, and the maple; the other occurs in grapes and diabetic urine, and is formed when starch, lignin, or sugar of milk is treated with dilute sulphuric acid. It is also known that, influenced by various circumstances, common sugar may be converted into sugar identical with that of starch.

Among the differences which exist between the two kinds of sugar, one of the most prominent (says M. Peligot) is in my opinion that which is observed when these bodies are put in contact with alkaline bases.

Common sugar, when added to potash, lime, or barytes, combines with these bases, and acts towards them the part of a true acid†: by boiling a mixed solution of barytes and sugar, I obtained by direct action a crystallized compound of these two bodies; the analysis of saccharate of barytes, and other analogous salts, proves that the sugar does not undergo any particular modification: on decomposing the saccharates by weak acids, the sugar reappears with its usual properties.

The case is entirely different with sugar of starch; the alkalies effect an essential alteration in it. On putting lime or barytes into a solution of this sugar, even cold, I observed that after a certain time these bases had lost their alkaline properties, and were saturated with a new and very powerful acid, which is formed by simple contact with the sugar, and which immediately unites with the alkalies, and forms perfectly neutral salts. This acid may be still more readily obtained by putting dry sugar of starch, fused at 212°, in contact with crystallized hydrate of barytes. Vivid reaction takes place almost immediately; the mixture swells, the temperature rises very much, and in a

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* See Lond. and Edinb. Phil. Mag., vol. x. pp. 417, 418, 422.

† See the present volume, p. 152.

few seconds the sugar is transformed into acid. The barytic salt is then dissolved in water, and the acid is precipitated by means of a solution of subacetate of lead, to be gradually added, in order first to separate a brown colouring matter which arises during the reaction, at least when operating in contact with the air. The last precipitate obtained is colourless, and contains the acid in the state of subsalt; it may then be separated by the usual means.

Besides this acid, another non-volatile body is produced, which possesses the property of immediately reducing, when cold, the salts of silver and of mercury.

The very easy formation of an acid, by the contact of sugar of starch or of grapes, with bases (M. Peligot observes), shows how proper it is to avoid the employment of too much lime in the purification of the beet-root juice; for although lime does not alter the sugar, it acts, when in excess, upon the sugar analogous to that of grapes, into which common sugar is easily converted by the influence of heat, acid, or fermentation. There are therefore two difficulties to be avoided; these may be apprehended at the same time,—the intervention of acids which decompose the sugar intended to be extracted, and the effects of the alkalies which act upon the sugar of starch resulting from this decomposition.—*L'Institut, Août,* 1837.

SULPHONAPHTHALIC ACID.

It was first observed by Mr. Faraday*, that when concentrated sulphuric acid was made to act upon naphthalin, two acids were produced, forming salts with barytes which were distinguished by their different solubility. Mr. Faraday considers these acids as formed by the direct combination of sulphuric acid with naphthalin; this was at first admitted by all chemists. More lately, and since the researches of M. Mitscherlich on the action of sulphuric acid upon benzon, it has been suspected that the composition of sulphonaphthalic acid might be analogous to that of sulphobenzic acid, especially as the analyses of Mr. Faraday and MM. Liebig and Wœhler did not agree well with their theoretic formula.

M. Regnault has endeavoured to clear up this subject; he has separately examined the action of common concentrated sulphuric acid, and that of the anhydrous acid upon naphthalin: the following are some of the results which he obtained. Sulphuric acid combined with one atom of water formed only one acid compound, which is separable from the excess of sulphuric acid by means of carbonate of barytes. The sulphonaphthalate of barytes, obtained by the cooling of a hot saturated solution, had the form of small crystalline tufts; but, by the spontaneous evaporation of a cold solution, it crystallizes in aggregated small irregular tables. This salt, which had been dried at 356° Fahr., gave by analysis $C^{20}\,H^{14}.\,S^{2}\,O^{5}\,BaO$; that is to say, common sulphuric acid produces on naphthalin the same reaction that the anhydrous acid does upon benzon. Two atoms of the hydrogen of the naphthalin remove one atom of oxygen from two atoms of sul-

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[*] Mr. Faraday's paper on this subject will be found in Phil. Mag. First Series, vol. lxvii. p. 326.

phuric acid, and the hyposulphuric acid formed combines with the modified naphthalin.

The crystallized sulphonaphthalate of barytes contains an atom of water, which it does not lose in vacuo; it is slightly soluble in water: 100 parts of water at 60° Fahr. dissolve 1·13 of this salt, and at 222° Fahr. 4·76 parts.

M. Regnault has analysed several other sulphonaphthalates, the composition of which indicated the same formula as the sulphonaphthalate of barytes. The oxide of lead forms with sulphonaphthalic acid a neutral salt and several subsalts; the latter are obtained by boiling massicot in the neutral solution of sulphonaphthalate; the sulphonaphthalate of potash crystallizes in very brilliant spangles: it contains one atom of water.

Free sulphonaphthalic acid is obtained by decomposing sulphonaphthalate of lead by means of sulphuretted hydrogen. This acid is extremely soluble in water and in alcohol, and by evaporation it is obtained in an irregular crystalline mass, and it is deliquescent in moist air. Its taste is very sour, astringent, and metallic; it readily fuses, becomes black, and if the temperature is increased, the smell of naphthalin begins to be perceptible; when more strongly heated it swells and leaves a very brilliant and bulky coal. The acid dried in vacuo contains three atoms of water of crystallization; it loses part of this water by the action of heat, but it decomposes before it is entirely dissipated.

Anhydrous sulphuric acid exerts a much more complicated action upon naphthalin; there are formed two acids producing soluble salts with barytes, and an insoluble matter. One of these acids is the common sulphonaphthalic acid; the other is a peculiar acid, which is distinguished from the first by the greater solubility of its salts. The salts formed by this acid cannot be obtained in crystals; they remain in an amorphous mass after the evaporation of the water, and there is no certainty as to their purity. Several analyses of the barytic salt, purified by solution in pyroxylic spirit, indicated its composition to be $C^{14} H^{10}. 2 SO^3 BaO$; but it does not appear how this is deducible from naphthalin. The insoluble matter, produced at the same time as the preceding acid, is a viscid mass, which appears to be a mixture of several substances. It has not been minutely examined. M. Regnault intends to continue his researches.—*Ibid.*

CONSERVATION OF LIVING PLANTS DURING LONG VOYAGES.

Extract from a letter from M. d'Eaubonne.

"Having constructed a case so that the air could not enter, by carefully fixing several bands of linen on all the joints with a glue not liable to alteration, I prepared," says M. d'Eaubonne, "with potters' clay, cow dung, and water a somewhat liquid mortar, in which I immersed the roots, having previously coated the stem; this being done, I covered them with moss and placed them in the case, filling the intervals carefully with straw, so that no friction might take place from the pitching or rolling of the vessel. I closed the case; and, after having used the same precautions for the exterior joints as for the inner ones, I had it placed in the hold of the vessel which was to carry it to the isle of Mauritius. The vessel arrived safe, the case was disembarked and opened before the customs, and instead of dry and sapless wood as was expected to be found, trees covered with leaves and flowers, much to our surprise were to be seen. These trees were afterwards distributed among several inhabitants of the colony."—*Comptes Rendus,* August, 1837, p. 260.

SHOOTING STARS.

M. Arago announces that an extraordinary appearance of shooting stars took place in the night of the 10th to the 11th of last August. His eldest son, who is no astronomer, and one of his friends, walking in the garden of the Observatory, counted not less than 107 between a quarter past 11 and a quarter past 12. From $12^h\ 37^m$ to $3^h\ 26^m$ at the beginning of the twilight, MM. Bouvard and Laugier, astronomical students, observed 184 of these meteors. The greater number seemed to take a direction towards Taurus, as was to be expected from the direction of the motion of translation of the earth.—*Comptes Rendus,* August, 1837, p. 184.

CORRECTION IN THE REV. N. S. HEINEKEN'S PAPER ON THE SHOCK-MULTIPLIER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I shall feel obliged if you will have the goodness to make the following correction in the paper relating to the "Shock Multiplier" inserted at page 460 of the last Number of the Philosophical Magazine. The passage commences at the fifth line from the bottom of the page, and should stand thus: "If a wire have a moist sponge attached to one extremity while the other is connected with one of the poles of a battery, and the hand grasp the moist sponge;—and if the other pole of the battery be connected by a wire with N, the other hand grasping a second sponge connected with the wire O, a rapid succession of shocks will, &c." I am, &c.

N. S. HEINEKEN.

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1837.

Chiswick.—Oct. 1. Overcast. 2. Slight rain. 3. Foggy. 4. Overcast: fine: clear and cold. 5. Foggy: clear. 6. Heavy rain: clear. 7. Foggy: very fine. 8. Overcast and fine. 9—14. Very fine. 15. Slight fog: fine. 16. Overcast. 17. Foggy: overcast and fine. 18. Overcast. 19, 20. Foggy: very fine. 21. Foggy: overcast. 22. Rain. 23. Fine: rain. 24. Heavy: rain. 25. Clear. 26. Fine. 27. Cloudy: rain. 28. Fine: rain. 29. Very clear: fine: overcast. 30. Rain. 31. Overcast and fine: clear and cold at night.

Boston.—Oct. 1. Rain. 2. Fine. 3. Foggy: rain P.M. 4. Fine: rain early A.M. 5. Fine. 6. Cloudy: rain early A.M. 7. Fine. 8. Cloudy: rain P.M. 9. Cloudy. 10. Fine. 11, 12. Cloudy. 13—15. Fine. 16, 17. Cloudy. 18, 19. Fine. 20. Fine: rain P.M. 21. Foggy. 22. Fine. 23. Cloudy. 24. Cloudy: rain P.M. 25. Cloudy: rain A.M. 26. Fine: ice this morning: rain P.M. 27. Stormy: rain early A.M. 28. Cloudy: rain A.M. 29. Fine: rain A.M. and P.M. 30. Rain: rain and stormy P.M. 31. Fine.

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

Days of Month. 1837. Oct.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Max.	Min.	Max.	Min.								
⊙ 1.	29·928	30·000	29·933	29·33	57·4	62·6	48·2	69	48	56	NE.	SW.	calm	·055	...	...	53
M. 2.	30·158	30·160	30·141	29·42	62·2	63·8	48·0	73	47	59	SSW.	SW.	calm	·025	·03	·12	58
T. 3.	30·174	30·167	30·072	29·55	59·3	67·2	54·0	72	54	56	SE.	SW.	calm	...	...	...	58
W. 4.	30·048	30·124	30·020	29·38	63·2	67·3	54·0	72	42	61	SW.	S.	calm	...	...	·15	58
Th. 5.	30·264	30·250	30·215	29·60	57·4	68·5	52·7	63	54	56	WSW.	S.	NW.	...	·29	...	57
F. 6.	30·088	30·130	30·078	29·40	59·3	61·8	55·0	66	40	57	SSW.	W.	NW.	·361	·09	·11	57
☽ S. 7.	30·246	30·247	30·139	29·63	53·7	63·4	49·2	66	42	56	SW.	W.	calm	·022	...	...	53
⊙ 8.	30·204	30·252	30·197	29·59	58·3	62·0	52·7	66	36	56	SSW.	W.	calm	...	...	...	54
M. 9.	30·304	30·318	30·308	29·76	53·8	61·5	47·4	64	36	49·5	W.	W.	W.	·033	...	·10	51
T. 10.	30·364	30·472	30·353	29·77	55·5	60·0	47·9	63	46	54	SW.	S.	W.	...	...	...	51
W. 11.	30·424	30·431	30·395	29·74	56·4	61·2	52·8	66	40	55	SW.	S.	W.	...	...	...	52
Th. 12.	30·470	30·535	30·461	29·92	52·4	62·7	49·2	63	36	53	SW.	E.	calm	...	...	...	50
○ F. 13.	30·596	30·631	30·600	30·04	47·7	60·5	43·8	57	40	45	N.	NE.	calm	...	...	...	44
S. 14.	30·708	30·713	30·701	30·23	47·2	56·5	43·9	57	30	39	N.	NE.	calm	...	...	...	45
⊙ 15.	30·640	30·664	30·576	30·10	44·2	55·6	38·5	57	35	43	W.	N.	calm	...	...	...	44
M. 16.	30·488	30·507	30·416	29·83	47·7	54·6	44·0	59	36	48·5	SW.	W.	calm	...	...	...	44
T. 17.	30·268	30·301	30·174	29·65	44·6	55·5	41·0	60	45	49	SSW.	SW.	calm	...	...	...	44
W. 18.	30·112	30·233	30·120	29·53	52·0	56·2	44·3	58	37	53	SW.	SW.	calm	...	·04	...	46
Th. 19.	30·400	30·444	30·406	29·81	45·2	58·4	44·0	60	39	42·5	S.	W.	calm	...	...	...	44
F. 20.	30·538	30·566	30·537	29·90	51·7	56·0	45·0	61	38	53	SW.	W.	NW.	...	...	...	48
☾ S. 21.	30·640	30·652	30·068	30·02	52·4	62·3	50·9	59	36	43·5	N.	SW.	calm	...	...	·02	49
⊙ 22.	30·456	30·446	30·310	29·80	49·4	56·2	47·0	59	49	52	SW.	SW.	W.	...	·02	...	47
M. 23.	30·046	30·047	29·731	29·44	54·7	58·3	49·2	61	47	53	SSW.	SW.	W.	...	·36	...	49
T. 24.	29·594	29·606	29·519	29·	50·4	60·2	50·2	50	32	49	SSW.	S.	calm	·388	·30	...	49
W. 25.	29·700	30·021	29·665	29 25	42·4	52·7	33·3	51	27	41	N.	N.	N.	·250	·10	1·06	42
Th. 26.	30·062	30·072	29·837	29·55	42·3	49·4	35·4	54	40	39	SW.	SW.	NW.	·061	...	·12	40
F. 27.	29·486	29·655	29·523	28·94	53·4	54·2	42·0	54	35	52	S. var.	SW.	W.	...	·09	·06	48
S. 28.	29·610	29·651	29·307	29·16	47·3	54·0	40·8	49	34	41	S.	S.	W.	...	·49	·07	42
● ⊙ 29.	29·458	29·478	29·424	29·	41·2	51·3	38·8	55	32	40	SW.	SW.	W.	·477	·04	·02	40
M. 30.	29·350	29·403	29·287	28·77	48·2	51·3	38·7	59	40	43	S.	SW.	W.	·041	·20	·03	42
T. 31·	29·356	29·530	29·362	28·82	45·0	58·2	43·2	56	38	43	SW.	W.	W.	·108	·34	·10	43
	30·135	30·183	30·060	29·54	51·5	58·8	46·1	60·4	39·6	49·6				Sum 1·821	2·39	1·96	48·4

INDEX TO VOL. XI.

END OF THE ELEVENTH VOLUME.

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