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PLATES IN VOL. X. (*New Series.*)

ERRATA.

Page 46, *for* 30 inches, *read* 29·92181 inches.
123, line 13, *for* fluoric acid, *read* fluoboric acid.
124, line 12, *for* neutral, *read* mutual.
152, line 5, *for* analysis, *read* analyses.

ANNALS

OF

PHILOSOPHY.

JULY, 1825.

ARTICLE I.

Essay on the Variation of the Mariner's Compass, its Cause, and the periodical Revolution of the Magnetic Pole. By Mr. C. Boner.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Great Bedford-street, Bath.*

THE direction of the magnetic needle towards one particular point in the horizon evinces a power, by which the needle is attracted.

The progressive change in the direction of the needle proves a change of position in the attractive power.

The consideration that every thing in nature is governed by invariable laws, places it beyond a doubt that the change of position in the attractive force, and consequently the variation of the needle, are governed by laws as constant as those by which all the other phenomena of nature are regulated.

Whether the cause of the variation, that is to say, the power which acts upon the compass needle, be in the earth, or in the heavens, is a question which I do not presume to decide; because, after the consideration that the most able men have been unsuccessful in their efforts to establish their theories upon solid grounds, and that besides circumstances have never permitted me to make the necessary experiments, from which I might have drawn just conclusions, I should think it ridiculous conceit, if I were positive in affirming that such and no other is the efficient cause of this extraordinary phenomenon.

However that neither of the existing systems is the true one, is evident from their disagreement with daily experience; and, therefore, every new conjecture on so important a subject is worth examining.

That there should be within the earth, as some pretend, a large magnet, revolving about a center, as the planets move about the sun, is possible, but not probable; and still less so is the opinion of those, who attribute the variation to the changes produced in the iron mines by the continual excavations made therein by men's hands.

The variation of the compass being evidently the effect of attraction, would it not be more philosophical to look for its cause in that universal power, by which the whole planetary system is acknowledged to be governed. Few persons deny the influence of the sun and the moon in raising the waters of the sea; and I can see no incongruity in the idea, that the variation is an effect of the same, or at least of a similar cause.

Repeated observations prove that at the same place the variation is different at different hours of the day; and Mr. Canton affirms, that in 574 observations he found the variation regularly increasing westward from about eight or nine in the morning till one or two in the afternoon; when the needle became stationary for some time, after which the absolute variation westward was decreasing, and the needle came back again to its former situation or near it in the night, or by the next morning.

I ask now whether there is any thing more like the periodical rise and fall of the waters of the sea, which, as every body knows, happen twice every four and twenty hours, and considering that the diurnal east and west variations are very nearly at the same distance from noon, I have little doubt but that if the observations were made as regularly during the night, we should discover the same changes before and after midnight, when the sun is in the opposite meridian, and thus find two magnetic tides, if I may use the expression, as we have two sea tides every day. It appears therefore not at all improbable to me, that the periodical change of the variation should be regulated by the situation of the heavenly bodies, and the whole revolution of the magnetic pole be performed within the space of 532 years, a period derived from the multiplication of the numbers 19 and 28; that is, of the lunar and solar cycles together.

But what confirms me still more in the idea that the principal cause of variation resides in the region of the planets, is, that by the first trial which I made to discover the place of the magnetic pole by means of the dip and variation observed in 1812, I found the annual progress of the magnetic pole in direct proportion as the annual progress of the nodes of Venus to the annual precession of the nodes of the earth; that is, as $31''$: $50''\cdot25$ :: annual progress of the magnetic pole : to one degree or 60 minutes; which gives the annual progress $37'\ 00''\ 53'''\cdot73$. And from the dip and variation observed in London in the year 1812, I find the annual progress of the magnetic pole, as it will be seen hereafter, equal to $37'\ 53''\ 11'''\cdot22$, differing only $52''\ 17'''\cdot49$ from the former; and if to the first $37'\ 00''\ 53'''\cdot73$ we add the annual precession of the equinoxes $50\frac{1}{4}''$ seconds, we obtain 37′ 51″ 8‴·73 for the annual progress of the magnetic pole as it is derived from the precession of the nodes of Venus and of the earth, being only 2″ 2‴·49 less than that derived from the dip and variation observed in 1812.

This calculation is upon the supposition that it be true, as it is generally believed, that there was no variation at London in the year 1657; an opinion upon which there can remain little doubt, when we consider that Mr. Gunter in the year 1622, that is 35 years before there was no variation, observed it to be 5° 56½′ east, and Mr. Halley in 1692, that is 35 years after there had been no variation, found it 6° west.

Now from dip and variation observed in London in 1812, that is 155 years after the time of no variation, I find the longitude of the magnetic pole 82° 7′ 36″ west of London, and as in 1657 it was evidently in longitude 180°, it must have proceeded during the 155 years, 97° 52′ 24″, which being divided by 155, give 37′ 53″ 11‴·22 for the annual progress of the magnetic pole, as it has been stated above, and the whole revolution = 570·1246 years, or 570 years and about 45½ days.

If the annual progress be made as tropical revolution of Venus to tropical revolution of the earth, we obtain 36′ 54″ 42‴, which is nearly one minute less than the above, and the whole revolution would require about 12 years more. If on the contrary we assume for the annual progress 38′ 51″ 28‴, which is nearly one minute more than the first, the whole revolution will be about twelve years less; namely, 557 years, 21 days, 18ʰ 11ᵐ 51ˢ, forming the astronomical period, called the period of eclipses. Further observations, it is to be hoped, will enable us to decide which of these data comes nearest the truth. Meanwhile let us observe that a mean between the four following numbers differs less than half a minute from the annual progress of the magnetic pole as derived from the dip and variation observed in 1812.

Annual progress deduced from annual precession of the nodes of Venus and the earth ..	37′	00″	53‴·73
From tropical revolution of Venus and the earth	36	54	42
From period of eclipses.	38	51	28
From solar and lunar cycles.	40	36	05·4
4)	153	23	09·13
Mean term	38	20	47·28
From dip and variation of 1812	37	53	11·22
Difference	0	27	36·06

In the calculations of the dip and variation I have taken the dip inversely as the distance from magnetic poles, of which I am persuaded there are but two diametrically opposite to one another, and not four irregularly dispersed within the earth, as some have pretended.

I have further to observe in favour of my theory that, as the revolution of the magnetic pole seems intimately connected with the relative situations of the planets, so does its latitude appear to be regulated by the inclinations of their axes.

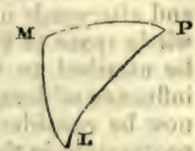
The inclination of the axis of the sun is allowed to be 8° or nearly so, which being subtracted from 23° 28′, the inclination of the axis of the earth, leave 15° 28′, which is very nearly the distance of the magnetic pole from the pole of the earth, as deduced from the dip and variation of 1812, namely 15° 17′ 23″. And if the latitude of the magnetic pole be supposed equal to the inclination of the axis of Venus, its distance from the pole of the earth will be 15°, and consequently again very near that found by calculation, which, it is well worth observing, is almost a perfect mean between the two numbers 15° 28′ and 15° resulting from the inclinations of the axes of the sun, of the earth, and of Venus, the difference being less than $3\frac{1}{4}$ minutes.

The celebrated Euler, suspecting the cause of the variation, like all the others that have occupied themselves about this subject, to be within the earth, fixed the north magnetic pole in longitude 96° west of Teneriffe, which being reduced to the meridian of London brings it to 112° 23′, which is the very point where it must have been then, in case its whole revolution be equal to the period, formed of the lunar and solar cycles.

When we reflect upon this perfect conformity with the revolution of the planets, we can hardly entertain a doubt concerning their ruling power over the magnet. We acknowledge their power over the waters of the sea, not because we see them really at work, but because time, place, and degree of elevation, are always in perfect harmony with their respective situations. From them we have learnt to foretel the time of setting in, and the quantity of the tide at any place; and from them we may expect to learn the time and place for any particular degree of variation. But it will require yet some labour and reflection before we shall be able to pronounce with certainty upon the extent of their influence. Could we depend on the correctness of former observations, the work would be greatly abridged, but the errors of many are obvious. For example, the same year 1657 is said to have been that in which there was no variation at Dublin and at London, which is impossible, if the variation follows any rule at all, as I have no doubt it does. For Dublin being 6° 6′ to the west of London, the time when there was no variation there must have been about nine years before the same could be observed in London. Again I find that there was no variation at Paris in 1666; that is, nine years later than in London; but Paris being 2° 20′ east of London ought to have experienced the same but about four years later. I find other instances where the variation amounted to about 7° in 68 years, and afterwards in the very same place to only 15 minutes in the whole space of 60 years. These contradictions must probably be ascribed to the incorrectness of the instruments, and the influence of local attraction. Both these inconveniences may now be considered as almost totally removed by the great improvements in the construction of compasses, and by Dr. Barlow's apparatus to counteract the influence of iron. The best instruments, however, are still found to have some small defects, which must be ascertained before we can use them with advantage. I see in Capt. Parry's voyage in the years 1819 and 1820, that at several places the variation was observed with four compasses, all of the best workmanship, and yet they all disagreed, and consequently three of them at least, if not all, must have been wrong. But this is no objection to their utility, for if after the experiment has been repeated with all of them in different parts of the world, their differences are found to bear always the same proportion, it is certain that we may then conclude from the variation of any single one, what would be that of either of the remaining three; and if all the compasses made use of for discovery had first been tried in this manner, we might then be able to reduce all the observations to one common standard, in the same manner as we may acquire the very same notions of the state of the atmosphere with barometers and thermometers of different constructions. I have no doubt, but that some regard must be paid to the temperature of the air, besides the respective situation of the sun, moon, and the earth. The latitude of the magnetic pole might also be sometimes increased or diminished according to the declinations of these objects, and it remains to be determined, whether it revolves in a circle, or in an ellipsis, As I have neither instruments, nor any of the resources requisite for the investigation of so intricate a subject, I must resign the honour of deciding these questions to those, whose happier circumstances allow them to indulge themselves in the daily contemplation and admiration of the wonders of the creation.

After having conceived the idea of establishing my theory on the revolution of the heavens, I chose, to prove it, the observation made in London by Dr. Gilpin in 1812, the only one of which I had also the dip. The result is, as I have mentioned before, that the annual progress of the magnetic pole round the pole of the earth is nearly in direct proportion as annual progress of the nodes of Venus to the annual progress of the nodes of the earth. It would have contributed greatly to my satisfaction, if I had been able to obtain the same result from two or more good observations made at the same time in different latitudes and longitudes, but these I could not procure; for those, which I might have taken from several voyages, were either not free from local attraction, or in other respects unfit for the purpose.

At London, in 1812, the variation was 24° 16′ W. and the dip 70° 32′. To find the place of the magnetic pole from these data.



P, the pole of the earth.

M, magnetic pole.

L, London, latitude 51° 31′ N.

P L, complement of latitude 38° 29′.

M L, complement of dip multiplied by 2 = 38° 56′ ∠ L = 24° 16′.

1. Cosine $\frac{PL + ML}{2}$ = 38° 42½′ ...... 9·892284

2. Cosine $\frac{ML - PL}{2}$ = 0 13½ ...... 9·999997

3. Cotang $\frac{\angle L}{2}$ = 12 8 ...... 10·667500

20·667497

4. Tang. $\frac{\angle\angle M + P}{2}$ = 80° 28′ 28″... 10·775213

5. Sine, No. 1 .... 9·796127
6. Sine, No. 2 .... 7·588760
7. Cot. No. 3 .... 10·667500

18·256260

8. T. $\frac{\angle P \infty M}{2}$ .... 8·460133 = 1° 39′ 8″

Hence ∠ P = 82° 7′ 36″
∠ M = 78 49 20

Sine, No. 8 .... 8·459882
Sine, No. 4 .... 9·993970
Tang. 2 .... 7·593764

17·587734

Tang. $\frac{PM}{2}$ .... 9·127852 = 7° 38′ 41½″

Hence P M = 15° 17′ 23″

Therefore the longitude of the magnetic pole in 1812 was 82° 7′ 36″ west of London, and its latitude 74° 42′ 37″. In 1657 its longitude was 180; therefore in 155 years it has proceeded 97° 52′ 24″, or 37′ 53″·187 per annum.

A table of the progress of the magnetic pole as derived from the dip and variation observed in London in the year 1812.

Number of Years.				
1	0°	37′	53″	11‴·22
2	1	15	46	22·44
3	1	53	39	33·66
4	2	31	32	44·88
5	3	09	25	56·10
6	3	47	19	07·32
7	4	25	12	18·54
8	5	03	05	29·76
9	5	40	58	40·98
10	6	18	51	52·20
1 month	0	03	09	25·93

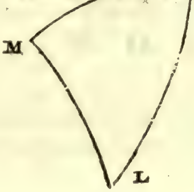
Progress of the magnetic pole according to a mean between that deduced from

1. The annual precession of the nodes of Venus and of the earth.
2. The tropical revolution of Venus and the earth.
3. The period of eclipses.
4. The Dyonisian period formed of the solar and lunar cycles.

Number of Years.				
1	0°	38′	20″	47‴·28
2	1	16	41	34·56
3	1	55	02	21·84
4	2	33	23	09·12
5	3	11	43	56·40
6	3	50	04	43·68
7	4	28	25	30·96
8	5	06	46	18·24
9	5	45	07	05·52
10	6	23	27	52·80
50	31	57	19	24·00
100	63	54	38	48·00
1 month	0	3	11	43·814

The whole revolution in 563·28 years, which is very nearly the period of eclipses, and may probably require to be reduced to it to be perfect. A few examples calculated according to this table will leave little doubt of its being nearly correct.

London, 1812.



$\angle$ P = 80° 56′ 17‴·86
$\angle$ L = 24 16 30·00
P L = 38 29 00·00

1. Cos. $\frac{\angle P + L}{2} = 52° \; 36' \; 24''$ ...... 9·78340

2. Cos. $\frac{\angle P \infty L}{2} = 28 \; 19 \; 54$ ...... 9·94459

3. T. $\frac{PL}{2} = 19 \; 14 \; 30$ ...... 9·54289

19·48748

4. T. $\frac{PM + ML}{2} = 26 \; 50 \; 07$ ...... 9·70408

5. Sine, No. 1 .... 9·90008
6. Sine, No. 2 .... 9·67630
7. T. No. 3 .... 9·54289

19·21919

8. T. $\frac{PM \infty ML}{2}$ ...... 10·31911 = 11° 46′ 39″

Therefore M L .... = 38 36 46
P M .... = 15 03 28
Dip = 90 − ½ M L. .... = 70 41 27
By observation 70 32 00

Difference 0 09 27
Sine 8 .... 9·30987
Sine 4 .... 9·65459
T. 2 .... 9·73154

19·38613

Cotan. $\frac{\angle M}{2}$ .... 10·07626 = 39° 59′ 16″

$\angle M = 79 \; 58 \; 32$

For 1814. $\angle P = 79° \; 39' \; 36\frac{1}{4}'''$
P L = 38 29
P M = 15 03 28 according to the preceding example.

1. Cos. $\frac{PL + PM}{2} = 26° \; 46' \; 14''$ ...... 9·95077

2. Cos. $\frac{PL \infty PM}{2} = 11 \; 42 \; 40$ ...... 9·99087

3. Cot. $\frac{\angle P}{2} = 39 \; 49 \; 48$ ...... 10·07881

20·06968

4. T. $\angle \frac{M + L}{2} = 52 \; 44 \; 50$ ...... 10·11891

5. Sine 1.	9·65362			
6. Sine 2.	9·30750			
7. Col. 3.	10·07881			
	19·38631			
8. T. $\frac{\angle M \infty L}{2}$.......	9·73269 =	28°	23′	08″
Therefore ∠ M	=	81	07	58
∠ L.	=	24	21	42
Was observed in August	=	24	21	10
Difference	=	0	0	32

Sine 8....	9·67706
Sine 4....	9·90089
T. 2.....	9·31664
	19·21753

T. $\frac{M\ L}{2}$ .. 9·54047 = 19° 08′ 33″ complement of dip.

Dip. .. = 70 51 27
M L .. = 38 17 06

How much the dip was by observation, I do not know; but to judge by that of 1812, this cannot be far from the truth, because it must necessarily have increased since.

According to the series of observations placed at the end, with which I have been favoured lately by some gentlemen of the Royal Society, it appears that the variation had attained the highest degree in 1814, and that it has been gradually diminishing from that time. This diminution, however, is not a sufficient proof of the actual return of the needle towards the north: for I find, from a number of observations made by Dr. Gilpin, that a much greater diminution had taken place in an equal space of time to that from 1814 to 1824, after which the variation was found to increase again; namely, in 1800 it was observed 24° 36′ W. and in 1809 it was found by the same gentleman 24° 11′; that is, 25 minutes less; after which it appears to have continued to increase until 1814, when it amounted to 24° 21′ 10″, and has retrograded since, in the space of ten years, to 24° 9′ 33″, which is only 11′ 37″ less than the greatest of 1814. If, therefore, 25′ diminution in nine years did not confirm the return of the needle towards the north, much less can 11′ 37″ diminution in 10 years confirm such a return; and the question must as yet remain undecided. I have further to observe that the ∠ M in the last example, being less than 90°, supposing the data correct, is an evident proof that the variation had not then attained its highest degree, which can only take place when that angle is 90°, which, according to the last given annual progress, will be in the year 1829, when the variation will be 24° 40′ 31″; the dip 72° 04′ 44″, and the longitude of the magnetic pole 70° 12′ 52″, its latitude 74° 56′ 32″.

When I say that these things are to take place, it will be understood of course that I do not pretend to affirm it positively, but only so far as the observations which I have made subservient to corroborate my theory, are correct. So much, I am confident, is true, that the annual progress cannot differ much from what I have stated it to be, and that in every case it will be found dependant on some one of the astronomical periods which I have mentioned. An inquiry into the cause of variation seems therefore to be a subject as much deserving the attention of the astronomer as that of the tide and the monsoon, with both of which it is probably connected. The result of his observations being every year inserted in the Nautical Almanac, would, I should think, be of material service to the mariner. I need not say, how easy it would be to find the longitude, if we could depend on the exactness of the dip and variation, as this must be obvious to every mathematician. It is therefore of the greatest importance, that we should learn to determine the true dip and variation, though the instruments made use of should not be quite perfect, in the same manner as we may know the exact time of the day by means of an incorrect watch, as soon as we are acquainted with its defects. I have been told that the dip cannot be depended on so much as the variation; but on examining a series of observations made by Capt. Parry and Capt. Lyon, I find to the contrary the dip almost constantly very nearly, what I should have expected it to be, whilst the variation bears not the least resemblance to truth; for I see it constantly west, when I had every reason to think it would have been east. The only way of accounting for this contradiction is to suppose that the poles of the needle have been changed, a fact which I have often witnessed myself; and concerning the reality of which the following passage, taken from the Imperial Encyclopædia, will remove every doubt:—

"Although magnetic attraction generally takes place only between the opposite poles of two magnets, yet it often happens that though the north pole of one magnet be presented to the north pole of another, that they show neither attraction nor repulsion; but that when placed very near each other, they will attract. For it often happens that one of the magnets, being more powerful than the other, will change the pole of that other magnet, and then an attraction will take place between two poles apparently of the same name, though in fact it is an attraction between poles of different names, because one of them has actually been changed."

Confirmed in my opinion by this passage, I concluded that if the poles of the needle had exchanged places, that end of it which, without such a change, would have pointed eastward, must now necessarily point westward, and that to judge rightly of the variation, we ought to make it equal to the supplement of that marked by the compass, and call it by a contrary name. Experience, however, did not quite answer my expectations; for in the first example, where I supposed the variation would turn out to be 61° 44′ E, being the supplement of 118° 16′ W; computation gave only 53° 50′ 26″; that is, nearly eight degrees less; but the dip was only 38′ 36″ less than that by observation.

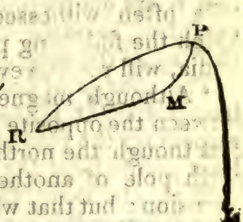
Suspecting now that the dip observed might be nearly correct, and having little doubt but that the longitude and latitude of the magnetic pole, calculated for the time of observation by the table of mean annual motion; namely, 38′ 20″ 47‴·28, would not be far from the truth, I concluded that the dip, with the latitude of the place of observation, would suffice to find the longitude in spite of the irregularity of the compass and of time-keepers, without any astronomical observation but what was requisite for the finding of latitude. The result was satisfactory beyond all expectation; the difference of longitude observed and that by computation being but half a minute.

Observations from Capt. Lyon's last Voyage.

1820.—Regent's Inlet,	Latitude	72°	45′ N
	Longitude	89	41 W
	Dip	88	26
	Variation	118	16 W
Computed latitude of magnetic pole.		71	10
Computed longitude of ditto		98	16

Supposing the longitude unknown, how could it be found by these data, of which the three last, owing to the irregularity of the compass, are positively wrong?

Solution.



P M, complement of latitude of magnetic pole .... 15° 03′ 28″

P R, complement of latitude of Regent's Inlet 17 15 00

M R, double complement of dip corrected 87° 47′ 24″ 4 25 12

P L, meridian of London, and angle MPL, longitude of magnetic pole in 1820 at 0° 38′ 20″ 47‴·28 annual progression = 75° 49′ $31\frac{1}{2}$″, its latitude considered an invariable quantity = 74° 56′ 32″.

Sine P M = 15° 03′ 28″ . ... 9·41462
Sine P R = 17 15 00 . ... 9·47209

Sum .... 18·88671

Complement of sum				1·11329	(*a*
Sine of $\frac{1}{2}$ sum of three sides	18°	21″	50″	9·49837	(*b*
Sine of differ. of $\frac{1}{2}$ sum and side M R	13	56	38	9·38196	(*c*
Sum of *a*, *b*, *c*.				2)19·99362	
Cosine $\angle \frac{P}{2} = 6° 56''$				9·99681	
Therefore $\angle$ P .... =	13°	52′			
+ Longitude of magnetic pole, or $\angle$ M P L =	75	49	$31\frac{1}{2}''$		
Hence long. of place of observation	89	41	$31\frac{1}{2}$		
Long. observed.	89	41	00		
Difference	0	0	$31\frac{1}{2}$		

After so perfect an agreement between observation and computation, I have reason to think that I had determined the true place of the magnetic pole for the time of observation, and that consequently it may, upon the same principle, be determined for any particular time, its revolution being necessarily as regular as the course of the stars. If this regularity be once irrevocably established, we shall have one side P M of the triangle, and the angle M P L constantly given, and P R, the complement of latitude, mostly without much difficulty. If then by any means we can succeed in discovering the error either of the variation, or the dip, we shall always have a sufficient number of data for the discovery of longitude. I know that the longitudes and latitudes of the magnetic pole calculated by Capt. Parry and other gentlemen, agree with their relative variations and dip, but they must be, calculated for every new observation, and every new result contradicts all the preceding operations, which, I am confident, is owing to an error of the compass, and not to a real shifting of the magnetic pole.

The change of position in the pole is slow and regular, and I have sufficient reasons to believe that I have not erred much respecting the rate of its periodical progress, or that I have at least pointed out the means of determining it.

If this be done, the most important question will be, how we may discover the errors of the compass and of the dipping needle, if we cannot prevent them? I should think, that the following experiment might be a means of obtaining that end, or at least of facilitating our research after it. Having determined the situation of the magnetic pole by the preceding table for any particular time in London, and calculated the variation and dip accordingly, then an observation made with a compass and dipping needle would show, how near their quantities agree with those found by calculation, and the same operation being repeated, with the very same instruments, at different places, at a proper distance from one another, then, if the differences between the quantities calculated and the quantities observed, bear every where the same ratio, it is obvious, that these instruments, were they ever so defective, would become as useful as the most correct, since we could always make a proper allowance for their defects.

Perhaps as good, and a shorter way would be, to make the observation with several compasses, and as many dipping needles, marked A, B, C, D, and having noted the difference of each, and the variation and dip found by computation, one might be sent to Petersburgh, another to Berlin, a third to Vienna or Paris, and a fourth to Edinburgh, and a particular day and hour appointed for the observations to be made. The result of these observations would also be particularly useful to correct any error respecting the place and periodical revolution of the magnetic pole. That we ought to make the compass to agree with the place of the magnetic pole, and not the situation of that pole with the compass, will not be disputed, unless it were by one, who would regulate the sun by the clock instead of regulating the clock by the sun.

Magnetic Variations.

1811—Sept.	24°	14′	02″	
1812—Oct.	24	16	30	
1813—Sept.	24	16	40	
1814—June	24	16	42	
July	24	17	54	
Aug.	24	21	10	greatest
Sept.	24	20	33	
1815—June	24	17	50	
1816	24	17	54	
1817	24	17	00	
1818	24	15	43	
1819	24	14	47	
1820	24	14	44	Dip about 71° 6′
1821	24	11	18	
1822	24	09	55	
1823	24	09	48	
1824	24	09	33	

Article II.

On a Compound of Iodine and Carbon. By Mr. M. Scanlan.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Dublin, April* 21, 1825.

In contriving a process for making iodide of potassium, which struck me as less objectionable than those with which I was already acquainted, I have formed a combination of iodine and carbon which, so far as I know, has not been described by chemists.

It may be thus obtained:

Add to an alcoholic solution of iodine caustic potash till the colour be destroyed, the liquor becomes turbid, and a white crystalline deposition ensues which is iodate of potash: distil, with a very gentle heat, the alcohol from the clear liquor which is yellowish; and on cooling, this substance is deposited in small micaceous plates, opaque, and of a bright sulphur-yellow colour: the solution of hydriodate of potash retains obstinately a portion of it which cannot be separated without decomposition.

Iodide of carbon, if I may so name it, has a powerful and aromatic odour somewhat resembling saffron. It is soluble in alcohol, and precipitated therefrom by water yellowish-white. It rises in distillation with water unchanged, but is readily decomposed by a heat little higher than boiling water. Exposure on a piece of writing paper over the flame of a candle renders visible the violet vapour of iodine: heated in a glass tube, it melts, and is decomposed; iodine sublimes leaving carbon.

Heated with iron or zinc, an iodide of the metal is formed.

Its alcoholic solution by spontaneous evaporation yields slender prismatic crystals.

I have not yet made any very satisfactory experiments to ascertain the proportions of its elements, but it appears to me, from such trials as I have made, that the best method of analysis would be to expose it to a sufficient heat in a sealed tube mixed with iodide of potassium; the iodine sublimes into the cooled end, and the carbon may be separated from the iodide by means of water.

In this way the weight of each element might be determined in the one operation. M. Scanlan.

Note.—Mr. Faraday has had the goodness, at my request, to examine the specimen of the supposed iodide of carbon, forwarded to us by Mr. Scanlan. The following is the report we have been favoured with by him on the subject. Our readers will recollect that we owe the discovery of the *hydriodide of carbon* to Mr. Faraday.—C.

DEAR SIR, *Royal Institution, June* 9, 1825.

I RECEIVED the substance you refer to in your note a day or two ago from Mr. Brande, and have examined it so far as to be able to say it is not the hydriodide of carbon which I made. With reference to its being an iodide or an hydriodide of carbon, I am not quite so sure. It is I perceive the same substance as was shown to me by Mr. Cooper, I should think, two years or more ago, and which he obtained during the preparation of iodine in large quantities. Mr. Cooper considered it at that time as an iodide of carbon, analysing it by passing it over oxide of copper. I saw many of his experiments, but was not quite convinced that it contained no hydrogen. I know he intended to publish his experiments, but have heard nothing of them since.

The number of iodine is so high, and the number of hydrogen so low, that it is very difficult to detect and confirm the existence (or to disprove it) of one proportional of the latter in compounds of the former; a proportional of hydrogen coming within the limits of probable errors in experiment. I am of opinion that the compound you sent me contains hydrogen; indeed I am certain of it, for when distilled in contact with, and over heated zinc filings, a gas is evolved which is combustible, and contains hydrogen; but whether this substance results in consequence of its existence in the compound, or whether it is due to adhering moisture, is more than I could say without entering into a course of precise experiments. It appears to me also to resemble the hydriodide of carbon which Serullas obtained.—(Ann. de Chim. xxii. 172, or Journal, xv. 297.)

I am, dear Sir, yours very truly,

J. G. Children, Esq. M. FARADAY.

ARTICLE III.

Facts proving the Efficacy of Sir H. Davy's Method of protecting the Copper of Ships by Electrochemical Action. Extracted from the Letters of a Correspondent, and Dr. Stewart Traill.

1. The *Carnebrea Castle*, an Indiaman, belonging to Messrs. Wigram, of 650 tons burden, was protected last spring by a quantity of iron in four portions, two on the bow, and two on the stern, equal to from $\frac{1}{100}$ to $\frac{1}{110}$ part. She has since made the voyage to India, and was for some time in the Ganges.

She appeared bright and clean during the whole of the voyage out and home; some mud collected on her bottom in the Ganges; but immediately disappeared when she began to sail. She was put into dry dock about a fortnight ago, and her bottom examined by Sir H. Davy, the proprietors, and various other persons. *Every part* of her bottom was bright and clean without a single adhesion of any kind, and as far as could be judged from the smoothness and appearance of the copper, it had not been at all worn by any chemical corrosion. The iron, which was about an inch and half in thickness, is considered a sufficient protector for two voyages more.

2. The *Elizabeth* yacht, belonging to the Earl of Darnley, was protected by two pieces of malleable iron in the stern, in May last, equal to about $\frac{1}{125}$ of the surface of the copper. After being employed in sailing during the summer, she was examined in November, when her bottom was found free from adhesions of any kind, and apparently untouched. The copper was bright, and even the nails not tarnished. In the course of the summer a few small barnacles had adhered to the rust of iron, which were easily and immediately washed off; but no weed or shell fish had ever fixed on the copper, which appeared in the same state as when she left the dock.

The following examples we owe to the kindness of Dr. Traill:

The ship *Huskisson,* belonging to Mr. Horsfall, was lately in dock after a voyage to and from Demerara, where she lay some weeks, in a river remarkably favourable to the adhesion of parasitical animals and weeds; yet, when I examined this vessel, her copper appeared perfectly clean, as far as it could be seen, when she was purposely *set by the stern* in unloading, in order to show her copper at the *bows* as low as possible. The Captain stated that before coming into port, while yet in clear water, he had seen her bottom even to the keel; and it seemed to him quite clean. This ship was *defended* by two bars of malleable iron bolted along the sides of her keel by copper fastenings, which covered about $\frac{1}{90}$ of the surface of her copper.

The Elizabeth, a vessel defended exactly in the same manner, with metals in the same proportions, had made the same voyage. Both had been newly coppered when they last left Liverpool; and the Elizabeth's copper appeared equally clean as that of the *Huskisson* when unloaded; but as she did not enter a *graving* dock, we cannot absolutely say whether she was quite clean, especially as the copper of the Dorothy (about to be mentioned) appeared equally so, until she was seen in the graving dock, when the flat part of her bottom was found to be quite covered with barnacles. The copper of the *Huskisson,* there is reason to believe, was *perfectly clean,* as was proved in the next case,

The ship *Dee.*—A very large vessel belonging to my relative, Mr. Sandbach. This ship was newly coppered about twelve months ago, and a bar of malleable iron, about $\frac{7}{8}$ of an inch thick, and three inches broad, was fastened on each side of the keel by *iron* spikes. It covered about $\frac{1}{90}$ of the surface of her copper. Since that period she has made two voyages to Demerara, and was, at the conclusion of the last, put into a graving dock, when her copper was found perfectly free from corrosion, and there were scarcely any substances adhering to it, except a very few minute barnacles, near the keel *fore* and *aft*. This case shows that *over defence* was not the cause of the foulness of the bottom of the *Tickler;* for both in this vessel and in the *Huskisson*, the proportion of iron to the copper was greater than in that ship. The iron spikes employed to fasten the iron on the keel of the Dee, were so much corroded, as to endanger the falling off of the bars; copper nails are, therefore, to be preferred.

The Dorothy.—Dr. Traill states, that the following particulars of the Dorothy's outfit and return, were communicated to him by his intelligent friend Mr. Horsfall, one of the owners of the ship in the beginning of May:—

"The *Dorothy* had been coppered about a year, and had made one voyage to Bombay and back to this port, when in May, 1824, it was determined to place bars of iron four inches broad, and one inch thick, along her keel, covering about $\frac{1}{70}$ part of the copper, in the expectation that the iron would at least so far preserve the copper from corrosion that it might be permitted to run a second voyage to India without being renewed, which can seldom be done with perfect safety. The iron extended from one end of the keel to the other, and was fastened on with copper nails with large heads. The Dorothy thus defended sailed again for Bombay in June, and returned to Liverpool about a month since. She was put into the graving dock yesterday (May 3), and an examination of her bottom took place as soon as the water had left her.

"The copper appeared no more reduced than at the termination of the first voyage. The iron was diminished generally about $\frac{3}{4}$ inch in breadth, and from $\frac{1}{4}$ to $\frac{1}{2}$ an inch in thickness. At the ends of the vessel, for about two or three feet, the iron was much more reduced than at any other part. It was covered with the usual rust, not at all resembling cast iron, under similar circumstances. The flat of the ships' bottom, from end to end, and from six to eight feet in breadth, was full of *fleshy barnacles* (lepas anatifera) of uncommon length, and a few of the large hard shell species (balanus tintinnabulum).*"

What remains of the iron is still considered a sufficient protection for a third voyage to India, and "it appears only to be necessary to drive the large copper nails up a little to secure the iron bars for the next voyage."

Note by Dr. Traill.—We remarked that the specimens of the

* Sulphuric acid was used to loosen and detach the shells.

lepas anatifera were considerably larger on the *starboard* than on the larboard side of the ship. On noticing this to the Captain, he informed us that the *larboard* had been the *lee* side of the vessel, almost constantly during the passage to Europe, and consequently most deeply immersed in the water—a circumstance in the economy of these animals not unworthy of notice.

It is evident that in all these last cases, particularly in the ship Dorothy, the proportion of iron has been too large, and the quantity of calcareous earth on the bottom of this ship proves that the electro-negative action has been in excess.—C.

Article IV.

On Diluvial Formations. By Professor Sedgwick.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Trinity College, Cambridge, May,* 1825.

The following remarks on certain *diluvial deposits* form a supplement to a paper which you did me the honour to publish in the *Annals of Philosophy* for the month of April. Circumstances, over which I have had no control, have prevented me from resuming the subject sooner; but I venture to hope that the statements which are now offered for insertion in your journal, will be found sufficient to explain and vindicate the opinions advanced in my former communication.

I have the honour to be, Gentlemen,

Your most faithful servant,

A. Sedgwick.

Separation of Alluvial and Diluvial Formations.

In my former paper on the origin of *alluvial* and *diluvial* formations, I endeavoured to explain the nature of the evidence on which the two classes of deposits had been separated from each other; and I also endeavoured to show, that diluvial formations have not originated in a succession of partial and transient inundations occasioned by the bursting of lakes, or by the *ordinary* operation of any cause with which we are acquainted. The last conclusion might, perhaps, be established by showing the constant order in the position of the two deposits, and the different suites of organic remains contained in them. It derives, however, its most direct support from the two following considerations: 1. That, with very limited exceptions, the earth's surface exhibits no traces of ancient lakes capable of producing any portion of the superficial gravel. 2. That admitting (although against direct evidence) the existence of such ancient lakes, we shall not, by that hypothesis, introduce an agent capable of producing the *diluvial debris* which is exhibited on almost every part of the earth's surface which has been well examined.

In illustration of the first of these two assertions, I need only state, after Prof. Buckland, that in none of the higher parts of England out of the reach of ordinary floods, have any traces been yet discovered of lacustrine terraces, such as those which are seen in one or two of the glens of Scotland, or of any other deposits indicating the former presence of extensive tracts of stagnant water. The hypothesis which ascribes the distribution of the enormous masses of diluvial gravel existing in so many parts of our island to the agency of a series of lakes, which from time to time have burst their barriers and descended to lower levels, may, therefore, at once be rejected as gratuitous.

Diluvian Action proved from the Form of many Valleys of Denudation.

There is another independent reason for rejecting the hypothesis, which may be properly stated in this place. That most of our secondary valleys have been formed by denudation, and that by the action of water many portions of the earth's surface have been greatly changed in form since the solid strata assumed their present elevation is universally admitted; the only question is respecting the manner in which such changes have been brought about. Now we may venture to assert, that in numberless instances the present drainage of the earth's surface could never have been effected, either by the long continued erosion of the elements, or by the bursting of any series of lakes once pent up among its higher regions; and if this statement be true, the present modifications in the external contour of the earth must have been effected by the action of water put in motion by powers which differ altogether with those with which we are acquainted. It is impossible in this place to enter on a detailed proof of the preceding assertion. By way of illustration, I shall only refer to two examples of the kind alluded to, though many others equally decisive of the question at issue, might be derived from various parts of our island.*

Wealds of Kent.

The first example to which I shall refer is supplied by the

* Some excellent observations connected with this subject may be found in the "Geological Survey of the Yorkshire Coast;" by Young and Bird, p. 279, 286. Many valleys appear to have been formed by an actual disruption of the strata produced at the time of their first elevation. Valleys of this kind are of course excepted from the remarks in the text, which apply exclusively to true valleys of denudation, such as those by which the greater part of the secondary strata of England are intersected.

natural drainage of a portion of the counties of Kent and Sussex. A number of small rivers take their rise in the central ridge of the Hastings sands (see Greenough's Geological Map of England), and descend from thence both on the north and south side into the longitudinal valleys occupied by the weald clay. Instead of finding their way to the sea through these valleys, the rivers proceed in a direction nearly transverse to them, and escape on the one side into the Thames, and on the other side into the Channel, by deep gorges cut through the escarpments of the North and South Downs.* In this way the whole region is intersected by a double system of valleys communicating with the sea, and crossing each other nearly at right angles. It is, I think, physically impossible that this singular contour should have been produced by the long continued erosion of the waters. For allowing that the rivers have scoured out the longitudinal valleys of the weald clay, no reason can be given why they should not flow down those valleys at this moment; and on this supposition it is inconceivable how they should ever have forced their way (in no less than eight places) through the high ridges of the North and South Downs. Again, if we suppose that the North and South Downs were once prolonged to the south-east so as to form a continuous ridge, we may shift the difficulty, but we shall not explain it. On this supposition a large inland lake might have occupied the region of the weald clay, and such a lake might have burst the chalk barrier, and formed one or two valleys of denudation. But it is impossible that such an agent should ever have formed the complex system of valleys by which the Downs are now intersected. That all these valleys have been opened out by the same disturbing forces which have produced the accumulations of superficial gravel in the neighbouring parts of England cannot admit of doubt. Yet we have the clearest physical evidence that the drainage could never have been effected by the ordinary operations of any of those disturbing forces which are now acting on the surface of the earth.

Drainage of the Isle of Wight.

The next example is supplied by the drainage of the Isle of Wight.† Two small rivers which rise on the south side of the central Downs might have escaped into the sea by low and direct channels cut through the incoherent ferruginous sands. Instead of this, they flow into the north channel, at Cowes and Brading, through two deep valleys which have been scooped out of the central chalk ridge. It is physically impossible that the rivers should have effected this passage for themselves. And if we suppose these valleys to be closed, it is incompatible with every

* See the "Outlines of the Geology of England and Wales," p. 145.

† See the map accompanying Dr. Fitton's paper published in the *Annals of Philosophy* for Nov. 1824.

thing we know of the structure of the country to conceive the existence of any lake whatsoever, much less of any body of waters capable of bursting through the high chalk downs, and of bringing the island into its present form. We are, therefore, compelled to admit, that the island has been reduced to its present form by some more powerful cause than any which is in ordinary action. A detailed examination of the surface of the country fully establishes this inference. For we have the most direct evidence to prove, that diluvian torrents have swept over every part of the Isle of Wight, its highest as well as its lowest elevations; and that they have scooped out deep valleys, and driven before them enormous masses of gravel, which are heaped upon the *upper freshwater beds* and all the other *tertiary* deposits which extend to the north channel.

Resulting Conclusions.

From a consideration of such facts as these, we may, I think, unequivocally establish the two following conclusions: 1. That during a period of time posterior to the deposition of the newest regular strata which are known to geologists, many parts of our island have been ravaged by powerful denuding forces. 2. That the form and direction of the valleys produced by these denuding forces, cannot be accounted for by any known action of the waters which are now draining off the surface of the country. On similar grounds the preceding conclusions might be extended to many other parts of the world; and they are obviously independent of any arguments drawn from the extent and the position of the *diluvial detritus.*

Position and Extent of the Diluvial Detritus.

In the remaining part of this paper I shall proceed to an examination of the materials which have been torn up by *diluvian* currents, and scattered over different parts of our island; and from the position and extent of these materials, I shall endeavour to prove that they cannot be accounted for by the ordinary operation of any known physical agent. It is not, however, my intention to enter on any general details connected with the history of this *detritus,* as they would inevitably lead me into ground which is already occupied by the author of the "*Reliquiæ Diluvianæ.*" I shall, therefore, only select from the facts which have come under my own observation, two or three which seem to bear more immediately on my present object. On this account I forbear to notice the successive valleys of denudation, and the almost continuous masses of *diluvium* which present themselves on the south coast; and for the same reason I pass over all the corresponding phenomena in the central and southern parts of our island. I may, however, express a conviction, founded on a very extensive range of observations, that there is not a single spot in the abovementioned parts of England which has been exempted from the attacks of those destructive forces which have produced the diluvial gravel. Whatever, therefore, may have been the origin of the phenomena in question, they are due to the operation of no partial or local agents.

Diluvium on the East Coast, &c.

I. The eastern parts of England from the chalk downs of Lincolnshire to those of Cambridgeshire, offer a series of striking facts connected with the history of diluvial phenomena. In the neighbourhood of Cambridge (and I believe also along the whole escarpment of the chalk in the counties of Norfolk and Suffolk), the diluvial deposits may be divided into two distinct classes. The first, composed of coarse materials, often lodged at considerable elevations, and apparently drifted into their present situation by the first rush of the waters: the second, generally found in lower elevations, and apparently comminuted by the continued attrition of the retiring waters. The extensive deposits of transported materials in the low region between Cambridge and Lynn, generally belong to the latter class; and the immense abundance of rolled flints contained in them seem to prove that the neighbouring chalk strata must once have extended considerably to the west of their present limits. An examination of the chalk downs themselves completely demonstrates that the denuding currents have not been confined to the lower part of the escarpments; but have pushed enormous masses of gravel over the very top of the downs, and have modified the whole surface of the district. Lastly, the bluff escarpment presented by the chalk on the coast of Norfolk, and the re-appearance of the same rock in the wolds of Lincolnshire, almost compel us to admit that the formation was once continuous, and that the whole Wash of Lincolnshire has been caused by denudation. Be this as it may, we may conclude with certainty that the present form of the chalk downs of Suffolk, Norfolk, and Cambridgeshire, could never have been produced by any known action of the waters which now drain off that part of England; and the nature and position of transported materials, which the denuding currents have drifted over many parts of the neighbouring region, lead us to exactly the same conclusion.

Diluvium of Huntingdonshire and Cambridgeshire.

The elevated plains, which extend on the confines of Bedfordshire, Cambridgeshire, and Huntingdonshire, exhibit several partial deposits of such transported materials, from which the Rev. J. Plumptre, of Great Gransden, has selected a vast variety of rolled masses derived from almost every known formation in England. His highly interesting collection, obtained from the *diluvium* in the neighbouring district, may be divided into the following classes:—1. Containing many ancient rocks derived from doubtful or unknown localities. 2. Many primitive and transition rocks resembling those existing *in situ* on the western side of our island. Some of these which are much rounded have probably, by an ancient catastrophe, been buried in the conglomerates of the new red sandstone; and afterwards, by the last catastrophe which has desolated the earth's surface, been transported into their present situation. 3. A fine series of specimens of mountain limestone and trap resembling the corresponding rocks of Derbyshire and Staffordshire. 4. An immense number of blocks drifted from the more recent strata. Out of this class one might select a good series of specimens characteristic of all the strata of England from the lias to the chalk.

Extensive deposits of diluvial rubbish similar to those last described occur in two or three places to the east and south-east of Cambridge. From the gravel on the top of the Gogmagog hills, I have found rolled masses of granite and porphyry; pebbles resembling those imbedded in the new red sandstone; masses of trap and mountain limestone; and a fine series of specimens derived from the oolitic formations. Masses of gravel of a nearly identical character lie scattered over several parts of the downs of Suffolk and Norfolk.* We must not imagine that the instances here given are indications of mere local operations. The gravel about Cambridge not only agrees in general character, but almost forms a continuous mass with the beds of gravel which are spread over many parts of the counties of Bedfordshire, Huntingdonshire, and Northamptonshire. And the patches of coarse diluvium which are scattered over the downs, are connected with a series of operations which have buried nearly the whole county of Norfolk and the greater part of the county of Suffolk under enormous masses of *diluvial debris.* Effects such as these are utterly beyond the reach of any known natural agent.

Plains of Cheshire and Derbyshire Hills, &c.

II. I intentionally pass over all details connected with the history of the transported materials in the great plain of the new red sandstone, and shall content myself with stating, that enormous masses of diluvium extend from the base of the great oolitic terrace through many parts of Leicestershire and Staffordshire, and through almost every part of the plains of Cheshire. The diluvial wreck of this region is found at all levels, for it is seen on the upper part of Charnwood forest as well as in the neighbouring vallies; and transported bowlders of considerable magnitude occur at the very top of several parts of the

* For some further details connected with this subject, see Prof. Hailstone's paper in the Geol. Transac. vol. iii. p. 244.

Derbyshire chain which overhangs the great plain of Cheshire. For example, many large smooth bowlders of primitive or transition rocks lie scattered over the surface of the ground on both sides of the high pass leading from Buxton to Macclesfield. These facts speak the same language with those which I have already quoted. They show the generality of the causes which have produced the superficial *detritus;* and they prove that their operations have not been confined to the lower parts of our island.

Western Moors, Central Plains, and East Coast of Yorkshire.

III. In my former paper I briefly noticed the great accumulation of coarse gravel on the plains which skirt the western moors of Yorkshire. Had this gravel been formed by a number of lakes which were once pent up among the mountains, and afterwards burst their way into the lower regions of the district, we might expect to find traces of such lakes in the interior of the moorlands, and distinct heaps of gravel marking the devastations produced by the discharge of the successive lakes into the plain of the new red sandstone. We, however, find no indications of such lakes; and the diluvial rubbish is spread out almost uniformly over the central plain by some cause which appears to have acted simultaneously, and which has left traces of its operation from the southern extremity of Yorkshire to the mouth of the Tees.

Every part of the Yorkshire coast and whole neighbouring region, bears witness to the operation of similar causes. The numberless valleys of denudation in the eastern moorlands—the immense accumulation of transported materials on the hills as well as in the valleys—the whole contour of the vale of Pickering—the enormous cap of diluvium containing rounded masses of primitive rocks many tons weight, and resting on the chalk hills near Flamborough Head—the external form of the Wolds—and the continuous mass of *diluvium* extending from Bridlington to Spurn Head, and from the chalk downs to the sea, are so many monuments of the gigantic powers which were let loose upon the world during the epoch of the diluvial gravel. In the summer of 1821, I had an opportunity of examining all these phenomena in detail; and I can bear unqualified testimony to the faithfulness of the descriptions given of them by the author of the "*Reliquiæ Diluvianæ*."*

The *diluvium* of Holderness is of great interest, partly from its immediate connexion with a series of operations which have affected all the neighbouring districts; partly also from its occupying the whole line of coast, and from its enormous thickness, which enable us to examine with detail all the circumstances

* See the "*Reliquiæ Diluvianæ*," p. 191, 194.

appearing to throw light upon its history. In many places where it occupies a succession of lofty cliffs, it puts on a rude appearance of stratification, or at least may be subdivided into separate masses which possess distinct characters.

The lower part of the cliffs, to the height of about twenty feet, generally consists of a stiff bluish clay, which in many places passes into a dark brown coloured loam.* Through the whole of this mass are imbedded an incredible number of smooth round blocks of granite, gneiss, greenstone, mica slate, &c. &c. resembling none of the rocks of England, but resembling specimens derived from various parts of the great Scandinavian chain. Irregularly mixed with the preceding are found, in perhaps still greater abundance, fragments of carboniferous limestone, of millstone grit, of lias, of oolite, and of chalk, torn up from the regular strata of the country, and driven into their present situation by a great eastern current which has left its traces on every part of the neighbouring district. In regard to the imbedded fragments above-mentioned, two things appear to deserve notice. 1. They exist in equal abundance in the upper as well as in the lower portions of the diluvial loam. This fact, though difficult of explanation, has been remarked in other similar deposits, and seems to prove the gigantic nature of the forces by which the materials have been drifted into their present position. 2. The bowlders derived from distant countries are rounded by attrition; but those which are derived from neighbouring rocks are little altered in form. The hard Norwegian rocks are smooth and spheroidal, but the fragments of oolite and lias, and still more the fragments of chalk, are often sharp and angular.

Over the preceding deposit come a set of beds of sand and comminuted gravel, very variable both in their structure and in their thickness. They seem to have been formed by a longer continued and a less violent action than that which produced the diluvial loam on which they rest.†

Over the sand and gravel we may sometimes find traces of ancient turf-bogs and of other alluvial deposits, formed in situations which were once in the interior of the country; but are brought into their present position by the encroachments of the coast.

* Large grinders of the mammoth have been found in several parts of this deposit.

† Near Bridlington there is a diluvial covering about sixty feet thick where we may observe, 1. The clay and loam with large imbedded fragments; 2. The sand and fine gravel; 3. Over the two preceding, and immediately under the vegetable soil, a bed composed of rolled fragments of chalk and of chalk flints; in some places cemented together so as to form a hard conglomerate. This bed is diluvial, and must be ascribed to the last action of the retiring waters. It may be traced to a considerable height on some parts of the downs when it rests immediately on the chalk; and following the inclination of the ground, it descends towards, and at length covers, the ordinary diluvial deposits abovementioned.

Lastly, over all the preceding we find in many places a considerable thickness of blown sand.

Such are the phenomena exhibited in the cliffs of Holderness. The masses of transported materials on the top of the Wolds, and still more the enormous masses which, on many parts of the coast between Filey Bridge and Redcar, are piled upon the regular strata to the thickness of 150 feet, admit of the same general subdivisions as the *diluvium* of Holderness, and undoubtedly belong to the same epoch. As we advance towards the north, the fragments of chalk begin to disappear; and fragments of magnesian limestone and of other rocks derived from the county of Durham begin to be more abundant.

Conclusions.

The following conclusions may, I think, be fairly deduced from the facts above stated. 1. The *diluvium* of Holderness and of the whole east coast of Yorkshire, is due to a set of causes which have acted over the western moors, and over all the great central plain of the county. 2. The diluvial currents which produced the gravel of Holderness were probably contemporaneous with other more powerful currents which drove large masses of primitive rocks from Scandinavia to the plains of Yorkshire. And it seems probable that the same currents were contemporaneous with those mighty propelling forces which have driven innumerable fragments of the Scandinavian rocks over the great plains of Russia, Poland, and Germany.

Diluvium at the Base of the Cumberland Mountains, &c. &c.

IV. During the three last years, I have examined every part of the great cluster of mountains which is bounded by the valleys of the Lune and the Eden, and by the western coast from Moricambe Bay to Solway Firth. On its eastern side, this region is united with the great central chain of England; but on all other sides through three-fourths of its circumference, it is skirted by a succession of plains, or lands of low elevation, which are almost entirely buried under accumulations of diluvial matter. From the foot of Stainmoor to Solway Firth, through the whole plain of the new red sandstone, the incoherent materials under the vegetable soil are spread over the greater part of the surface, and are often of such an enormous thickness as entirely to conceal all the subjacent strata. These accumulations are not partial or irregular; but seem to have been rolled out over the surface of the country by an inundation which acted at one moment over the whole district; and like all similar deposits, they contain an incredible number of large bowlders, principally derived from the neighbouring mountains.

On approaching that part of the plain which borders on the northern extremity of the hilly region, we meet with pebbles and bowlders which have been drifted across the Firth from the rocks of Dumfriesshire; and in the *diluvium* still further to the south-west, near the termination of the new red sandstone at Maryport, the imbedded fragments of the transition rocks of Cumberland become rare in comparison with the bowlders derived from the opposite coast of Scotland. In the diluvial rubbish capping a hill near Hayton Castle, about four miles north-east of Maryport, I found some large granitic bowlders resembling the rocks of the Criffel. Among them was one spheroidal mass, the greatest diameter of which was ten feet and a half long, and the part which appeared above the ground was more than four feet high.

From Maryport to St. Bees Head, the cliffs are occupied by a succession of coal strata; and the diluvial phenomena, though of constant occurrence, present nothing worth remarking in this place.

West Coast of Cumberland.

From St. Bees Head to the southern extremity of Cumberland, the region bordering on the coast is formed of one almost continuous mass of *diluvium,* interrupted here and there by low hills of blown sand, and by other recent formations. In this part of the county, the cliffs are of a deep red colour, caused by the presence of innumerable imbedded fragments of the subjacent new red sandstone. With these fragments, bowlders of granite, porphyry, and greenstone, are scattered through the whole diluvial covering; sometimes in such abundance as to give it the appearance of a true conglomerate; especially in places where, by the infiltration of a new cementing principle, the whole mass has begun to assume a coherent form.*

Some of the granite blocks imbedded in the cliffs are of great magnitude. In the diluvial cliffs near Bootle, I found one of a rude prismatic form which was twelve feet long, six feet wide, and five feet and a half high. All specimens of this kind of rock have been drifted to the coast from the granitic region which extends from Wastdale foot, through Muncaster fen to the neighbourhood of Bootle; and occupies a part of Wastdale Head, and all the lower parts of the valleys of the Mite and the Esk.

Diluvium of Low Furness.

If we cross the estuary of the Duddon to the shores of Low Furness, we find an exact repetition of the phenomena we have

* When these diluvial conglomerates are not seen *in situ*, they may be separated from the older conglomerates by the freshness of their imbedded pebbles. Fragments imbedded in the older conglomerates are generally in a state of decomposition, which appears frequently to originate in a similar cause to that which so often produces decomposition in crystals after they have become coated over by a deposition of newer crystalline matter.

left behind. All the country bordering on the western shore is covered by an enormously thick deposit of red coloured diluvial gravel containing innumerable rolled fragments of rocks derived from every part of the lake mountains; and all the neighbouring islands are composed exclusively of the same materials. A rolled mass of Eskdale granite, which had been imbedded in the highest portion of the diluvial cliff near Rampside, fell down upon the strand in the year 1822. It rose nine or ten feet above the rubbish in which it was at that time partially buried. At the base of the cliffs of the isles of Barrow and Foudrey, among innumerable bowlders of granite, and of other Cumberland rocks, were some specimens of a beautiful variety of compact felspar which I afterwards found *in situ* near the top of Sca-fell and Bow-fell.*

In places where the inferior strata are so completely concealed it is impossible to ascertain the whole thickness of the diluvial covering. In many parts of Low Furness, it must, I think, be considerably more than 100 feet. Near Newbiggin, where they were searching for coal in the year 1822, they passed through 60 feet of diluvial loam before they reached any rock *in situ*.

The phenomena above described have obviously been caused by a violent rush of descending waters. Whatever forces may have put these waters in motion, it is, I think, obvious, from the facts already stated, that they have not acted partially, but have swept over the whole cluster of the neighbouring mountains.

Diluvial Deposits in the Mid Region of the Mountains, &c.

V. If the accumulations of diluvial gravel, such as have been last described, were produced by descending currents which brought fragments of rock down from the very crests of the neighbouring mountains; we may expect to find some traces of such currents in the mid regions of the district between the highest elevations and the surrounding plains. In such situations, for obvious reasons, we must not look for those accumulations of diluvial loam which are extended over the lower country. The transported materials will only find a partial lodgment, or will appear in the form of scattered bowlders which the propel-

* The direction in which the diluvian currents have swept over the western coast of Cumberland, is plainly indicated by the immense accumulations of bowlders of Eskdale granite in Low Furness, and in the whole cluster of the neighbouring islands, and would lead us to expect the appearance of rolled masses of the same variety of rock on the plains of Lancashire. Prof. Buckland states (Reliquiæ Diluvianæ, p. 199), that they have been drifted in great numbers over the plains of Lancashire, Cheshire, and Staffordshire. In a description given by Dr. Hibbert, in the Edinburgh Journal of Science for last April, of an interesting diluvial deposit containing granite bowlders which occurs near Manchester, it is conjectured that the transported blocks are derived from the granite of Dufton near Appleby. Had the author been acquainted with the facts detailed above, he would probably have referred the bowlders in question to the Eskdale granite.

ling currents have left behind. Such is the case in the mid region of the lake mountains, where innumerable scattered bowlders give the clearest indications of the force and of the direction of the torrents which have swept over it. Any thing like a regular history of such phenomena would lead me into endless details. One or two facts bearing upon the subject will be enough for my present purpose.

1. On the granitic hills which extend from Bootle into Eskdale are many large bowlders derived from various parts of the green slate formation. Among the rest are some specimens of a striped hornstone, identical with the rocks immediately under the crest of Sca-fell, the highest mountain in Cumberland. These blocks are at present separated from the parent rock by the deep valley of the Esk.

Carrock Bowlders.

2. Millions of large bowlders lie scattered over the hills which form the north-west boundary of the mountainous region; but they are seldom sufficiently characteristic to enable us to determine the exact spot from which they have descended. The syenitic blocks of Carrock-fell, principally composed of hypersthene and compact felspar, may, however, be traced from the diluvial loam and gravel of the plains, through the valleys and over the hills of the mid region, to the very foot of the parent rock. On the side of High Pike (near the path leading from Nether-row to the lead mines) are innumerable bowlders of the Carrock syenite. The largest (which is known in the country by the name of golden rock) is 21 feet long, more than ten feet high, and about nine feet wide. The back of Carrock, where the same kind of rock exists *in situ*, is about two miles distant from the great bowlder, and is at present separated from it by a deep valley.

3. Rolled masses of the porphyry of St. John's vale almost cover the ground near Penruddock, and from thence follow the course of the valleys of denudation which descend into the Eamont. Blocks derived from a dyke of beautiful red porphyry which traverses a part of the ridge to the west of Thirlmere, are found scattered about on the lower part of the hills near Keswick.

Shap Granite.

4. Spherical bowlders of shap granite occur in great abundance on the calcareous hills south of Appleby. Among them I found one or two which were about twelve feet in diameter. On the south side of the calcareous zone, the granite blocks are incomparably more abundant; and on approaching Wastdale Head (a few miles south of Shap), where the granite is *in situ*, they literally cover the ground. Near Shap Wells there is a rolled mass of granite fifteen feet long, ten feet wide, and eight feet high.

Bowlders on Kendal Fells, &c.

5. Equally striking examples may be found on the south side of the mountainous region. On the flat tops of the calcareous hills on the west side of Kendal are many rounded blocks, apparently drifted from the green slate formation at the head of Kentmeer and Long Sleddale. These calcareous hills are now separated by deep valleys from every part of the slate formation. Similar phenomena appear on several parts of the mountains between Kendal and Sedbergh, and among the rolled masses are a few bowlders of shap granite. The instances now given are sufficient for my present purpose; for they completely bear out the observations by which they were preceded.

Proofs of Diluvian Action at the Tops of the Mountains.

VI. It is stated by Buckland (*Reliquiæ Diluvianæ*, p. 221), "that all mountain regions he has ever visited bear, in the form of their component hills, the same evidence of being modified by the force of water, as do the hills of the lower regions of the earth." My own observations, as far as they go, confirm the truth of this remark. Some of the highest mountains of Cumberland and Westmorland, which consist of a soft decomposing slate, are as plainly modified by the action of denuding currents as any of the secondary ridges of our island. We must, however, remember that the earth's surface has been ravaged by the action of water during several distinct catastrophes, and that the present modifications in the form of some of our mountain chains *may*, therefore, have been effected during some epoch long antecedent to that of the diluvial gravel. To prove that the floods which produced the superficial gravel have swept over the tops of the highest mountains, requires, therefore, more direct evidence than that which is afforded by the external forms of the mountains themselves.* I think it has already been proved that diluvian torrents have swept over every part of the Cumberland chain; because we find water-worn masses, derived from the highest elevations of the country, imbedded in the diluvial loam which covers almost all the neighbouring plains; and because we find large bowlders of the same rocks scattered over many parts of the mid region of the mountains, in situations to which they could never have been drifted by any less powerful agent than that to which they have been ascribed. I may also observe that the bowlders in question, at whatever elevation, are all in the same state of preservation, and all appear, as far as we can judge from their external characters, to have been produced at the same epoch.

Admitting the fact that the waters of a great inundation have

* For the direct evidence offered on this subject by Prof. Buckland, see the "*Reliquiæ Diluvianæ*," p. 221—223.

swept over some of the highest elevations of the earth, it is still obvious that true diluvial deposits must necessarily be of rare occurrence near the crests of mountain chains. On this account I thought myself fortunate in being able to discover two or three examples of such deposits among the mountains of Cumberland.

Sca-fell.

1. In the deep water-worn channels which descend from Scafell towards Burnmoor Tarn, are great accumulations of *detritus*, which, when I visited the spot in 1822, I considered to be undoubtedly diluvial. These accumulations are apparently connected with the transported blocks which are scattered over the ground between Barnmoor Tarn and Wastdale Head, and exactly resemble the *detritus* which still further down is accumulated in the valley of the Mite.

Ridge, near Red Pike.

2. On the very top of the lofty ridge which separates the valleys of Ennerdale and Buttermere, are most striking and unequivocal proofs of the action of diluvian torrents. Between Red Pike and Ennerdale Scaw, the top of the ridge is partly composed of syenite, and partly of a soft variety of clay slate. A smooth round-topped hill (called Starling Dod), composed of the soft slate, forms the highest part of the crest between the two summits before-mentioned. I was persuaded, before I ascended this hill, that its singular form must have been produced by the action of water; and on reaching its summit, which is about 2500 feet above the level of the sea, I found it covered with water-worn bowlders of red syenite and other rocks drifted from the more lofty eminences of the same ridge near Red Pike. The same kind of bowlders are found near the top of Mellbreak, a mountain which overhangs the west side of Crummock lake; and they may be traced on the sides of the water-worn hills, and down the valleys which communicate with Loweswater and Crummock foot; and from thence the descending currents have drifted them into the lower regions of the district where they are mixed with the diluvium of the plains.

Borrowdale Fells, &c.

3. Near the top of Glaramara, one of the mountain crests of Borrowdale, and at the back of the Hay Stacks, near the top of the ridge between Ennerdale Head and Buttermere, I saw several bowlders which had been caught among the serrated edges of those rugged elevations. The transported blocks were not of a kind to enable one to point out the spot from which they had been drifted; but their presence was enough to demonstrate the former action of violent disturbing forces which had affected the highest points of the mountain region.

To account for such phenomena as those above described, by the bursting of lakes, of the existence of which we have no proof; and which, had they ever existed, could only have existed at much lower levels, would be to adopt an hypothesis contradicted by the very facts which it is intended to explain. The condition of the transported blocks, their association with others which have descended into the mid region, and their identity with many other masses which are imbedded in the diluvium of the plains, forbid us to ascribe their appearance to any of the more ancient catastrophes in the physical history of the earth. The conclusion then to be drawn from them is obvious, and is in accordance with the other facts which have been stated in this paper.

Directions in which the Shap Granite has been drifted.

VII. The great uniformity in the mineralogical character of the rocks in many parts of Cumberland, often prevents us from ascertaining the direction in which the diluvial bowlders have been drifted from their native beds. This difficulty we do not meet with in following the blocks of Shap granite, as they cannot be confounded with any other rocks in the north of England. It has already been stated that they almost cover the ground in many places near Shap; and that they have been lifted over the escarpment of the carboniferous limestone, and drifted over the hills near Appleby. I may now add, that they have been scattered far and wide over the plain of the new red sandstone—that they have rolled over the great central chain of England into the plains of Yorkshire—that they are imbedded in the diluvium on both banks of the Tees—and that a few straggling blocks have, if I mistake not, found their way to the eastern coast.

The passage of the same kind of granitic blocks into the valley of the Kent is, if possible, still more difficult to explain by the operations of any known agent. For the granite only exists *in situ* on the very outskirts of the mountain group, and almost abuts against the calcareous zone near Shap wells. Yet a set of gorges have been opened out of the higher and more central parts of the group, through which the granite bowlders have been driven (in a direction exactly opposite to that in which they have been already traced), and from which they have not only descended in great abundance into the valley of the Kent, but have also been drifted into a part of the ridge between the Kent and the Lune. With these remarks on the extraordinary directions in which masses of Shap granite have been drifted from their native bed, I terminate my observations on the position and extent of the masses of incoherent *detritus* which lie scattered over many parts of our island.

Concluding Remarks.

As the general result of the facts detailed in this and the preceding paper, we may conclude—that the floods which produced the diluvial detritus swept over every part of England—that they were put in motion by no powers of nature with which we are acquainted—and that they took place during an epoch which was posterior to the deposition of all the regular strata of the earth, and prior to all known accumulations of alluvial matter.

We have evidence enough to justify us in extending the same conclusions to every part of the European basin, and there is some evidence which makes it probable that they may be extended to the remotest parts of the earth's surface. Indeed the mighty disturbing forces which produced the accumulations of diluvial *detritus* between the western extremities of Europe and the central plains of Asia, must probably have acted with sufficient energy to leave some traces of their power over every quarter of the globe. On the continent of America the succession of formations seems to be very nearly the same with that of our own country; and over all the regular strata, there occur in many places alluvial and diluvial formations in every respect like those of Europe. It is, therefore, to say the least of it, probable, that the diluvial phenomena of Europe and America belong to the same epoch.

The actual duration of the diluvian era, it is of course impossible to ascertain; for as the powers of the agent are unknown, it is obviously impossible for us to form an estimate of the time which was necessary to the production of such effects as are visible on the earth's surface. The facts which have been detailed seem, however, to make it probable that the floods which produced the diluvial gravel were sudden and transient.

In the present state of our information, we have certainly no evidence to prove that all the highest elevations of the globe were submerged by the diluvian waters; for the form of the great mountain chains may have been produced by some more ancient catastrophe, and we have no right to assume the existence of diluvial *detritus* in parts of the world which have not been examined, or which are inaccessible. We have, however, direct evidence to prove, that the diluvian floods acted on some of the highest points of Europe, and it is probable also that they have acted on some of the highest parts of Asia.

As we are unacquainted with the forces which put the diluvian waters in motion, we are also, with very limited exceptions, unable to determine the direction in which the currents have moved over the earth's surface. Many parts of the north of Europe seem to have been swept over by a great current which set in from the north. In some parts of Scotland there has been a great rush of water from the north-west.* The details given above, show that the currents which have swept over different parts of England have not been confined to any given direction. It may, perhaps, be laid down as a general rule, that the diluvial gravel has been drifted down all the great inclined planes which the earth's surface presented to the retiring waters.

That the details given in the preceding papers tend, as far as they go, to confirm the general argument of Buckland's "*Reliquiæ Diluvianæ*" cannot admit of doubt. Indeed, the facts brought to light by the combined labours of the modern school of geologists, seem, as far as I comprehend them, completely to demonstrate the reality of a great diluvian catastrophe during a comparatively recent period in the natural history of the earth. In the preceding speculations, I have carefully abstained from any allusion to the sacred records of the history of mankind; and I deny that Professor Buckland, or any other practical geologist of our time has *rashly attempted* to unite the speculations of his favourite science with the truths of revelation.†

The authority of the sacred records has been established by a great mass of evidence at once conclusive and appropriate; but differing altogether in kind from the evidence of observation and experiment, by which alone physical truth can ever be established. It must, therefore, at once be rash and unphilosophical to look to the language of revelation for any direct proof of the truths of physical science. But truth must at all times be consistent with itself. The conclusions established on the authority of the sacred records may, therefore, consistently with the soundest philosophy, be compared with the conclusions established on the evidence of observation and experiment; and such conclusions, if fairly deduced, must necessarily be in accordance with each other. This principle has been acted on by Cuvier, and appears to be recognized in every part of the "*Reliquiæ Diluvianæ.*" The application is obvious. The sacred records tell us—that a few thousand years ago "the fountains of the great deep were broken up"—and that the earth's surface was submerged by the waters of a general deluge; and the investigations of geology tend to prove that the accumulations of alluvial matter have not been going on many housand years; and that they were preceded by a great catastrophe which has left traces of its operation in the *diluvial detritus* which is spread out over all the strata of the earth.

* This is proved in an original and excellent paper, published by Sir James Hall, in the Transactions of the Royal Society of Edinburgh, vol. vii. See also the "*Reliquiæ Diluvianæ,*" p. 201—205.

† See the Edinburgh Philosophical Journal, No. 22, p. 304.

Between these conclusions, derived from sources entirely independent of each other, there is, therefore, a general coincidence which it is impossible to overlook, and the importance of which it would be most unreasonable to deny. The coincidence has not been assumed hypothetically, but has been proved legitimately, by an immense number of direct observations conducted with indefatigable labour, and all tending to the establishment of the same general truth.

APPENDIX.

[The following account of the drainage of a part of the fen lands bordering on the Wash of Lincolnshire, is principally abridged from Dugdale on "The History of Imbanking and Drayninge," chap. 54; and from "Badeslade on the Navigation of King's-Lynn, and of Cambridge." It was intended to appear in the form of a note to the fifth section of a paper in the *Annals of Philosophy* for April last; but it was not transmitted to the Editors in time for the press.]

A short account of the drainage of a part of the fens, bordering on the Wash, during a period within the reach of authentic records, will explain and confirm the assertion in the text.* In the early parts of that period, the drainage was effected in the following manner: 1. By the channel of the Witham, which had nearly the same course which it has at the present time. 2. By the Welland, which, after descending by Stamford, Crowland, and Spalding, united with the waters of the Glen in the estuary, north of Holland-fen. 3. By the Nene, which, after passing Wansford and Peterborough, descended by Whittlesea-meer, Ugg-meer, and Ramsey-meer to Benwick, where it was joined by the Old West-water, one of the branches of the Great Ouse; from Benwick it flowed on the north side of March and Doddington (which stand, if I mistake not, on low diluvial hills) to Upwell, where it was joined by the Welney river, then the principal branch of the Great Ouse; and from Upwell the united waters proceeded directly to Wisbeach, anciently called Ousebeach. 4. By the Great Ouse, which, after passing Huntingdon and St. Ives, descended to Erith (a small village at the SW. end of the old and new Bedford rivers) when it divided into two branches. One called the Old West-water ran to Benwick, as before stated, and there united with the Nene. The other branch, now called the Old Ouse (sometimes erroneously marked as the Old West-water), descended by Cottenham fen, and was joined by the Cam a few miles above Ely. After passing Ely, it was joined by the Mildenhall river; and it then passed, by the way of Littleport and

* See *Annals* for April, Editor's note, sect. 5.

Welney, to Upwell; where (as above stated) it joined the waters of the Nene and descended to the sea at Wisbeach. 5. By the Little Ouse (then a very inconsiderable river), which (after passing Brandon, and being joined by some small tributary streams from the Norfolk side) fell into the sea a. Lynn. In the preceding account, all the old artificial drains, and several minute bifurcations of the rivers, after they reached the alluvial delta, are intentionally omitted.

As early as the twelfth century, the accumulations of alluvial silt near the mouths of the Welland and the Nene, caused a great back-water; and in the early part of the thirteenth century (by the great rise of the fen lands near the coast) the out-fall of the waters by some of the old channels entirely failed. During this time, the bed of the Little Ouse, not having been silted up in the same manner, was much below the mean level of the alluvial delta, extending through the mouths of the other rivers above mentioned; and a great drain was consequently cut from Littleport Chair to Rebeck, making the first direct communication between the Great and Little Ouse. The effect was exactly what might have been anticipated. The waters which had been pent up at a higher level descended with irresistible force through this new drain into the channel of the Little Ouse, and so escaped into the sea at Lynn. About this time the out-fall at Spalding had so completely failed, that the waters of the Welland found their way through the Catswater into the Nene; and a new direction having been given to all the currents, in consequence of the channel which was now opened below the level of the ancient out-fall at Wisbeach, the united waters of the Nene flowed back into the Great Ouse through the Old West-water, through the Welney branch, and through all the other cross drains of the country; and were then conveyed by the new communication into the Lynn river. In this way, for many years, nearly all the waters of the alluvial delta, south of the Witham, found their way into the sea at Lynn: and the river, which had formerly run between banks which were not more than twelve perches asunder, was, after the changes above described, more than a mile wide.

Many attempts were made to prevent this great discharge of waters through the Ouse. In the year 1292, several dams were constructed near Upwell, to prevent the influx of the Nene. But they produced such ruinous effects on many parts of the marsh lands, and on the banks of the Ouse as far as St. Neots, that in 1332 they were ordered to be destroyed. For many years afterwards, the great drainage of the delta was effected nearly in the manner above described.

In the year 1490, the discharge by the Ouse was partially relieved by a great cut (called Morton's *Leam*) from Peterborough to Guyhirn near Wisbeach. This was intended to convey the waters of the Nene direct to their old channel at Wisbeach, but was never entirely effective before the year 1638: when Vermuyden, under King Charles I. erected high banks on each side of the *Leam*, and opened out a channel to the sea. Since that time the Nene has continued to flow down to the sea by Wisbeach.

Notwithstanding the indirect nature of the new drainage which conveyed the waters of the Welland, the Nene, and the Ouse, into the sea by the Lynn channel, the fens appear for many years afterwards to have been in a good condition; a fact which can only be explained by the low level of the great outfall. In course of time, however, the new channels began to silt up, and new works became necessary. Of these works, the old and new Bedford rivers were the most important, extending from Erith to Salters Lode, a distance of about twenty miles. Soon after the year 1648, when the new Bedford river was completed, the waters of the Ouse were shut out by a sluice at Erith from their old channel, so that they did not mix with the waters of the Cam and its tributary branches, till they had been conducted by the new drainage to Salters Lode. These new works appear from the first to have been injurious to the natural drainage of the Cam; for the floods of the Ouse by the new passage reached Salters Lode much sooner than the floods of the Cam; moreover, the bottom of the new Bedford river was about eight feet above the bottom of the old Ouse. On both these accounts, the banks of the Cam were perpetually flooded by the back-waters of the Ouse. One great flood of the Ouse in 1720, is said to have backed up the Cam for twenty days, and to have silted up a part of the old channel below Ely, to the thickness of three or four feet. These ruinous effects have been partly counteracted by the erection of different sluices; which, although affording a cure for an immediate evil, have ultimately produced the very evil they were intended to remedy; for, partly by their agency, the whole bed of the Cam is now silted up to the level of the Bedford rivers.

If such extraordinary effects as those described in this note be produced by the accumulation of alluvial matter in course of a few hundred years, we may be well assured that the whole form of the neighbouring coast must have been greatly modified by the same causes acting without interruption, and without any modification from works of art, for 3000 or 4000 years.

Article V.

Corrections in Right Ascension of 37 Stars of the Greenwich Catalogue. By James South, FRS.

Mean AR 1825.	γ Pegasi h. m. s. 0 4 14·25	Polaris h. m. s. 0 58 17·50	α Arietis h. m. s. 1 57 19·77	α Ceti h. m. s. 2 53 8·58	Aldebaran h. m. s. 4 25 53·44	Capella h. m. s. 5 3 46·61	Rigel h. m. s. 5 6 8·00	β Tauri h. m. s. 5 15 14·29	α Orionis h. m. s. 5 45 42·18
July 1	+ 2·84″	+ 11·49″	+ 2·41	+ 1·93″	+ 1·79″	+ 2·06″	+ 1·23″	+ 1·83″	+ 1·47″
2	87	12·29	44	96	81	09	25	85	49
3	90	13·09	47	99	84	12	27	87	51
4	94	13·88	51	2·01	87	14	29	90	52
5	97	14·69	54	04	89	17	31	92	54
6	3·00	15·49	57	07	91	20	33	94	56
7	03	16·30	60	10	94	23	35	97	57
8	07	17·11	64	13	96	25	37	99	59
9	10	17·91	67	16	98	28	39	2·01	61
10	13	18·70	70	19	2·01	31	41	04	63
11	16	19·49	74	22	03	34	43	06	65
12	19	20·28	77	25	06	38	46	09	67
13	22	21·07	80	28	09	41	48	11	69
14	24	21·86	83	31	12	44	50	14	71
15	27	22·67	87	34	14	47	52	17	73
16	30	23·47	90	36	17	50	54	19	75
17	33	24·28	93	39	20	54	57	22	77
18	36	25·09	97	42	22	57	59	24	79
19	39	25·89	3·00	45	25	60	61	27	81
20	42	26·66	03	48	28	64	63	30	83
21	45	27·43	06	51	31	67	66	32	86
22	47	28·21	10	54	34	71	68	35	88
23	50	28·98	13	57	37	74	71	38	90
24	53	29·75	16	60	40	78	73	41	92
25	56	30·52	19	64	42	81	75	44	95
26	59	31·28	22	67	45	85	78	46	97
27	61	32·05	26	70	48	88	80	49	2·00
28	64	32·81	29	73	51	92	83	52	02
29	67	33·58	32	76	54	95	85	55	04
30	69	34·31	35	79	57	99	88	58	06
31	72	35·04	38	82	60	3·02	90	61	09
Aug. 1	74	35·77	42	85	63	06	93	64	11
2	77	36·50	45	88	66	10	96	67	14
3	79	37·23	48	91	68	14	98	70	16
4	82	37·95	51	94	71	17	01	72	19
5	84	38·68	54	96	74	21	04	75	21
6	87	39·40	58	99	77	24	06	78	23
7	89	40·13	61	3·02	80	28	09	81	26
8	92	40·85	64	05	83	32	11	84	28
9	94	41·53	67	08	86	36	14	87	31
10	97	42·20	70	11	89	40	16	90	33
11	99	42·88	73	14	92	44	19	94	36
12	4·01	43·56	76	17	95	48	21	97	39
13	03	44·23	79	19	99	53	24	3·00	42
14	05	44·88	82	22	3·02	57	27	03	44
15	07	45·53	85	25	05	61	29	06	47
16	09	46·18	88	28	08	65	32	10	49
17	11	46·83	91	31	11	69	35	13	52
18	13	47·48	94	34	14	73	38	16	55
19	15	48·08	97	37	17	77	41	19	58
20	17	48·68	4·00	39	20	81	44	23	60
21	18	49·27	02	42	23	85	46	26	63
22	20	49·87	05	45	26	89	49	29	66
23	22	50·47	08	48	29	93	52	32	69
24	24	51·05	11	50	32	97	54	36	71
25	25	51·63	14	53	35	4·01	57	39	74
26	27	52·22	17	55	38	05	60	42	77
27	29	52·80	20	58	41	09	62	46	79
28	31	53·38	22	61	44	14	66	49	82
29	32	53·89	25	64	47	18	69	52	85
30	34	54·40	27	66	50	23	72	56	88
31	35	54·92	30	69	53	27	75	59	91

Mean AR 1825.	Sirius h. m. s. 6 37 26·11	Castor h. m. s. 7 23 25·30	Procyon h. m. s. 7 30 8·47	Pollux h. m. s. 7 34 35·85	α Hydræ h. m. s. 9 18 59·40	Regulus h. m. s. 9 59 2·78	β Leonis h. m. s. 11 40 7·80	β Virginis h. m. s. 11 41 34·98	Spica Virg h. m. s. 13 15 59·52
July 1	+ 0·90″	+ 1·73″	+ 1·40″	+ 1·69″	+ 1·53″	+ 1·89″	+ 2·28″	+ 2·36″	+ 2· 95
2	91	74	41	70	53	89	27	35	94
3	92	75	42	71	53	88	26	34	93
4	93	76	42	71	52	88	26	34	92
5	94	77	43	72	52	88	25	33	91
6	96	78	44	73	52	87	24	32	90
7	97	78	45	74	52	87	23	31	89
8	98	79	45	74	52	87	22	31	88
9	1·00	80	46	75	51	86	21	30	87
10	01	81	47	76	51	86	20	29	86
11	03	83	48	77	51	86	19	28	85
12	04	84	50	78	51	85	18	28	84
13	05	86	51	79	51	85	18	27	83
14	06	87	52	80	51	85	17	26	82
15	08	89	53	82	51	84	16	25	81
16	09	90	54	83	51	84	15	24	80
17	10	91	55	84	51	84	14	24	79
18	12	93	57	85	51	83	13	23	78
19	13	94	58	86	51	83	12	22	77
20	15	96	59	88	52	83	11	21	76
21	17	97	60	89	52	83	10	20	75
22	18	99	62	91	52	83	09	20	74
23	20	2·01	63	92	52	83	08	19	73
24	22	03	64	94	53	83	07	18	72
25	24	04	65	96	53	83	07	17	70
26	26	06	66	97	53	83	06	16	69
27	27	08	68	99	54	83	05	16	68
28	29	09	69	2·00	54	83	04	15	67
29	31	11	70	02	54	83	03	14	66
30	33	13	72	04	55	84	02	14	65
31	35	15	73	06	55	84	02	13	64
Aug. 1	37	17	75	07	56	84	01	13	62
2	39	19	77	09	56	84	00	12	61
3	41	21	79	11	57	85	1·99	12	60
4	43	23	80	13	57	85	99	11	59
5	45	25	82	15	58	85	98	11	58
6	47	27	84	16	58	85	97	10	56
7	49	29	85	18	59	86	96	10	55
8	51	31	87	20	60	86	96	09	54
9	53	33	89	22	61	86	96	09	53
10	55	36	91	24	62	87	95	08	52
11	58	38	93	27	63	87	95	08	51
12	60	41	95	29	64	87	95	07	50
13	62	43	97	31	64	88	94	07	49
14	64	45	99	33	65	88	94	07	48
15	66	48	2·01	35	66	89	93	06	47
16	69	50	03	38	67	89	93	06	46
17	71	53	05	40	68	90	93	06	45
18	73	55	07	42	69	90	92	05	44
19	75	58	09	44	70	91	92	05	43
20	78	60	11	47	71	92	92	05	42
21	80	63	13	49	73	93	92	05	41
22	82	65	15	52	74	94	92	05	40
23	84	68	17	54	75	95	92	05	39
24	87	71	19	56	76	96	91	05	38
25	89	74	21	59	77	97	91	04	38
26	91	76	23	61	78	98	91	04	37
27	94	79	25	64	80	99	91	04	36
28	96	81	27	66	81	2·00	91	04	35
29	99	84	29	69	82	01	91	04	34
30	2·02	87	32	71	84	02	91	04	34
31	04	90	34	74	85	04	91	04	33

Mean AR 1825.	Arcturus h. m. s. 14 7 41·06	2α Libræ h. m. s. 14 41 12·92	α Cor.Bor. h. m. s. 15 27 16·97	α Serpent. h. m. s. 15 35 39·42	Antares h. m. s. 16 18 41·57	α Herculis h. m. s. 17 6 40·44	α Ophiuchi h. m. s. 17 26 49·02	α Lyræ h. m. s. 18 31 1·01	γ Aquilæ h. m. s. 19 37 56·55
July 1	+ 2·90″	+ 3·57″	+ 3·16″	+ 3·46″	+ 4·39″	+ 3·61″	+ 3·68″	+ 3·45″	+ 3·76″
2	89	56	15	46	39	61	68	45	77
3	88	56	15	45	39	61	69	46	78
4	87	55	14	45	38	61	69	46	80
5	86	54	14	44	38	61	69	47	81
6	85	54	13	43	38	61	69	47	82
7	84	53	12	43	38	61	69	48	84
8	83	52	11	42	37	61	70	48	85
9	81	51	10	42	37	61	70	49	86
10	80	50	09	41	37	61	70	49	87
11	78	49	07	41	36	60	70	49	88
12	77	48	06	40	36	60	69	49	89
13	76	47	05	39	35	60	69	49	90
14	75	46	04	38	35	60	69	49	91
15	73	45	03	37	34	59	69	48	92
16	72	44	01	37	34	59	68	48	93
17	71	43	00	36	33	59	68	48	94
18	70	42	2·99	35	33	58	68	48	95
19	68	41	97	34	32	58	67	48	96
20	67	40	96	33	31	57	67	47	96
21	65	39	94	32	30	56	66	47	97
22	64	38	93	30	30	56	66	46	97
23	63	37	91	29	29	55	65	46	98
24	61	36	90	28	28	54	65	45	98
25	60	35	88	27	27	53	64	45	99
26	58	34	87	26	26	52	64	44	99
27	57	33	85	24	25	51	63	44	4·00
28	55	32	84	23	24	50	63	43	00
29	54	30	82	22	23	49	62	42	01
30	53	29	80	21	22	48	61	41	01
31	51	27	79	20	20	47	60	40	01
Aug. 1	50	26	77	18	19	46	59	39	01
2	48	25	76	17	18	45	58	38	01
3	47	24	74	16	16	44	57	37	01
4	45	23	73	15	15	43	56	36	02
5	44	21	71	14	13	42	55	35	02
6	42	20	70	12	12	41	54	34	02
7	41	19	68	11	10	40	53	33	02
8	39	17	66	10	09	39	52	32	02
9	38	16	65	09	08	38	51	31	02
10	36	14	63	07	07	36	50	29	01
11	35	13	61	06	05	35	48	28	01
12	33	12	60	04	04	34	47	26	01
13	32	10	58	03	03	32	46	25	01
14	30	09	56	02	02	31	45	24	00
15	29	08	54	00	01	29	44	22	00
16	27	06	52	2·99	3·99	28	42	21	00
17	26	05	50	97	98	26	41	19	3·99
18	24	03	48	96	97	25	40	18	99
19	23	02	46	94	95	23	39	16	98
20	21	00	45	93	93	22	37	14	98
21	20	2·99	43	91	92	20	36	12	97
22	19	98	41	90	90	19	34	10	97
23	17	97	39	88	89	17	33	08	96
24	16	95	37	87	87	16	31	07	96
25	14	94	35	85	85	14	30	05	95
26	13	93	33	84	83	13	28	03	94
27	11	92	31	82	81	11	27	01	94
28	10	90	29	81	79	10	25	2·99	93
29	09	89	27	79	77	08	23	97	92
30	07	88	25	78	76	06	22	95	91
31	06	86	23	76	74	05	20	92	90

Mean AR 1825.	α Aquilæ h. m. s. 19 42 14·84	β Aquilæ h. m. s. 19 46 43·20	2 α Capri. h. m. s. 20 8 20·34	α Cygni h. m. s. 20 35 28·24	α Aquarii h. m. s. 21 56 47·77	Fomalhaut h. m. s. 22 47 57·67	α Pegasi h. m. s. 22 56 3·16	α Androm. h. m. s. 23 59 21·74
July 1	+ 3·79″	+ 3·80″	+ 4·09″	+ 3·45″	+ 3·49″	+ 3·49″	+ 3·16″	+ 2·93″
2	80	81	11	47	52	52	19	96
3	81	83	12	48	54	55	22	3·00
4	83	84	14	50	57	59	25	03
5	84	86	15	52	59	62	28	06
6	86	87	17	54	62	65	31	10
7	87	89	18	56	64	68	35	13
8	89	90	21	58	67	72	37	16
9	90	92	23	60	69	75	39	20
10	91	93	24	61	71	78	42	23
11	92	94	26	63	73	81	44	26
12	93	95	27	64	75	84	47	29
13	94	96	28	65	77	88	49	32
14	95	97	30	67	79	91	52	35
15	96	98	31	68	82	94	54	39
16	98	99	33	70	84	97	57	42
17	99	4·01	34	71	86	4·00	60	45
18	4·00	02	35	73	88	03	62	48
19	01	03	37	74	90	07	65	51
20	02	04	38	75	92	09	67	54
21	02	04	39	76	94	11	69	57
22	03	05	40	77	95	13	72	59
23	03	06	41	78	97	15	74	62
24	04	06	42	79	99	17	76	65
25	04	07	43	79	4·01	19	78	68
26	05	07	44	80	03	21	80	71
27	05	08	45	81	04	23	83	73
28	06	08	45	82	06	25	85	76
29	06	09	46	83	08	27	87	79
30	06	09	46	83	09	29	89	82
31	06	09	47	84	11	31	91	84
Aug. 1	07	10	47	84	12	33	93	87
2	07	10	48	84	14	35	95	90
3	07	10	48	85	15	37	97	93
4	07	10	49	85	17	39	99	95
5	07	10	49	85	18	41	4·01	98
6	08	11	50	86	20	43	03	4·01
7	08	11	50	86	21	45	05	03
8	08	11	51	86	23	47	07	06
9	08	11	51	86	24	49	09	08
10	08	11	51	85	25	51	10	10
11	07	11	51	85	26	52	12	12
12	07	10	51	85	27	54	13	15
13	07	10	51	85	28	55	15	17
14	06	10	52	84	29	57	16	19
15	06	10	52	84	30	59	18	21
16	06	09	52	84	31	60	19	24
17	05	09	52	83	32	62	21	26
18	05	09	52	83	33	63	22	28
19	04	08	52	82	34	64	23	30
20	04	08	51	82	34	65	24	32
21	03	08	51	81	35	67	25	34
22	03	07	51	80	35	68	26	36
23	02	07	50	80	36	69	27	38
24	02	06	50	79	37	70	28	40
25	01	06	49	78	38	71	30	42
26	01	05	49	78	38	73	31	44
27	00	04	48	76	39	74	32	45
28	3·99	03	47	76	39	75	33	47
29	98	03	46	75	39	76	34	48
30	97	02	46	73	39	76	35	50
31	96	01	45	72	39	77	35	51

	γ Pegasi	Polaris	α Arietis	α Ceti	Aldebaran	Capella	Rigel	β Tauri	α Orionis
Mean AR 1825.	h. m. s. 0 4 14·25	h. m. s. 0 58 17·50	h. m. s. 1 57 19·77	h. m. s. 2 53 8·58	h. m. s. 4 25 53·44	h. m. s. 5 3 46·61	h. m. s. 5 6 8·00	h. m. s. 5 15 14·29	h. m. s. 5 45 42·18
Sept. 1	+ 4·36″	+ 55·43″	+ 4·32″	+ 3 72″	+ 3·56″	+ 4·32″	+ 2·78″	+ 3·63″	+ 2·94″
2	38	55·94	35	75	59	36	80	66	96
3	39	56·41	38	77	62	40	83	69	99
4	41	56·87	40	80	65	44	86	73	3·02
5	42	57·34	43	83	68	49	89	76	05
6	44	57·81	45	85	71	53	92	80	08
7	45	58·27	47	88	74	57	95	83	11
8	46	58·67	49	90	77	61	98	86	14
9	47	59·08	51	92	80	65	3·00	90	17
10	48	59·48	54	94	83	69	03	93	19
11	49	59·89	56	96	86	73	06	96	22
12	50	60·29	58	98	89	78	09	99	25
13	51	60·65	60	4·00	92	82	11	4·03	28
14	52	61·02	63	02	95	86	14	06	30
15	53	61·38	65	04	98	90	16	09	33
16	54	61·75	67	06	4·01	94	19	13	36
17	55	62·11	69	08	04	98	22	16	39
18	56	62·40	71	11	07	5·02	25	19	42
19	57	62·69	73	13	10	06	28	23	45
20	57	62·99	75	16	13	10	30	26	48
21	58	63·28	77	18	16	14	33	29	51
22	59	63·57	79	21	19	18	36	32	54
23	60	63·80	82	23	22	23	39	36	57
24	60	64·03	84	26	24	27	41	39	60
25	61	64·27	86	28	27	31	44	42	63
26	61	64·50	88	31	30	35	47	46	66
27	62	64·73	90	34	33	39	50	49	69
28	62	64·90	92	36	36	43	53	52	72
29	63	65·07	94	38	38	47	55	55	75
30	63	65·23	95	40	40	51	58	59	78

	Sirius	Castor	Procyon	Pollux	α Hydræ	Regulus	β Leonis	β Virginis	Spica Virg.
Mean AR 1825.	h. m. s. 6 37 26·11	h. m. s. 7 23 25·30	h. m. s. 7 30 8·47	h. m. s. 7 34 35·85	h. m. s. 9 18 59·40	h. m. s. 9 59 2·78	h. m. s. 11 40 7·80	h. m. s. 11 41 34·98	h. m. s. 13 15 59·22
Sept. 1	+ 2·07″	+ 2·93″	+ 2·36″	+ 2·76″	+ 1·87″	+ 2·05″	+ 1·91″	+ 2·04″	+ 2·32″
2	10	96	39	79	88	06	91	04	31
3	13	98	41	82	90	07	92	05	30
4	16	3·01	44	84	91	08	92	05	30
5	18	04	46	87	93	10	92	05	29
6	21	07	49	89	94	11	92	05	28
7	24	10	51	92	96	12	92	05	28
8	27	13	54	95	98	13	93	06	28
9	29	16	56	98	99	15	93	06	27
10	32	19	59	3·01	2·01	16	93	06	27
11	35	22	61	04	03	18	94	07	26
12	38	26	64	07	05	19	94	07	26
13	40	29	67	10	06	20	94	07	25
14	43	32	70	13	08	22	95	08	25
15	46	35	72	16	09	23	95	08	24
16	48	38	75	19	11	25	96	09	24
17	51	41	77	22	13	26	96	09	23
18	54	44	80	25	15	28	97	10	23
19	57	47	82	28	17	30	97	10	23
20	59	51	85	31	19	31	98	11	23
21	62	54	88	34	21	33	99	12	23
22	65	57	91	37	23	35	2·00	13	23
23	68	60	93	40	26	37	00	13	23
24	71	63	96	43	28	39	01	14	22
25	73	67	99	46	30	40	02	15	22
26	76	70	3·01	49	32	42	02	16	22
27	79	73	04	52	34	44	03	17	22
28	82	76	07	55	36	46	04	18	22
29	85	80	10	59	38	48	05	19	22
30	88	83	12	62	41	50	07	20	22

Mean AR 1825.	Arcturus h. m. s. 14 7 41·06	2 α Libræ h. m. s. 14 41 12·92	α Cor.Bor. h. m. s. 15 27 16·97	α Serpent. h. m. s. 15 35 39·42	Antares h. m. s. 16 18 41·57	α Herculis h. m. s. 17 6 40·44	α Ophiuchi h. m. s. 17 26 49·02	α Lyræ h. m. s. 18 31 1·01	γ Aquilæ h. m. s. 19 37 56·55
Sept. 1	+ 2·04″	+ 2·85″	+ 2·21″	+ 2·75″	+ 3·72″	+ 3·03″	+ 3·18″	+ 2·90″	+ 3·88″
2	03	84	20	73	70	01	16	88	87
3	02	83	18	71	69	2·99	15	86	86
4	01	82	16	70	67	97	13	84	85
5	1·99	80	14	68	65	96	11	81	83
6	98	79	12	67	64	94	09	79	82
7	97	78	10	65	62	92	08	77	81
8	96	77	08	64	60	90	06	75	80
9	95	76	06	62	58	88	04	72	78
10	93	75	05	61	57	87	03	70	77
11	92	74	03	59	55	85	01	68	76
12	91	73	01	58	53	83	2·99	66	74
13	90	71	1·99	56	51	81	97	63	73
14	89	70	97	55	49	79	95	61	71
15	88	69	96	53	48	78	94	59	70
16	87	68	94	52	46	76	92	56	68
17	86	67	92	50	44	74	90	54	67
18	85	66	90	49	43	72	88	52	65
19	84	65	89	47	41	70	86	49	64
20	83	64	87	46	40	69	85	47	62
21	82	63	85	45	38	67	83	44	61
22	81	62	83	44	37	65	81	42	59
23	80	61	82	42	35	63	79	39	58
24	79	60	80	41	34	61	77	37	56
25	78	59	78	40	32	59	76	34	54
26	77	58	77	38	31	57	74	32	53
27	77	58	75	37	29	56	72	29	51
28	76	57	74	36	28	54	70	26	49
29	76	56	72	35	26	53	69	24	48
30	75	56	71	34	25	51	67	21	46

Mean AR 1825.	α Aquilæ h. m. s. 19 42 14·84	β Aquilæ h. m. s. 19 46 43·20	2 α Capricor h. m. s. 20 8 20·34	α Cygni h. m. s. 20 35 28·24	α Aquarii h. m. s. 21 56 47·77	Fomalhaut h. m. s. 22 47 57·67	α Pegasi h. m. s. 22 56 3·16	α Androm. h. m. s. 23 59 21·74
Sept. 1	+ 3·95″	+ 4·00″	+ 4·44″	+ 3·71″	+ 4·39″	+ 4·78″	+ 4·36″	+ 4·53″
2	94	3·99	43	69	40	79	37	54
3	93	98	43	68	40	79	38	56
4	92	97	42	66	40	80	39	57
5	91	96	41	65	40	81	39	59
6	90	95	41	64	40	82	40	60
7	89	94	40	62	40	82	41	62
8	88	93	39	61	40	82	41	63
9	86	91	38	59	40	83	42	64
10	85	90	37	58	39	83	42	65
11	83	89	36	56	39	83	42	66
12	82	87	35	55	39	83	43	67
13	81	86	34	53	39	84	43	68
14	79	84	33	52	39	84	43	69
15	78	83	32	50	38	84	43	70
16	76	82	31	48	38	85	44	71
17	75	80	30	46	38	85	44	72
18	73	79	29	44	38	85	44	73
19	72	77	27	42	37	85	44	74
20	70	76	26	40	37	84	44	74
21	69	74	25	38	37	84	44	75
22	67	73	23	36	36	84	44	76
23	66	71	22	34	36	83	44	76
24	64	70	20	32	35	83	43	77
25	63	68	19	30	35	83	43	77
26	61	67	17	27	34	82	43	78
27	60	65	16	25	33	82	43	78
28	58	64	15	23	32	82	43	78
29	57	62	13	20	32	81	42	79
30	55	61	12	18	30	81	42	79

ARTICLE VI.

Astronomical Observations, 1825. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

May 31. Lunar eclipse {Begin. 11h 52′ 43″; End.. 12 15 58} Mn. T. at Bushey. Shadow ill defined.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Siderial Time.

1825.	Stars.	Transit.		
May 28.	— *ι* Virginis	13h	17′	32·37″
28.	—Moon's First or West Limb	13	26	37·69
28.	—89 Virginis	13	40	26·31
29.	—317 Virginis	14	01	21·77
29.	—22 Virginis	14	05	50·19
29.	—38 Solitarii	14	09	02·69
29.	—116 Virginis	14	25	04·69
29.	—Moon's First or West Limb	14	26	36·41
29.	—212 Libræ	14	47	19·70
30.	—19 Scorpii	15	06	20·03
30.	—91 Libræ	15	21	44·30
30.	—Moon's First or West Limb	15	28	51·60
30.	—δ Scorpii	15	50	04·14
30.	—ω² Scorpii	15	56	39·14
31.	—δ Scorpii	15	50	04·40
31.	—ω¹ Scorpii	15	56	39·42
31.	—*o* Scorpii	16	10	11·88
31.	—*g* Scorpii	16	15	10·96
31.	—*i* Scorpii	16	18	40·18
31.	—ω Ophiu	16	21	51·02
31.	—Moon's First or West Limb	16	32	18·56
31.	—24 Ophiu	16	46	20·53
31.	—39 Ophiu	17	07	25·46
31.	—θ Ophiu	17	11	21·03
31.	—*b* Ophiu	17	15	46·08
31.	—*e*² Ophiu	17	20	49·35

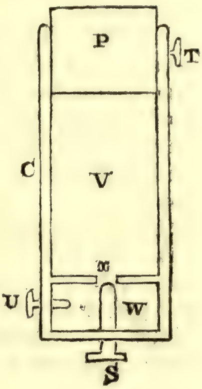
ARTICLE VII.

Explanation of the Theory of the Barometrical Measurements of Heights.

(*Continued from* vol. ix. p. 438.)

Of the Density of Aqueous Vapour in a Vacuum.

HAVING filled the lower division (or reservoir) W, of the cylindrical vessel C with perfectly *pure* water, close the aperture *x*, by screwing inwards the stopper S. The upper division V being a vacuum, and quite dry, affix within it by means of the clamp T, at any height above x, the cylindrical weight, or piston, P, of the specific gravity of mercury at 32° Fahr. and restore the communication between the water and the vacuum by unscrewing the stopper S. (22.) The temperature of the two divisions being preserved uniformly and constantly at 50° F. the elastic vapour emanating from the liquid will ascend through the aperture x, and instantaneously filling the chamber V, will press therein in every direction with a force determined solely by the temperature. The vertical height of the piston being 0·4 in. we may unclamp it, and although suffered to gravitate freely, it will continue perfectly stationary :—a column of mercury at 32° F. and of the height of 0·4 in. forming an exact counterpoise to the force, tension, or elasticity, of aqueous vapour of the temp. of 50° F.



(23.) Provided the temperature of the space continue at 50° F. the tension of the vapour therein will not be affected by an augmentation of the temperature of the liquid in W.

(24.) The weight (height) of the liberated piston being diminished in any ratio, the *excess* of force of the vapour would oblige it to ascend until the elasticity of the vapour, decreasing as the volume augmented, would merely support the reduced pressure; but being sustained at its initial force as the space increases, by continued supplies from the reservoir of water, it will succeed, if not prevented, in forcing the piston completely out of the cylindrical vessel C.

(25.) If we increase in the least degree the compressing weight, and suffer it to gravitate, the *whole* of the vapour will be condensed, and regaining in its liquid state the reservoir W, will allow the piston to descend to x.

(26.) Repeating our first experiment with a pressure equal to the force of the vapour, if we *compel* the piston to descend by degrees, we shall find that as the space diminishes, the vapour occupying that space will be liquefied without affecting in the least the force of the residue. The piston will consequently remain stationary every time we abandon it solely to its own gravitating force.

(27.) If we diminish in any degree the temperature of the space containing the vapour, a portion will repass to the liquid state; the elasticity of the remainder will be reduced, and become the same as though it had been formed in that diminished temperature.

(28.) The chamber V being saturated with vapour of any elasticity, cut off the communication with the reservoir W, by means of the stopper S. The piston having a pressure equal, or superior to the force of the vapour, will, in the former case, remain stationary on being liberated: on the latter supposition, it will descend, as before, to x, liquefying the whole of the vapour.

(29.) The pressure being inferior in any ratio to the force of the vapour, the volume of the vapour will be augmented, and its elasticity and density diminished in the same proportion.

(30.) The vapour not being in contact with the generating liquid, and supporting any pressure whatever, will have its volume augmented on being exposed to *superior* temperatures, at the uniform rate of $\frac{1}{480}$ per degree from 32° F.; whence its diminished density and increased elasticity may be easily calculated. In this, and in the preceding case, the space containing the vapour is said to be but partially saturated.

(31.) It is inferred from the experiments of Gay-Lussac that aqueous vapour is specifically lighter than dry air of equal elasticity and existing in the same temperature, very nearly in the ratio of 5 to 8. The temperature of the vapour of the experiments being 100° C. (some little inferior to 212° F.) in order to prevent the possibility of any portion of the liquid, of which the weight had been previously ascertained, remaining unevaporated, which might have occasioned the grossest errors, the pressure was slightly reduced, so as not to be fully equal to the tension of the vapour, or to 30 inches. Saussure, as well as Watt, had before estimated the difference to be as that of 5 to 7, but as the experiments of Gay-Lussac were made under greater advantages, and subsequent to the discoveries of Dalton, they are undoubtedly entitled to the preference. We should, however, remark, that Laplace and Biot have made use of the ratio of 5 to 7 in their barometrical investigations.

Of Air containing aqueous Vapour.

The chamber V containing perfectly dry air of an elasticity equal to the pressure of the gravitating weight P, secure the latter by means of the clamp T, and effect a communication between the dry air and water of the reservoir W, by unscrewing the stopper S.

(32.) Vapour of a force determined solely by the temperature of the air (supposed to be uniform), rising from the aqueous fluid, will gradually, but not instantaneously, diffuse itself uniformly within the space occupied by the air, precisely the same as in a vacuum. Arrived at the point of extreme saturation, the height of the water, slightly reduced by evaporation, continues unaltered. On restoring the dry air to its original volume, increased in proportion to the diminution of that of the aqueous fluid, by turning inwards the screw U, if we now increase the compressing weight in proportion to the augmentation of elasticity derived from the introduced vapour, we shall find, on liberating the weight, that it will remain precisely where we had clamped it: the additional pressure compensates the additional elasticity, and the original volume, or height, remains unaltered.

(33.) Varying the experiment, if we *double* the pressure now sustained by the mixture, the volume will be found to be decreased *more* than one-half. As the space, or volume, diminishes, the vapour existing therein liquefies and is precipitated. This is continued until the elasticity of the air, augmenting as its volume diminishes, added to that of the residual vapour, still at its initial force, equal together the doubled pressure.

(34.) From the former of the two experiments, we learn that the same quantity of vapour, and of the same force, is formed in a given space, uniformly of a given temperature, whether containing dry air, or being a perfect vacuum; for if we mix equal quantities of aëriform fluids of different elasticities, they may be compressed into the space occupied by one by submitting them collectively to a pressure equal to the sum of their elasticities.

(35.) From the other experiments, it is evident that when any space, being a vacuum, or containing dry air of any elasticity, is fully saturated with aqueous vapour in contact with water or not, we cannot increase, or otherwise alter the force of the vapour so long as any exists by augmenting the pressure, or otherwise partially but not entirely diminishing the space.

(36.) It is also obvious that the maximum of the force of the vapour existing in dry air will be determined by the temperature of the latter; so that a volume of vapour generated at a temperature of 50° F. could not possibly exist in its aëriform state in an equal volume of dry air of the temperature of 49° F. A portion of it would be precipitated, and the elasticity be reduced from 0·4 in. to 0·388 in. The air would now hold its maximum quantity of vapour, or be what is termed completely saturated.

(37.) Saturated air supporting any pressure being exposed, when out of contact with water, to any other *superior* temperature, or having its pressure *diminished,* will have its volume increased, and its density diminished, in the same ratio as dry air. In both cases, as well as when the supply of water is cut off before the volume of air has acquired the whole of the quantity of vapour it is capable of containing, the air is said to be but partially saturated, or only to a certain degree.

Of the Hygrometer.

(38.) A polished surface of glass, brass, &c. inferior in the slightest degree to the temperature of the air *saturated* with moisture with which it may be in contact will be perceptibly clouded with light dew.

(39.) When the air is but *partially* saturated, we must continue to lower the temperature of the polished surface, and mark the degree of the thermometer at which the deposition of the dew *first* takes place.

(40.) This temperature is termed the point of condensation, dew-point, or constituent temperature of the vapour.

(41.) The elasticity of the vapour contained in the air is understood to be equal to that of vapour generated and existing in its saturated state in a temperature equal to that of the observed dew-point. The hygrometer of Mr. Daniell is an elegant, but somewhat expensive instrument, to ascertain the temperature of the point of condensation.

(42.) When the air is not wholly saturated with aqueous vapour, a minute portion of water of the temperature of the air being isolated within it will evaporate with a sensible reduction of its heat, determined by the degree of saturation, temperature, &c.; consequently a thermometer having its bulb covered with humid bibulous paper, &c. will be observed at a temperature inferior to that of the ambient air; which difference, together with the other data, easily procured, will enable us to calculate the tension of the vapour contained in the air.

(43.) Hygrometers constructed of organic substances elongating from humidity indicate merely the *degree* of saturation; yet with this datum in addition to that of the observed temperature of the medium, we may ascertain the elastic force of the vapour with sufficient exactness for barometrical calculations.

Of the Density of Air containing aqueous Vapour.

(44.) The densities of *equal* volumes of different fluids are as their weights; and their weights, if elastic fluids having the same temperature, will be as their elasticities, or the pressures they sustain, multiplied by their respective specific gravities, determined when their temperatures and elasticities were the same.

(45.) Recollecting that as much vapour is formed in the same space, being a vacuum, or containing dry air, we may readily find the density of air wholly or partially saturated with moisture compared to that of dry air supporting the same pressure, and existing in the same temperature, by adding together the weights of the vapour and that of the dry air of the mixture. The elasticity of the dry air being 15 inches, and that of the vapour 10 inches, the observed pressure would be equal to the *sum* of their elasticities, or to 25 inches. Calling 1 the weight of a cubic foot of dry air of this latter pressure (25 inches), an equal volume of the dry air of the compound would weigh $\frac{15}{25}$ of 1, or .... 0·60

The cubic foot of vapour supporting the remaining pressure (10 inches) would weigh $\frac{5}{8}$ of $\frac{10}{25}$ of 1, or. .... 0·25

Sum ...... 0·85

The densities being as the weights, the moist air would be specifically lighter than dry air under the same observed pressure in the ratio of 85 to 100.

(46.) Were the density of aqueous vapour *equal* to that of dry air, the density of the mixture would not differ from that of dry air alone, observed under the same pressure; but being $\frac{3}{8}$ lighter, we must reduce in the same ratio what would have been on the former supposition its proportion of the total weight of the compound, equal to its proportion of the elasticity of the whole (or $\frac{10}{25}$ of 1).

Example.—Density at 25 inches .. 1·00
Deduct $\frac{3}{8}$ of $\frac{10}{25}$ of 1. .. 0·15

0·85 as before.

But this method of expressing the ratio of density of dry to moist air has the defect of not exhibiting the *value* of their difference in the most striking point of view, and would intolerably encumber and embarrass our calculations. We shall find it more convenient and intelligible to indicate at what (superior) temperature the density of dry air would be equivalent (or be reduced) to that of moist air observed under the same pressure.

To render the proposed method the more intelligible, we shall confine our calculations in the first instance to air in a state of complete saturation. Suppose, for example, that we had 480 volumes of dry air of a density, which we will call 480, at the temperature of 32° F. when supporting the pressure of 30 inches of mercury; required to know at what other (superior) temperature the density would be equal to that of saturated air supporting the same pressure, and existing in a temperature of 90° F.?

Calculation.

Dry air	at 32·00° F.	volume 480·00	Density 480·000
	90·00	538·00	428·253
	99·78	547·78	420·598

Saturated air at 90°. Dew-point 90°. (Force of the vapour 1·43 inch.) Density per formula 420·598 (or 428·253 *minus* $\frac{3}{8}$ of $\frac{1\cdot43}{30\cdot00}$ of 428·253).

Hence the density of saturated air observed at a temperature of 90° F. and under the pressure of 30 inches of mercury, is precisely equal to that of dry air under the same pressure, and of the temperature of 99·8° F. Had we substituted the ratio of 5 to 7, as given by Saussure and Watt, the temperature would have come out 97·4°.

Entering the following table with the degree of temperature indicated by a thermometer in contact with saturated air supporting a pressure of 30 inches, we have but to *add* to the observed temperature the corresponding equation, given in degrees and tenths, and the sum will indicate the temperature at which the density of dry air under the same pressure will equal that of the saturated air. When the pressure differs from 30 inches, multiply the equation by 30 inches, and divide the product by the observed pressure. The error *in defect* resulting from this approximative method will not exceed one-third of a degree in any case within the limits of barometrical observations.

Table of Equations for saturated Air.—Pressure 30 *Inches.*

Temp.	Add	Temp.	Add	Temp.	Add	Temp.	Add	Temp.	Add	Temp.	Add	Temp.	Add
0° F.	0·4°	13° F.	0·6°	26° F.	1·0°	39° F.	1·7°	52° F.	2·7°	65 °	4·2°	78° F.	6·7°
1	0·4	14	0·7	27	1·1	40	1·7	53	2·8	66	4·4	79	6·9
2	0·4	15	0·7	28	1·1	41	1·8	54	2·9	67	4·6	80	7·1
3	0·4	16	0·7	29	1·2	42	1·8	55	3·0	68	4·7	81	7·3
4	0·4	17	0·7	30	1·2	43	1·9	56	3·1	69	4·9	82	7·6
5	0·5	18	0·8	31	1·3	44	2·0	57	3·2	70	5·0	83	7·8
6	0·5	19	0·8	32	1·3	45	2·1	58	3·3	71	5·2	84	8·1
7	0·5	20	0·8	33	1·3	46	2·2	59	3·5	72	5·4	85	8·3
8	0·5	21	0·9	34	1·4	47	2·2	60	3·6	73	5·6	86	8·6
9	0·5	22	0·9	35	1·4	48	2·3	61	3·7	74	5·8	87	8·9
10	0·6	23	0·9	36	1·5	49	2·4	62	3·9	75	6·0	88	9·2
11	0·6	24	1·0	37	1·5	50	2·5	63	4·0	76	6·2	89	9·5
12	0·6	25	1·0	38	1·6	51	2·6	64	4·1	77	6·4	90	9·8

We must now propose a case of air *partially* saturated with aqueous vapour, the pressure being as before 30 inches, the temperature 90°, and the dew-point of the vapour 70°.

Calculation.

$$\text{Dry air}\begin{cases}\text{at } 32{\cdot}00^\circ & \text{Volume } 480{\cdot}00 & \text{Density } 480{\cdot}000\\ 90{\cdot}00 & 538{\cdot}00 & 428{\cdot}253\\ 95{\cdot}23 & 543{\cdot}23 & 424{\cdot}131\end{cases}$$

Moist air at 90°. Dew-point 70°. (Force of the vapour 0·77 in.) Density per formula 424·131 (or 428·253 *minus* $\frac{3}{8}$ of $\frac{0{\cdot}77}{30{\cdot}00}$ of 428·253).

The density of the moist air is consequently equivalent to that of dry air of the temperature of 95·23°.

With some trifling sacrifice to *extreme* accuracy, the preceding table will also serve to determine the equations for air not containing its maximum quantity of humidity. Enter the table with the observed dew-point (70°), instead of the degree of temperature indicated by the detached thermometer (90°), and add the corresponding equation (5°) to the temperature of the air (90°); and their sum (95°) will denote the temperature required. In the extreme case before us the discrepancy falls short of a quarter of a degree,—a quantity much inferior to the probable error of observation.

In case the construction of the hygrometer should be such as to indicate merely the degree of saturation, find by the table the equation for saturated air at the observed temperature, and reduce the quantity in proportion. The correction at 60° for air two-thirds saturated with moisture, and supporting a pressure of 30 inches, would be equal to $\frac{2}{3}$ of 3·6°, or to 2·4°. (See the tables given at the end of the first volume of the *Traité de Physique* by M. Biot, to reduce the degrees of saturation of the hair hygrometer of Saussure to the degree of tension of the vapour existing in the atmosphere.)

In the barometrical table of Mr. Daniell before alluded to are given the densities of saturated air at different temperatures under the pressure of *thirty inches;* but as no allusion is made to any correction for difference of pressure, and as the calculation illustrating the table is worked without introducing one, we must necessarily conclude that Mr. Daniell conceives the density of saturated air of any given temperature, supporting the pressure of 30 inches, to be specifically lighter than dry air of that pressure in the *same* ratio that saturated air of the same temperature under any *other* pressure is specifically lighter than dry air supporting that *other* pressure. To prove the incorrectness of the idea, let us find the density of a stratum of saturated air supporting a pressure of 30 inches, and that of another stratum under the pressure of 15 inches, the temperature of both being 90° F.

Density of dry air at 30 inches	1·0
Ditto at 15 inches....................	0·5

Density of saturated air at 30 in..	0·876245
Ditto at 15 inches	0·430149

Had the ratio been constant, the density of the saturated air at 15 inches would have been 0·4381225. It is evident that as the stratum under the lesser pressure contains a *greater* proportion of the *lighter* fluid, it must be specifically lighter than dry air in a *greater* ratio than the stratum supporting the heavier pressure.

When the *force* of the vapour rising from the surface of a liquid freely exposed to the atmosphere equals the *pressure* of the latter, ebullition ensues. Consequently if we note the temperature of the liquid, or that of the vapour immediately above its surface, when the ebullition is perfect, we may find by the tables giving the force of aqueous vapour at different tempera tures, the value of the atmospheric pressure, or height of the barometer. The objections to the method in regard to accuracy and convenience are, however, too serious to induce the observer to adopt it as a substitute for the latter instrument.

(*To be continued.*)

Article VIII.

A Letter from Dr. Black to James Smithson, Esq. describing a very sensible Balance.

Dear Sir, *Edinburgh, Sept.* 18, 1790.

I had the pleasure to receive your letter of the 9th. The apparatus I use for weighing small globules of metals, or the like, is as follows: A thin piece of fir wood not thicker than a shilling, and a foot long, $\frac{3}{10}$ of an inch broad in the middle, and $\frac{1\frac{1}{2}}{10}$ at each end, is divided by transverse lines into 20 parts; that is, 10 parts on each side of the middle. These are the principal divisions, and each of them is subdivided into halves and quarters. Across the middle is fixed one of the smallest needles I could procure to serve as an axis, and it is fixed in its place by means of a little sealing wax. The numeration of the divisions is from the middle to each end of the beam. The fulcrum is a bit of plate brass, the middle of which lies flat on my table when I use the balance, and the two ends are bent up to a right angle so as to stand upright. These two ends are ground at the same time on a flat hone, that the extreme surfaces of them may be in the same plane; and their distance is such that the needle when laid across them rests on them at a small distance from the sides of the beam. They rise above the surface of the table only one and a half or two-tenths of an inch, so that the beam is very limited in its play.

The weights I use are one globule of gold, which weighs one grain; and two or three others which weigh one-tenth of a grain each; and also a number of small rings of fine brass wire made in the manner first mentioned by Mr. Lewis, by appending a weight to the wire, and coiling it with the tension of that weight round a thicker brass wire in a close spiral, after which the extremity of the spiral being tied hard with waxed thread, I put the covered wire in a vice, and applying a sharp knife which is struck with a hammer, I cut through a great number of the coils at one stroke, and find them as exactly equal to one another as can be desired. Those I use happen to be the 1-30th part of a grain each, or 300 of them weigh 10 grains; but I have others much lighter.

You will perceive that by means of these weights placed on different parts of the beam, I can learn the weight of any little mass from one grain or a little more to the $\frac{1}{1200}$ of a grain. For if the thing to be weighed weighs one grain, it will, when placed on one extremity of the beam, counterpoise the large gold weight at the other extremity. If it weighs half a grain, it will counterpoise the heavy gold weight placed at 5. If it weigh $\frac{6}{10}$ of a grain, you must place the heavy gold weight at 5, and one of the lighter ones at the extremity to counterpoise it; and if it weighs only 1, or 2, or 3, or 4-100ths of a grain, it will be counterpoised by one of the small gold weights placed at the first, or second, or third, or fourth division. If on the contrary it weigh one grain and a fraction, it will be counterpoised by the heavy gold weight at the extremity, and one or more of the lighter ones placed in some other part of the beam.

This beam has served me hitherto for every purpose; but had I occasion for a more delicate one, I could make it easily by taking a much thinner and lighter slip of wood, and grinding the needle to give it an edge. It would also be easy to make it carry small scales of paper for particular purposes.

We have no chemical news. I am employed in examining the Iceland waters, but have been often interrupted. I never heard before of the quartz-like crystals of barytes aërata, nor of the sand and new earth from New Holland. Indistinct reports of new metals have reached us, but no particulars. Some further account of these things from you will, therefore, be very agreeable. Dr. Hutton joins me in compliments, and wishing you all good things; and I am, Dear Sir,

Your faithful humble servant,

JOSEPH BLACK.

Note by Mr. Smithson.—The rings mentioned above have the defect of their weight being entirely accidental; and consequently most times very inconvenient fractions of the grain. I have found that a preferable method is to ascertain the weight of a certain length of wire, and then take the length of it which corresponds to the weight wanted. If fine wire is employed, a set of small weights may be thus made with great accuracy and ease. Inconvenience from the length of the wire in the higher weights is obviated by rolling it round a cylindrical body to a ring, and twisting this to a cord.

This little balance is a very valuable addition to the blowpipe apparatus, as it enables the determination of quantities in the experiments with that instrument, which was an unhoped-for accession to its powers.

Dr. Black mentioned to me its having been used by an assayer in Cornwall, to whom he had made it known; and I have since heard, from another person, of an assayer in that county, who, finding the assays he was employed to make, cost him more in fuel than he was paid for them, had contrived means of making them at the blowpipe on one grain of matter. I presume him to have been the same Dr. Black had spoken of.

London, May 12, 1825.

ARTICLE IX.

Notice on the Diluvium of Jamaica. By H. T. De la Beche, FRS. &c. (Read at the Bristol Philosophical Society, May 12, 1825.)

ANY addition to our information respecting diluvium cannot be without interest to geologists, more particularly when it is derived from countries far distant from those which have been previously examined. Prof. Buckland's distinctions between diluvium and alluvium are too well known to require any explanation. That objections have been raised to these distinctions, and the discoveries connected with them, is most certain; but as Prof. Sedgwick very justly observes (*Annals of Philosophy*, April, 1825), "the greater part of the objectors are undeserving of any animadversion, as they appear entirely ignorant of the very elements of geology, and far too imperfectly acquainted with the facts about which they write to have it in their power to turn them to any account." In this class may not unfairly be placed the work which a writer in the Quarterly Journal of Science very gravely informs us is *masterly!!*

The following observations were made during a residence in the Island of Jamaica from December, 1823, to December, 1824. The first district which I shall notice is the great plain of Liguanea, upon the lower part of which the city of Kingston is situated. This presents an inclined surface, falling gradually from a height of about 750 feet (where the plain abuts against the mountains bounding it on the N) to the sea. This plain is almost wholly composed of diluvial gravel, consisting of the detritus of the Jamaica mountains, and evidently produced by causes not now in action.

The above section will explain better than words the manner in which the diluvium (A) abuts against the St. Andrew's mountains (B). The latter are composed of porphyry, syenite, greenstone, brownish red porphyritic conglomerate, siliceous sandstones, shales, and coal, that resemble our coal measures, with red sandstones and conglomerates of an older date resting upon transition rocks. Rounded portions of all these rocks compose the gravel of Liguanea; the pebbles are not in general very large; blocks, however, of siliceous sandstone, and of considerable dimensions, are found near the Hope estate, the property of the Duke of Buckingham.

The Hope river, with the Mammee river which falls into it, drains a considerable portion of the St. Andrew's mountains, and, when the waters are low, loses itself among the Liguanea gravels, at that part of its course where first quitting the rocky defiles of the mountains it enters upon this diluvial plain; but when the river is swollen by heavy tropical rains, it becomes a torrent of considerable magnitude, rushing with great force through the defile which opens upon the plain, by the continual recurrence of which it has excavated the gravel to a considerable depth; so that in fact the causes now in action tend to destroy the gravel plain rather than form it. The section of diluvial gravel made by the river near the Hope Tavern, cannot be less than between 300 and 400 in depth, and the tavern itself is, according to the barometrical measurement I made, 698 feet above the level of the sea. The diluvial gravel rises some height above the tavern.

Proceeding up the Hope Valley, a part of the road displays a section of diluvium (the rounded rock pieces of which are large), resting upon a projecting portion of the mountain that rises about 600 feet above the bed of the river.

In addition to the Hope river, numerous gullies formed by heavy tropical rains, cut the diluvial plain of Liguanea in various directions, so that far from being formed by the waters which now descend from the mountains, every stream that traverses it tends to destroy it, and carry the gravel into the sea.

This plain descending gradually to the sea, and being protected from the ravages of the latter by the Palisades, a sand bank extending several miles from Port Royal to the main land, alluvial matter is deposited on parts of the shore, more particularly between Kingston and Port Henderson, on which mangrove trees are numerous; in fact these trees are extremely well calculated for the accumulation of alluvium, their long stilt-like roots collecting mud and other matters together, and then protecting what they have collected from being washed away by any violent rush of water, the numerous roots breaking its force.

It is almost impossible to stand upon the gravel plain of Liguanea without feeling convinced that it could not have been formed by any causes now in action, but that the porphyry, greenstone, and other pebbles, which constitute, with a few clay and sand beds, the mass of the plain, were derived from the Jamaica mountains in the same manner, and at the same period, as the numerous European tracts of gravel, which have resulted from the destruction of European rocks, and which contain the remains of elephants, &c. It is true that bones have not yet been discovered in the Jamaica gravels, but it should be recollected that the opportunities for such discoveries are by no means so abundant as in those countries where gravels are extensively used for roads; the climate moreover is such, that few are tempted to risk their health by prosecuting researches of this nature beneath a tropical sun.

The diluvial plain of Liguanea is continued westward through the low lands of St. Catherine and St. Dorothy; sands and clays are more abundant in the latter districts, but in other respects the diluvium is the same. The sections afforded by the rivers and gullies are of considerable interest, though I nowhere observed one so deep as that of the Hope River.

To the westward of the above-mentioned plain, but separated from it by a range of low white limestone hills, is another great plain forming the parish of Vere, and the lower part of that of Clarendon; it is surrounded by white limestone hills and mountains* on all sides but on the S and SW, where it is washed by the sea, with the exception of the space occupied by Portland Ridge.

The greater part of this plain is diluvial, consisting of gravels, clays, and sands; the former is principally composed of porphyry, greenstone, and other trap rock pebbles, which are all most probably derived from the destruction of part of the St. John's and Clarendon mountains.

Many fine sections of this diluvium are afforded by the Rio Minho, which traverses it nearly through its whole length, as also by numerous deep gullies: it is easy to remark here, as in the case of Liguanea, that the causes now in action tend to destroy this plain, and are altogether inadequate to its formation.

It is remarkable that though this diluvium is nearly surrounded by hills and mountains of the white limestone formation, very few fragments of it are to be discovered in the gravel, arising probably from its being less hard than the porphyry and greenstone, and, therefore, less able to resist any violent attrition than the latter.

* It is sufficient at present to observe, that the Jamaica white limestone formation consists of compact white limestone beds, resembling the compact varieties of the Jura limestone; these beds are often of very considerable thickness, and are associated with softer limestones (even in some places resembling chalk), white marls, and thick beds of red sandstone and marl. The whole formation cannot be less than 2000 feet thick in some places, and contains fossils, which are, however, very rare in it, of a tertiary character, such as cones, cerithia, nummulites, &c.

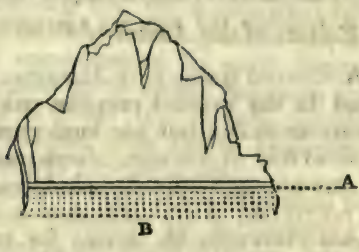
Beneath this diluvium, but above the white limestone formation, an ambiguous conglomerate occurs, which, from the superior degree of its consolidation, it seems difficult to refer to the diluvial period: the pebbles, however, very closely resemble those which are certainly the products of that geological era. The Vere plain, like that of St. Dorothy and Liguanea, is bounded, where it touches the sea, by alluvium and mangrove trees.

There are other diluvial districts in Jamaica; the above are, however, sufficient for my present purpose, which is merely to show that the diluvium of Jamaica has been produced by similar causes with the diluvium of Europe, which latter I have had opportunities of examining in the British Isles, France, Italy, Germany, &c.

As connected with this subject, it may be as well to notice the valleys formed in the white limestone hills and mountains: these present the usual varieties observed in other parts of the world; yet instances are rare in which water will be found to flow through them; the white limestone formation is in fact extremely cavernous, and the rains that fall, which, it is well known, are very heavy in the tropics, are received into innumerable sink holes and cavities, and disappear; sometimes, but rarely, again rising and flowing for a short distance, again to be swallowed up. The districts occupied by this cavernous limestone are very extensive, and their places are generally shown on a *good* map of Jamaica by a want of rivers; whereas the latter are abundant among the other rocks. Here we have instances of valleys, several of which are of very considerable depth, without running waters in them; they could not, therefore, be formed by the waters which now traverse them, since there are none which do so: these valleys, then, are completely opposed to the theory that valleys owe their origin to the streams or rivers which now run through them.

A valley of denudation is seen at Williamsfield (the property of Lord Harewood) in St. Thomas in the Vale. The caps of the hills, or rather mountains, on either side are formed of white limestone in nearly horizontal beds; these rest on porphyritic and other trap rocks forming the bottom of the valley in which the Rio d'Or flows. This valley is, therefore, similar (as far as respects denudation) to the valleys formed in the green sand and lias near Lyme, and the green sand and new red sandstone near Sidmouth; for there would appear no more reason to doubt that the white limestone near Williamsfield had once been joined by strata now swept away, than that the green sand of the hills in the neighbourhood of Sidmouth and Lyme had once been continuous.

During my residence in Jamaica I visited, among other caverns, that most celebrated, which is named Portland Cave, from being situated in Portland Ridge, Vere. The following is a section of part of this cave, in which two or three circumstances deserve attention, as they cannot fail to remind the reader of some of Prof. Buckland's cavern sections.



A stalagmitic floor (A) rests upon a fine silty clay (B), the depth of which I could not ascertain; one or two large stalactite columns appear also to rest upon the clay; but of this I am not certain; the heat, in fact, was so oppressive (from being near the surface) during the time I visited it, that I was prevented from remaining long in the cavern.

This cave is situated on the side of a hill, and is a short distance from the sea, but sufficiently elevated above it to prevent the possibility of the clay being derived from it at its present level. The crust of stalagmite is of sufficient thickness to show that it must have taken a long time to form. I did not observe any bones beneath it, and am now sorry that proper search was not made, as the depth of the silty clay has not been ascertained, and as it might contain bones.

Portland Cave has been visited by hundreds of persons, most of whom have written their names on almost every accessible portion of it: the floor, therefore, cannot be expected to be in the condition in which it was first discovered, and it would be difficult to say how far the stalagmitic crust might have extended. The portion that I observed was not large, and is in itself of little importance; but it becomes interesting as connected with the sections of caverns, beneath the stalagmitic floors of which bones have been discovered.

Article X.

On the Genus Ursus of Cuvier, with its Divisions into Subgenera. By John Edward Gray, Esq. FGS.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *British Museum.*

Linnæus placed in the genus Ursus the whole of the heel walking carnivorous animals; but the modern zoologists have reduced it to those of his genus which have one or more small distant false grinders between the true grinders and the canine teeth; therefore restricting it to Ursus Arctos and Maritimus, the only two species known in his time. But by the exertions of travellers we have become acquainted with six other species, which most authors have found very difficult to characterise, or they have at least been involved in considerable obscurity from the want of observing and comparing them together.

Having lately had the opportunity of examining six species alive, three of which are at present in the menagerie attached to the Tower of London, and the rest moving about the country in the caravans of the itinerant exhibitors, I have been enabled to divide the genus into sections, which I hope will facilitate the knowledge of the species.

The divisions may be regarded as subgenera; like most natural groups they are each confined to particular parts of the globe, with a few exceptions, which may be explained by considering the confusion that has hitherto existed regarding their species: thus Desmarest, when he placed India among the habitat of the common bear, appears by his description to have confounded the Malay or long liped bear with that species.

I propose to divide the genus into

I. *Those which have short conical recurved claws, adapted for climbing.*

This group may be considered as the type of the genus, and it contains the

1. *European Bears,* which have convex foreheads and long heels, as

1. Ursus Arctos. *Lin. Syst. Nat.* i. *β albida.*
2. Ursus collaris. *F. Cuvier, Mamm. Lithog.*
3. ——- Pyreniacus. *F. Cuvier, Mamm. Lithog.*

The two latter may be only varieties of the former species, and *Ursus Tibethianus* may, perhaps, be more properly referable to this group.?

2. The *American Bears,* with flattened foreheads and short heels, as

4. Ursus Americanus, *Pallas.* U. gularis, *Geoff.*

The cinnamon, or yellow bear, of Catton's animals, and the chocolate bear, both of which are alive in the Tower, may, perhaps, be considered as varieties of this species; but I regret that I am not enabled to verify this fact, as I have never seen either a live specimen or skull of the type of the species, which would have enabled me to speak with more precision.

II. *Those which have long compressed claws, fitted for digging.*

This group contains three sections, all which differ considerably from the type of the genus, but the centre one particularly, as may be judged from the fact, that it has ever been placed in a different part of the system.

3. The *Great American Bear,* which agrees with the other American bears in their general form, but differs from them in its longer heels, are very large nearly straight claws, as

5. Ursus, ferox, *Desm.* Ursus cinereus, U. horibilis, *Ord.* The grisly bear of Lewis and Clark's Travels. Danis ferox, *nob.*

This species is very distinct from the two presumed varieties of the other American species, next to which it is placed in the Tower, where it has been kept for 15 years, as is well known to most of the visitors, by the name of Old Martin : it is upwards of seven feet long, and exceedingly strong; but it is obedient to the keeper, and sits upon its haunches when desired.

This animal exhibits in a remarkable manner the fastidiousness of zoological artists. It has been in this country for many years, and most of the animals which surround it in the above-named collection have been published two, and some even three times over, while this has never even been drawn. I cannot find that any figure has been published of it on the Continent, which is not to be wondered at when we consider its habitation. Mr. Say, in his excellent account of the animals collected in Mr. James's interesting Travels to the Rocky Mountains, refers to a figure belonging to that work, which may be in the American edition, but it is certainly not to be found in the English one. This section will form a very distinct subgenus, for which I propose the name of *Danis.* Clinton considered it to be the recent state of *Megalonyx* of Jeferson, but Mr. Cuvier has referred the latter animal to his genus *Megatherium;* but as it is probable that it will at least form a distinct genus, I cannot use the above expressive name for the recent animal, as it has been preoccupied for the fossil one.

4. The *Asiatic Bears,* which have very long, extensile, and exceedingly mobile lips, narrow, long, and extensile tongue, very broad and rather depressed heads, and are usually of a dark brown colour, with a white forked mark upon their chest, as

1. Prochilus labiatus, *nob.* Ursus labiatus, *Cuv.* Bradypus ursinus, *Shaw.* Prochilus ...... *Illiger,* Melursus. *Meyer,* Chrondorhynchus, *Fischer.*

2. Prochilus Malayanus, *nob.* Ursus Malayanus, *Raffles.*

3. Ursus Tebethianus. *F. Cuv. Mamm. Lithog.*

The specimen of the first described species of this division was destitute of cutting teeth, and was, therefore, against the example of Linnæus himself, placed with the Bradypi, the only genus possessing canine teeth and grinders, and wanting the cutting teeth, under the name of *Bradypus ursinus,* or *Ursine Sloth.* The illustrious Illiger, not knowing that the cutting teeth had been destroyed, but aware that it had not the habits of the sloth, by its external organisation, formed it into a genus distinct from them under the name of *Prochilus.* Meyer, regardless of the name of Illiger, gave it the name of *Melursus,* and Fischer in his Zoognomia that of *Chrondyrhynchus.* It was Buchanan, in his Travels in the Mysore, that first pointed it out as being a bear. This group forms a very distinct subgenus; I should, therefore, recommend the adoption of the former name of Illiger's, on account of its aptness and priority for the whole group.

I have seen four specimens of the *Prochilus labiatus,* all of which had their cutting teeth destroyed; but whether it was done before they arrived in this country, or by the showmen to make them sloths, and thus agree with their bills, I know not, nor could ever discover.

The *Malay Bear* is very remarkable for the depressed form of its body, and its low manner of walking; for its body (when in confinement at least) nearly touches the ground, and its feet are uncurved so as nearly to touch each other. It was first described by Sir Stamford Raffles in the Linnean Transactions, and figured by Mr. Griffith, from a drawing by my friend Major Hamilton Smith, after a stuffed specimen in the Museum, which was presented to that establishment by Lady Banks; and again by him in his Translation of Cuvier's Animal Kingdom, from an excellent drawing by Landseer, after the specimen at present alive in the Tower; but the attitude of neither of the plates gives the peculiar appearance of the animal when it walks—a peculiarity to be observed, but not so fully developed in the Ursus labiatus. The Malay Bear has the very peculiar depressed broad-rounded head, the thin lengthened snout, very long, extensile, narrow tongue of the Prochilus labiatus, with which it was confounded by the showmen when it was first brought alive to this country.* I have not been able to learn if the same peculiarities are found in the Tibeth Bear, as I only knew that species from the figure and description of Mr. Frederic Cuvier. I have, therefore, placed in this section with a mark of doubt. The

* And this species has been confounded together on the Continent.

latter species agrees with the rest of the section, in having the white mark on the neck, but this character is also found in the European species.

The peculiarity of the nose and lips is found in a slight degree in the American Bears, especially the *Ursus horribilis*; but I have not observed it in the common brown bear.

Dr. Leach figured the skull of the Malay Bear from the specimen in the Museum, if I recollect rightly, under a new generic name; but I can neither find the skull, or a copy of the lithographic plate: the former might have been given by Sir Joseph Banks for his private collection; and with regard to the latter, I do not much regret its scarcity, as it would only be adding another name to the numerous synonyma already given to the subgenus to which it belongs.

III. *Those species which have rather short uncurved claws, and broad hairy paws for swimming*: they are usually of a white colour, and have long heads, and several false grinders in the space between the canine and grinding teeth, as the *Sea Bears*.

Ursus Maritimus, *Lin. nob.* Thalarctos polaris.

I have heard several zoologists observe, that they believe there are two species confounded under the name of the *Polar Bear;* but I have examined several skulls, and seen three living specimens, all of which certainly belonged to one species.

This species forms a very distinct subgenus on account of its habits, colour, the form of its skull, and the number of its false grinders. I propose to designate it with the name of *Thalarctos.*

Article XI.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1824. *Part II.*

The following papers are contained in this part of the Philosophical Transactions:—

XI. *Some curious Facts respecting the Walrus and Seal, discovered by the Examination of Specimens brought to England by the different Ships lately returned from the Polar Circle.* By Sir E. Home, Bart. VPRS. In a letter addressed to Sir H. Davy, Bart. Pres. RS.

Some account of this communication will be found in the *Annals* for April 1824, in the report of the proceedings of the Royal Society, p. 307.

XII. *Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of the*

Copper Sheathing of Ships, and to other Purposes. By Sir H. Davy, Bart. Pres. RS.

This valuable paper was given entire in our number for April last.

XIII. *On the Apparent Direction of Eyes in a Portrait.* By W. H. Wollaston, MD. FRS. and VP.

As it would be impossible to convey a satisfactory knowledge of the contents of this curious explanation of an interesting question in perspective, without giving the beautiful engravings by which it is illustrated, in which the skill of the President of the Royal Academy has been exerted; we must refer such of our readers, as may be particularly interested in the subject, to the original communication: the following extracts, however, embrace the chief points of inquiry, and will indicate the nature of the explanation.

"When we consider the precision, with which we commonly judge whether the eyes of another person are fixed upon ourselves, and the immediateness of our perception, that even a momentary glance is turned upon us, it is very surprising that the grounds of so accurate a judgment are not distinctly known, and that most persons, in attempting to explain the subject, would overlook some of the circumstances by which it will appear they are generally guided.

"Though it may not be possible to demonstrate, by any decisive experiment on the eyes of living persons, what those circumstances are, still we may find convincing arguments to prove their influence, if it can be shown, in the case of portraits, that the same ready decision we pronounce on the direction of the eyes is founded in great measure on the view of parts which, as far as I can learn, have not been considered as assisting our judgment.

"Previous to a full examination of this question, one might imagine that the circular form of the iris would be a sufficient criterion of the direction in which an eye is looking; since, when the living eye is pointed to us, this part is always circular, but cannot appear strictly so, when turned in such a manner that we view it with any degree of obliquity. But, upon farther consideration, it is evident that we cannot judge of exact circularity with sufficient precision for this purpose, even when the whole circle is fully seen, and in many cases we see too small a portion of the circumference of the iris to distinguish whether it is circular or elliptic.

"Moreover, in a portrait, although the iris be drawn most truly circular, and consequently will appear so when we have a direct view of it; still, in all oblique positions, it must be seen as an ellipse. And yet the eye, as is well known, apparently continues to look at the spectator, even when he moves to view them very obliquely, and sees them of a form most decidedly elliptic.

"The reason why the eyes of a portrait seem to follow us will be hereafter considered, but cannot be rightly explained, until the circumstances on which apparent direction in the front view depends, are fully understood.

"If we examine with attention the eyes of a person opposite to us, looking horizontally within about twenty degrees on either side of us, we find that the most perceptible variation in the appearance of his eyes, in consequence of their lateral motion, is an increase and decrease of the white parts at the angles of each eye, dependent on their being turned to or from the nose.

"In the central position of an eye, the two portions of white are nearly equal. By this equality, we are able to decide that a person is looking neither to his right nor to his left, but straight forward in the direction of his nose, as index of the general position of his face.

"If, on the contrary, he turn his eyes to one side, we are immediately made sensible of the change by a diminution of the white of the eye on that side to which they turn; and by this test alone we are able to estimate in what degree they deviate in direction from the face to which they belong.

"But their direction with reference to ourselves is perfectly distinct from the former; and in judging of this, it seems probable that, even in viewing real eyes, we are not guided by the eyes alone, but are unconsciously aided by the concurrent position of the entire face; for in a portrait, the effect of this further condition admits of being proved by a distinct and decisive experiment.

"If a pair of eyes be drawn with correctness, looking at the spectator, at such moderate deviation from the general position of the face as is usual in the best portraits, unless some touch be added to suggest the turn of face, the direction of the eyes seems vague, and so undetermined, that their direction will not appear the same to all persons; and to the same person they may be made appear directed either to him or from him by the addition of other features strongly marking that essential circumstance—the position of the face."

Dr. Wollaston then proceeds to explain the illustrative engravings already mentioned, which completely establish his positions, and gives the subjoined remarks on a collateral subject at the conclusion of the paper.

"With this previous knowledge of the influence which the general perspective of the face in a portrait has upon the apparent direction of the eyes, we shall be prepared to examine why, if they look at the spectator when he stands in front of the picture, they follow, and appear to look at him in every other direction.

"If we consider the effect produced by our change of position with reference to any other perspective drawing, we find a similar permanence of apparent position of the objects represented with respect to ourselves, and corresponding change of direction with reference to the plane of representation, or to the room in which it hangs; and we shall be able, in this case, distinctly to trace its origin in the simplest principles of perspective drawing.

"When two objects are seen on the ground at different distances from us in the same direction, one will appear, and must be represented, exactly above the other. The line joining them is an upright line on the plane of the picture, and represents a vertical plane passing through the eye and these objects. When objects that are at different elevations, are said to be in a line with us, the strict meaning is, that they are so placed that a vertical plane from the eye would pass through them. Now, since the upright line (drawn or supposed to be drawn on the plane of the picture, and representing a vertical plane) will be seen upright, however far we move to one side, and will continue to represent a vertical plane, it follows that the same set of objects, even in the most oblique direction in which the representation can be viewed, are still in the same vertical plane, and consequently will seem still to be in a line with us, exactly as in the front view; seeming, as we move, to turn round with us, from their first direction, toward any oblique position that we may choose to assume.

"In portraits, the phænomena of direction with reference to the spectator, and corresponding change of apparent position in space when he moves to either side, depend precisely on the same principles. A nose drawn directly in front with its central line upright, continues directed to the spectator, though viewed obliquely. Or, if the right side of the nose is represented, it must appear directed to the right of the spectator in all situations; and eyes that turn in a due degree from that direction towards the spectator, so as to look at him when viewed in front, will continue to do so when viewed obliquely."

XIV. *Further Particulars of a Case of Pneumato-Thorax.* By John Davy, MD. FRS.

A brief abstract of this communication will be found in the *Annals* for May 1824, p. 383.

XV. *On the Action of finely divided Platinum on Gaseous Mixtures, &c.* By Dr. Henry.

Given at large in our last number.

(*To be continued.*)

Article XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

June 2.—A paper was read, entitled, "Microscopical Observations on the Materials of the Brain, Ova, and Testicular Secretions of Animals, to show the Analogy that exists between them; by Sir Everard Home, Bart. VPRS."

June 9.—The following papers were read:

Description of a Method of determining the Direction of the Meridian; by John Pond, Esq. FRS. Astronomer Royal.

Further Researches on the Preservation of Metals by Electrochemical Combinations; by Sir Humphry Davy, Bart. PRS.

In this paper Sir H. Davy enters into a minute detail of the causes which operate in producing foulness, as it is called, or the adhesion of weeds and shell fish to the copper of ships. This he attributes to a crust of carbonate and submuriate of copper, and carbonate of lime and magnesia, which gradually fix upon the sheathing, and which by rendering the copper in the surrounding parts positive, occasions its corrosion, so that ships are sometimes found, in the common course of wear, foul in some parts, and the copper worn in holes in other parts.

He conceives that *proper protection,* if not in excess, by producing a similarity of *electrical* state or of disposition to chemical change in every part of the copper, will prevent the rapidity of its wear without giving it any disposition to foulness; but if iron or zinc are used in such quantities as to save all the *copper,* then they will increase the disposition of that metal to become covered with weeds and shell fish, except in cases of rapid motion, such as in *steam boats,* where the chemical action of sea water upon copper may be entirely prevented without the possibility of the copper becoming foul.

The President describes a number of experiments which show that the most rapid motion does not interfere with the principle of protection. He describes the relations of this property of electrochemical agency to the conducting powers of metals and of fluid conductors; and he shows that a certain contact with fluid conductors, even upon a small scale, is sufficient to enable oxidable metals to preserve more difficultly oxidable metals; and that *slight* chemical changes are sufficient for the effect. Iron in a solution of brine which contains no air is very slowly acted upon, and yet iron in brine in one cup will preserve copper in sea water in another cup, provided they are connected by a moist thread of cotton. He points out the limits to this kind of action, and illustrates it by a very curious experiment. If of two vessels containing salt and water connected by moist cotton, and forming an electrochemical series by means of zinc and iron, a few drops of solution of potash or soda be poured into that containing the iron, the action of the iron on the sea water will be diminished; but the copper will still be protected: but if the solution containing the iron be made alkaline to any extent, the copper will begin to corrode, and the iron will become the electro-negative metal.

Sir Humphry ends this paper by the important practical conclusion, that copper may be preserved by nails, or masses of zinc or iron placed *under* the sheathing; and that in this way there is less loss of the oxidable metal, which is partly revived upon the interior of the copper, so that the same metal will act for a long time, and thus protectors may be applied to save the whole or any portion of the copper without interfering with the external surface of it, and without the deposition of any matter likely to cause adhesion.

June 16.—MM. Bessel, Encke, Fresnel, and Brongniart, and Count Chaptal, were elected Foreign Members; C. M. Clarke, Esq. was admitted a Fellow of the Society; and portions of the following papers were read:—

On some new Compounds of Carbon and Hydrogen, and on certain other Products obtained during the Decomposition of Oil by Heat; by M. Faraday, FRS.

Account of the Repetition of M. Arago's Experiments on the Magnetism developed in various Substances during the Act of Rotation; by C. Babbage, Esq. FRS. and J. F. W. Herschel, Esq. Sec. RS.

Experiments on Magnetism produced by Rotation; by S. H. Christie, Esq. MA. FRS.

On the Annual Variation of some of the principal fixed Stars; by John Pond, Esq. FRS.

Description of an improved Hygrometer; by Mr. T. Jones: communicated by Capt. Kater, FRS.

On the Nature of the Function of Mortality, and on a new Mode of determining the Value of Life Contingencies; by Benjamin Gompertz, Esq. FRS.

The Society then adjourned to the 17th of November next.

LINNEAN SOCIETY.

March 15.—"Read a paper from R. A. Salisbury, Esq. FRS. FLS. &c. on the *Trichomanes elegans* of Mr. Rudge's *Plantæ Guianæ*. It appears that M. Bory de St. Vincent asserts in the 6th vol. of the *Dictionnaire Classique d'Histoire Naturelle*, under the article *Fougere*, that Mr. Rudge's plant t. 35, is composed of two species of different genera, one of which Mr. Bory proposes as a *Feea*, and the other, as constituting a new genus, under the name of *Hymenostachys.* Mr. Salisbury, however, insists, that M. Bory's assertions are devoid of any foundation, and he attributes his criticisms to an ignorance of the Latin language. In confirmation of this opinion, Mr. S. exhibited the specimen itself from which the figure had been drawn, that he might afford ocular demonstration that it consisted of one individual.

"To corroborate this opinion, he adduces the testimony of Professor Hooker, who, in his 52d plate of his *Exotic Flora,* refers to Mr. Rudge's figure, and gives a coloured one of *T. elegans,* the involucrum of which contained ripe capsules.

"The question being a matter of reference to the Society, the Vice President named Mr. Edward Forster, Mr. Bicheno, and Mr. Menzies, to investigate the matter, and report thereon, in pursuance of a bye-law of the Society."

April 5.—A valuable present of stuffed birds and fishes was received from Capt. King, collected by him in his late expedition to explore the north-west coast of New Holland.

"The committee appointed at the preceding meeting made their report relative to Mr. Salisbury's paper on *Trichomanes elegans;* and stated that the plant was represented to have been gathered in Guiana, by M. Martin, and to have been purchased by Mr. Rudge. It belongs to the genus *Trichomanes* of Smith. M. Bory asserts that the spike described as the mature fructification, is of a totally different structure from the others, which are regarded as immature. It appears that Hooker did not doubt the fidelity of Mr. Rudge's plant, though his own figure supports M. Bory's opinion, inasmuch as the fronds there delineated differ from those in Mr. Rudge's figure.

M. Poiret has described, and M. Desveaux has both described and figured, the plant which corresponds with the fructification supposed to be mature. Weber and Mohr have also the same species.

In the Banksian and Mr. Brown's collection, were found several specimens of each of the two plants, alleged by the French author to be confounded in all stages of fructification. In every instance the Committee found the barren frond of Mr. Rudge's specimen combined with the fructification which he calls the young; and as constantly the frond, figured by Hooker, with the spike which is said to be mature.

The specimen itself was also subject to their inspection; and upon a minute examination of it, they were satisfied that it was composed of two individuals. They therefore reported that M. Bory appeared to them to be justified in his conclusions It was added, that they thought it but justice to Mr. Brown to say, that Mr. Salisbury was correct in stating that M. Bory had fallen into the error of making Mr. Brown adopt Willdenow's arrangement of the Ferns, whereas Mr. Brown's work made its appearance in the same spring, but before Willdenow's, and his arrangement is materially different."

A farther portion of Dr. Hamilton's Commentary on the third part of the *Hortus Malabaricus* was also read.

ASTRONOMICAL SOCIETY.

May 13.—The reading of Mr. Henry Atkinson's elaborate communication on the subject of Refraction was concluded. In the course of this paper the author has collected and arranged a great variety of meteorological observations, made in different seasons, and at different parts of the world, for the purpose of enabling him to ascertain the mean temperature at the equator and in different latitudes, as well as the law of variation in the temperature of the air at different heights above the level of the sea. From these data he has deduced formulæ, by the use of which the *computed* and *observed* mean temperatures in any given place, or at any given height, appear to agree in a remarkable manner. His next inquiry is, to ascertain the law by which the *height* and the *elasticity* of the air is connected; and also the relation between the *elasticity* and *density* at any given height. These inquiries are guided by observations and experiments that have been made and published by men of eminence in this department of science. The reasoning and deductions are founded on acknowledged facts; and hypothesis furnishes no part of the data from which the *tables*, founded on these investigations, are computed. *Astronomical* observations supply no portion of the materials which form the basis of the computations, but all the results are obtained by formulæ depending on *optical principles;* so that the near agreement of the quantities contained in these tables (when properly collected) with those given by the most approved modern tables of refraction proves that the various formulæ by which these quantities were obtained are founded in nature, as well as happily applied. The atmosphere is divided into a variety of strata, and each stratum has its appropriate formula for determining its share of mean refraction; and when the different portions belonging to the different strata are put together in succession, they constitute such an arrangement of quantities as proceed by a regular gradation, or very nearly so; and nothing but a close examination of the differences can detect that the whole succession has not depended on one continued formula. Besides the atmospheric refractions adapted as corrections for celestial observations, the author has applied one of his formulæ successfully to determine the terrestrial refraction as it has reference to two objects standing in different elevations: so that whether this memoir be considered as a meteorological, geodetical, or astronomical communication, it cannot but be regarded as copious, elaborate, and interesting.

There was also laid before the meeting an account of observations made at Paramatta, in New South Wales, by Major-Gen. Sir Thomas Brisbane, KCB. Governor, &c.; communicated in a letter to Francis Bailey, Esq. President of this Society. These refer to the solar eclipse on January 1, 1824; to several occultations of fixed stars by the Moon; to stars observed with the Moon near her parallel; to observations before and after the superior conjunction of Venus with the Sun, July and August 1824; to observations on the planet *Uranus* near the opposition in July 1824; and to observations on two comets, one of which was not observed in Europe.

Next there was read a report On the Properties and Powers of an Altitude and Azimuth Circle constructed by E. Troughton, and divided by T. Jones; drawn up by the Rev. W. Pearson, LLD. FRS. and Treasurer to this Society. The peculiarities of the construction of this fine instrument cannot be adequately described in an abstract. But some estimate may be observed of its accuracy from stating, that by comparing the *mean* latitude of South Kilworth Rectory (Leicestershire) with each and all of *sixteen* separate determinations, it does not differ more than one second and one-tenth from the extreme latitude; that the true obliquity of the ecliptic at the December solstice 1824, as determined by this instrument, was 23° 27′ 44″,01; while the mean of the determinations of Delambre, Brinkley, and Bessel, is 23° 27′ 44″,55. Observations on the pole-star, and another determination of the obliquity of the ecliptic, by a method suggested by Dr. Brinkley, serve still further to confirm the character of the instrument for accuracy, and the value of such an instrument when used by a skilful, scientific, and experienced observer.

The reading was commenced of a paper On the Construction and Use of some new Tables for determining the apparent place of about 3000 principal fixed Stars; drawn up, at the request of the Council, by the President.

June 10.—The reading of Mr. F. Baily's introduction to the new Tables for determining the apparent places of 3000 fixed stars, was resumed and completed. This copious introduction commences with a historic sketch of the most important tables which have hitherto been published for similar purposes; none of which, however, are so extensive as the tables to which the present paper is introductory. They comprehend, first, all stars above the *fifth* magnitude wherever situated; secondly, all the stars, not less than the *sixth* magnitude, situated within 30° of the *equator;* thirdly, all the stars, not less than the *seventh* magnitude, situated within 10° of the *ecliptic.*

After a few general observations, Mr. Baily speaks in succession of the distinct topics of *aberration, annual precession,* and *nutation;* exhibiting the analytical formulæ which have been proposed for the computation of their respective values at any time, past, present, or future; assigning the reasons for the adoption of those values of the *constants* which he has preferred; and so transforming the several formulæ, as to facilitate and effect their reduction into one class of comparative simplicity, which forms the basis of the tables themselves. Thus the total corrections for right ascension and declination respectively, assume the forms

$$\Delta\,\alpha = \mathrm{A}\,a + \mathrm{B}\,b + \mathrm{C}\,c + \mathrm{D}\,d$$
$$\Delta\,\delta = \mathrm{A}\,a' + \mathrm{B}\,b' + \mathrm{C}\,c' + \mathrm{D}\,d'$$

where the quantities denoted by a, b, c, d, and the accentuated a', b', c', d', are *constant* for each star, while the quantities A, B, C, D, are common to every star. The quantities A, B, are rendered equally constant for all the stars by the assumption of a fictitious year, commencing at that moment when the Sun's *mean* longitude at Greenwich at *mean* noon on Jan. 1, is 281°; which is, therefore, assumed as the *tabular date,* and the mode of adopting it to the current date is explained.

The author then explains the arrangement and use of the tables. The general catalogue of the stars is arranged in the order of the right ascensions, and reduced to Jan. 1, 1830. The left hand page is confined to the *right ascensions,* the right hand page to the *declinations.* Col. 1, on the left hand, exhibits the *numbers* of the stars. Col. 2, the *names;* to which are prefixed Flamstead's numbers, and the letters of the alphabet, by which they are usually distinguished. Col. 3, denotes the *magnitudes* of the stars. Col. 4, AR *in time,* for Jan. 1, 1830. Col. 5, the annual precession *in time.* The remaining columns contain the logs. of a, b, c, d, each previously divided by 15 to reduce them to time.

On the right hand page, Col. 1 is the same as Col. 1 on the left hand. Col. 2, exhibits the *declinations* of the stars, Jan. 1, 1830. Col. 3, *annual precession.* Cols. 4, 5, 6, 7, the values of a', b', c', d'. Then there are two columns headed B and P, denoting the corresponding numbers in the catalogues of Bessel and Piazzi respectively; while the last column is reserved for those which are to be found in Hevelius, Lacaille, Mayer, Zach, &c.

There are several subsidiary tables which Mr. Baily also succinctly explains; and he further developes the principles of correction for *proper motion,* &c. when necessary.

The general rule for the use of the tables is this; viz. Take out from the general catalogue, and opposite the given star, the logarithms of a, b, c, d, and a', b', c', d', with their proper signs;

and from the subsidiary Tables I. and II. opposite the given day, the logs. of A, B, C, D, with their proper signs; which must be written down under the preceding logarithms: then add each pair A, *a*; B, *b*; &c. together; and take out respectively the natural numbers corresponding to the sum of the two logarithms; and observing that the signs only affect the resulting natural numbers, incorporate them by addition or subtraction accordingly; the amount will be the total correction required; that arising from *a*, *b*, *c*, *d*, being the correction in AR; that, from *a'*, *b'*, *c'*, *d'*, the correction in Declination.

The tables are arranged to *mean solar* time, which, it is presumed, will extend their utility. And it may be observed, that, by way of *artificial memory* to facilitate the recollection of the precise subject to which each column refers (as in B for Bessel, P for Piazza, already mentioned), Mr. Baily has made A B represent the quantity by which the A B *erration* is determined, C the quantity by which the *pre* C *ession* is determined, and D that by which the D *eviation* or *Nutation* is determined. These contrivances, though avowedly subordinate, will not be despised by those who know how much the pursuits of science are at all times promoted by the introduction of a happy technical mnemonics.

After the reading of this elaborate and interesting paper, the Society adjourned to Friday the 11th of November next.

Article XIII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Preparation of pure Potash.*

Mr. Donovan proposes the following as a more easy method of obtaining pure potash than the methods commonly employed.

The crystallized bicarbonate of potash of the shops is to be purified by dissolving it in water at the temperature of 100°. The saturated solution must be filtered and poured into a flat dish, and placed before the fire; in a few hours a crop of crystals of the pure bicarbonate will be obtained. The crystals may then be rinced with a very small quantity of water, and dried on blotting paper.

The crystals are now to be dissolved in water, and boiled with their own weight of hydrate of lime for 15 minutes; the solution is then to be filtered in the usual manner.* We have thus at

* In our next we shall give a description of the simple but ingenious apparatus invented by Mr. Donovan for filtering solutions out of contact with the atmosphere.—*Ed.*

once a solution of pure potash, without the additional trouble of evaporation and solution in alcohol; and we obviate the reconversion of the alkali into carbonate, which always happens during the evaporation in the common process, unless the tedious and troublesome method of evaporating without contact of the atmosphere be adopted, in which case silver vessels become necessary.

As a test to ascertain whether or not a solution of potash be perfectly caustic, chemists make use of a dilute acid; but this method gives no information unless the acid be added in excess. A small quantity will only displace the carbonic acid from one portion of potash; but the remaining portion will unite with the liberated acid so as to prevent any appearance of effervescence. Thus an alkali that is in fact partly carbonated will not be affected apparently by the affusion of a small quantity of a test acid.—(Dublin Philosophical Journal.)

2. *Account of Mr. Dalton's Process for determining the Value of Indigo.*

In order to find the value of any sample of indigo, Mr. Dalton directs us to take one grain carefully weighed from a mass finely pulverised. Put this into a wine glass, and drop two or three grains of concentrated sulphuric acid upon it. Having triturated them well, pour in water, and transfer the coloured liquid into a tall cylindrical jar, about one inch inside diameter. When the mixture is diluted with water, so as to show the flame of a candle through it, mix the liquid solution of oxymuriate of lime with it, agitating it slowly, and never putting any more in till the smell of the preceding portion has vanished. The liquid soon becomes transparent, and of a beautiful greenish yellow appearance. After the dross has subsided, the clear liquid may be passed off, and a little more water put into the sediment, with a few drops of oxymuriate of lime, and a drop of dilute sulphuric acid; if more yellow liquid is produced, it arises from particles of indigo which have escaped the action of the oxymuriate before, and must be added to the rest. The value of the indigo Mr. Dalton considers to be in proportion to the quantity of real oxymuriate of lime necessary to destroy its colour. He is of opinion also that the value may be well estimated by the quantity and intensity of the amber-coloured liquid which the indigo produces, which is found independently of any valuation of the oxymuriate of lime. The following results obtained with several samples show the great value of this method.

	Oxymuriate of lime used to destroy its colour.
Precipitated and sublimed indigo	140 grains
Flora indigo	70

Another sample	70 grains
Two other indigos	60
Two other samples.	50
Another sample	40
Another sample	30 or 35

Mr. Dalton is of opinion that to destroy indigo by oxymuriatic acid, twice the quantity of oxygen is necessary that is required to revive it from the lime solution. See *Manchester Memoirs*, New Series, vol. iv. p. 437, 438, 439.—(Edin. Jour. of Science.)

Mineralogy.

3. *On the Geological Situation of the Beryl, discovered in the County of Down.* By Sir Charles Giesecke.

This substance, which had been discovered some years ago in the county of Wicklow, in Ireland, in a coarse granular granite, has also been found lately in the county of Down, between Kilkeele and Newcastle, fifteen miles from Rostrevor, where it occurs in a coarse granular granite, which is more or less decomposed. It is very remarkable that this granite bears an extraordinary resemblance to the granite of Adontschelon, in Dauria, in which the beryls are found there. It is of a perfect crystalline structure, all its constituent parts presenting more or less perfect crystals. Those of rock crystal are the most distinct, and are generally of a brown colour, of different shades. The felspar is generally of a milk-white and yellowish-white, seldom of an ochre-yellow colour. Mica occurs only in small silver-white and greyish-white particles, and is wanting entirely in some parts of the rocks, particularly where the beryl is found in veins. The beryl itself occurs in a part of the Morne mountains, about three miles from the shore, partly in small veins, partly irregularly imbedded in the rock, and partly in detached and broken crystals in the sand of decomposed granite, and in the overlaying bog land.

Then follows a description of the beryl and its crystalline forms, which our limits oblige us to quote very briefly. Their colour is principally blue, of various shades—sometimes green, and pale wine-yellow. Some of the crystals present, on the end of their lateral edges, towards the terminating edges, perfect triangular delineations of a pearl-white colour, which have the appearance of a previous truncation of the terminating angles, filled up again by some process of nature. The largest crystals were from four to five inches long, and one inch in diameter. The form of the crystals is that of a six-sided prism, perfect, and variously truncated.

The rock crystal which accompanies the beryl is of different shades of brown, seldom of a greyish white, and yellowish-white colour.

The common felspar occurs of a milk-white, yellowish-white, and ochre-yellow colour. It exhibits sometimes a faintly opalescent play of colour similar to that of Adularia. It is found in a crystalline form, and in regular crystals.—(Dublin Philosophical Journal.)

4. *Description of Leveyne, a new Mineral Species.*

The following abstract is taken from Dr. Brewster's paper, in the Edinburgh Journal of Science for April, 1825.

The mineral, of which I propose to give a brief description, was kindly transmitted to me for examination about a year ago, by Mr. Heuland. In the memorandum which accompanied it, Mr. Heuland stated that he suspected it to be new, and upon examining its optical properties, and comparing it with those minerals with which it seemed to be most closely allied, I had no doubt that it constituted a new and interesting species.

This mineral occurs in the cavities of an amygdaloidal rock, from Dalsnypen, in Faroe, and sometimes accompanies the chabasie and analcime, but particularly a new variety of the heulandite.

Although this mineral is evidently a compound one from the distinctness of the re-entering angles; yet this composition is not seen when examined by polarised light, through the faces perpendicular to the axis. This circumstance would of itself have been sufficient to show that it has only one axis of *double refraction, but I determined this to be the case* by the direct examination of the polarised rings. Its double refraction is negative, like that of calcareous spar, and other obtuse rhomboids, and though not great, yet the images may be easily separated. Its ordinary refraction is a little greater than that of almond oil, and very nearly the same as that of primitive chabasie.

I have sent a specimen containing a few minute crystals of this substance to M. Berzelius for analysis; but I have not yet received the results which he has obtained from them.

It is not soluble in acids, nor does it gelatinise with them. It whitens and intumesces with heat like chabasie and mesotype, and, according to Mr. Haidinger's observations, it yields with salt of phosphorus a transparent globule, which contains a skeleton of silica, and becomes opaque on cooling.

Cleavage, indistinct. Fracture imperfect conchoidal.

Lustre vitreous. Colour white. Streak white. Semitransparent.

Brittle. Hardness = 4·0.

I propose to distinguish this species by the name of Leveyne, in compliment to Mr. A. Levy, M.A. of the University of Paris, who is already well known to mineralogists, by his crystallographic acquirements, and by his determination of several new and interesting mineral species.—(Edin. Jour. Science.)

*** We are obliged to omit Mr. Haidinger's crystallographic observations on Leveyne, as they cannot be well understood without a figure.—*Ed.*

MISCELLANEOUS.

5. *Astronomical Prize.*

At a Sitting of the Academy of Sciences of Paris on the 23d of May, the astronomical prize was unanimously adjudged to Mr. Herschel and Mr. South for their observations of 380 double and triple stars, communicated to the Royal Society of London, and by them published in their Transactions.

6. *Falling Star seen at Mid-day.*

On the 13th of August, 1823, at a quarter-past eleven in the forenoon, as I was employed in measuring the zenith distances of the pole-star to determine the latitude, a luminous body passed over the field of the universal instrument telescope, the light of which was somewhat greater than that of the pole-star. Its apparent motion was from below upwards; but as the telescope shows images in an inverted position, its real motion, like that of every falling body, was from above downwards. It passed over the telescope in the space of a second, or a second and a half, and its motion was neither perfectly equal nor rectilinear, but resembled very much the unequal and somewhat serpentine motion of an ascending rocket, from the unequal burning of the charge, and the irregular reaction of the stream of air issuing from it on the atmospheric air. It was thus evident that this meteor moved in our atmosphere, but it must have been at a considerable height, since its angular motion was so slow. This is, perhaps, the only instance in which a shooting star has been seen at mid-day in clear sunshine. *Hansteen.*—(Edin. Phil. Jour.)

7. *Notice regarding Copernicus.*

The name of this celebrated astronomer was written Koppernick; he was a canon and physician, and occupied himself in directing buildings. The aqueducts which he constructed at Graudenz, Thorn, and Dantzig, still exist. He took 24 years to produce his famous astronomical system, against which the thunders of the Vatican were hurled when the author was dead. The sentence of condemnation was only repealed at Rome in 1821; Copernicus died in 1543. The monument which Bishop Kromer erected to him in the cathedral of Frauenbourg no longer exists. Prussia claims Copernicus as one of her sons, although, at this period, Thorn did not belong to the Prussians. —(Edin. Phil. Jour.)

Article XIV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Travels in Brazil, Chili, Peru, and the Sandwich Islands, by F. G. Matheson, Esq.

The Mechanic's Common Place Book, by Olinthus Gregory, LL.D. of the Royal Military Academy, Woolwich. 1 vol. 8vo. with numerous Diagrams.

The English Flora, by Sir James E. Smith, Pres. Lin. Soc. Vol. 3.

Disquisitions on painted Greek Vases, and their Connexion with the Eleusinian and other Mysteries, by J. Christie.

JUST PUBLISHED.

Icones Fossilium Sectilium Centuria Prima. By C. G. König. Small folio. 10*s.* coloured; 7*s.* 6*d.* plain.

Elements of Operative Midwifery. By D. Davis, MD. Illustrated with numerous Plates. 4to. 2*l.* 2*s.*

Key to Nicholson's and Rowbotham's Algebra. 7*s.* 6*d.* boards. 8*s.* bound.

Holbroke on Hydrocele. 8vo. 4*s.* 6*d.*

A new Theory of Light. By W. Hunt. 2*s.* 6*d.*

Reid's Introduction to Chemistry. 2 vols. 12mo. 15*s.* 6*d.*

Ainslie on the Cholera Morbus of India. 3*s.* 6*d.*

The Dictionary of Mechanical Science, enriched with upwards of 100 Copper-plates and Cuts. By Dr. Jamieson. Part I. 5*s.*

Article XV.

NEW PATENTS.

W. Turner, Winslow, Chester, saddler, and W. Mosedale, Park-street, Grosvenor-square, coach-maker, for an improvement on collars for draft horses.—April 2.

R. W. Brandling, Low Gosforth, near Newcastle-upon-Tyne, for improvements in rail-roads, and carriages to be employed thereon and elsewhere.—April 12.

W. Shalders, Norwich, leather-cutter, for a gravitating expressing fountain for raising and conveying water or any other fluid for any purpose.—April 12.

W. Gilman, Whitechapel-road, engineer, and J. W. Sowerby, Birchin-lane, merchant, for improvements in generating steam, and on engines to be worked by steam or other elastic fluids.—April 13.

T. Sunderland, Croom's Hill Cottage, Blackheath, for a newcombination of fuel.—April 20.

C. Ogilvy, Verulam-buildings, Gray's Inn, for an improved apparatus for storing gas.—April 20.

J. Broomfield, Islington, near Birmingham, engineer, and J. Luckcock, Edgbaston, near Birmingham, for improvements in the machinery for propelling vessels.—April 20.

L. W. Wright, Wellclose-square, Middlesex, engineer, for improvements on apparatus for washing or bleaching of linens, cotton, &c.—April 20.

A. L. Hunout, Brewer-street, Golden-square, for improvements in artillery, musquetry, and other fire-arms.—April 23.

T. A. Roberts, Monford-place, Kennington-green, for a method of preserving potatoes, and other vegetables.—April 23.

S. Ryder, Gower-place, Euston-square, coach-maker, for an improvement in carriages, by affixing the pole to the carriage by new invented apparatus.—April 28.

D. Dunn, King's-row, Pentonville, for an improved apparatus, for the purpose of beneficially separating the infusion of tea or coffee from its dregs.—April 30.

W. Davis, Leeds, engineer, for improvements in machinery for reducing or converting wool into slivers or threads, of any desired length, unlike worsted, namely, presenting more numerous hair points projecting from the surface of the slivers or threads.—May 7.

T. Hill, the younger, Ashton-under-Line, land surveyor and engineer, for improvements in the construction of railways and tram-roads, and in carriages to be used thereon, and on other roads.—May 10.

E. Elliss, Crexton, near Rochester, lime merchant, for an improved brick, or substitute for brick.—May 14.

S. Pratt, New Bond-street, camp equipage manufacturer, for an improved manner of combining wood and metal so as to form rails or rods adapted to the manufacture of bedsteads, cornices, and other works, where strength and lightness are desirable.—May 14.

J. C. C. Raddatz, Salisbury-square, Fleet-street, merchant, for improvements in steam-engines.—May 14.

J. F. Gravier, Cannon-street, merchant, for a method of regulating the emission or flow of gas from portable reservoirs, and of increasing the safety of such reservoirs.—May 14.

T. Pyke, Broadway, near Ilminster, for an apparatus to prevent the overturning or falling of carriages.—May 14.

A. Galloway, West-street, engineer, for a machine for the forming and moulding of bricks and other bodies usually made from clay, plastic, or any of the usual materials from which building and fire bricks are commonly made.—May 14.

W. Grimble, Cow-cross-street, Middlesex, for improvements in the construction of apparatus for distilling spirituous liquors.—May 14.

E. Garsed, Leeds, flax-spinner, for improvements in a machine for hacking, combing, or dressing flax, hemp, and other fibrous materials.—May 14.

H. O. Weatherley, Queen Anne-street, Saint Mary-le-bone, for a machine for the purpose of splitting, or cleaving of wood, and forming and securing the same in bundles.—May 14.

G. Gurney, Argyle-street, Hanover-square, surgeon, for an apparatus for propelling carriages on common roads or on railways.— May 14.

J. Young, Wolverhampton, cooper, for improvements in the construction of locks for doors, and other purposes.—May 14.

ARTICLE XVI.

METEOROLOGICAL TABLE.

1825.	Wind.	Barometer. Max.	Min.	Thermometer. Max.	Min.	Evap.	Rain.
5th Mon.							
May 1	S	29·88	29·82	64	44	—	85
2	S	29·86	29·82	62	46	—	12
3	S W	30·08	29·86	61	48	—	
4	S W	30·08	29·98	71	57	—	15
5	E	29·98	29·95	69	52	—	1
6	S	29·95	29·94	74	52	—	
7	S W	30·04	29·95	74	50	·94	
8	S	30·18	30·04	70	46	—	—
9	W	30·19	30·18	70	50	—	
10	N W	30·19	30·14	70	52	—	
11	E	30·14	30·02	70	50	—	9
12	E	30·04	30·02	64	50	—	1·70
13	E	30·29	30·04	52	40	—	8
14	N	30·35	30·29	60	36	—	
15	N E	30·34	30·21	56	42	—	
16	N E	30·28	30·21	60	42	—	
17	N E	30·41	30·28	58	54	·95	
18	N E	30·41	30·39	65	40	—	
19	N E	30·39	30·38	55	34	—	
20	E	30·38	30·31	58	38	—	
21	E	30·31	30·23	69	37	—	
22	E	30·23	30·11	70	45	—	
23	W	30·11	29·92	80	50	·94	
24	W	29·92	29·80	70	50	—	
25	S W	29·82	29·80	69	51	—	
26	N	29·88	29·81	63	44	—	23
27	N	30·02	29·88	58	36	—	
28	S W	30·05	30·02	60	36	—	22
29	S W	30·17	30·05	64	39	—	
30	N E	30·46	30·17	62	32	—	
31	N E	30·50	30·46	64	32	·94	
		30·50	29·80	80	32	3·97	3·45

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Fifth Month.—1. Fine day: rainy night. 2. Showery. 3. Fine. 4. Fine: some lightning in the evening. 5. A heavy shower about eight, a. m. 6—11. Fine. 12. Rainy. 13. Some rain, a. m.: fine, p. m. 14—23. Fine. 24. Cloudy. 25. Fine. 26. Cloudy: rainy evening. 27. Fine. 28. Showery. 29—31. Fine.

RESULTS.

Winds: N, 3; NE, 7; E, 7; S, 4; SW, 6; W, 3; NW, 1.

Barometer: Mean height

For the month	30·113 inches.
For the lunar period, ending the 9th	29·999
For 13 days, ending the 11th (moon south)	29·984
For 14 days, ending the 25th (moon north)	30·188

Thermometer: Mean height

For the month	54·741°
For the lunar period, ending the 9th.	53·310
For 31 days, the sun in Taurus	54·096

Evaporation	3·97 in.
Rain	3·45
By a second guage	3·54

*** At Tottenham, on the 30th, about six, p. m. a smart shower of rain, preceded by large hail, very well indicated the near approach of the cold current, by which the temperature on the following nights was lowered to the freezing point.

Laboratory, Stratford, Sixth Month, 21, 1825. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

AUGUST, 1825.

Article I.

Explanation of the Theory of the Barometrical Measurement of Heights. By Mr. Nixon.

(*Continued from* p. 52.)

Of the Ratio of Density of dry Air to Mercury.

In possession of formulæ enabling us to calculate the density of dry air as varying from pressure and temperature, we have to ascertain in the next place the ratio of its specific gravity at any given temperature compared to that of the liquid of the barometer indicating the pressure. From the numerous experiments of MM. Arago and Biot made (at Paris, at an elevation of about 200 feet above the level of the sea) on moist air of various temperatures, it is inferred that 12,000 cubic feet of perfectly *dry* air, of the temperature of 32° F. supporting at the level of the sea, in latitude 45°, a pressure of 26·0988 inches, would be equal in weight to 1 cubic foot of the liquid of the barometer. Consequently a column of the dry air, if uniformly dense, and of the vertical height of 12,000 inches, or 1000 feet, would exactly counterpoise a column of the liquid, of the same base, *one* inch in height.*

* Whatever the specific gravity of the mercury (or other liquid) of the barometer, 12,000 measures of the dry air would equal in weight one measure of the same liquid as that contained in the barometer; the liquid in the instrument and in the scales being of the same temperature.

Repeating the experiment with water substituted for mercury, and specifically lighter in the ratio of 13 to 1, the barometer would now exhibit a pressure of 13 times 26·0988 inches, or 339·2844 inches, and one-thirteenth part of 12,000 cubic feet, or 923 + cubic feet of the air, would counterpoise one of water. Yet as the densities of dry air are directly as the pressures, we should infer that when the water barometer (carried to the requisite altitude) stood at 26·0988 inches, then would the density of the air there be diminished one-thirteenth, and that 12,000 measures of it would be required to balance one of water.

The value of the inch or foot being lost might be easily regained by ascertaining how many measures of dry air equalled in weight one measure of the mercury of the barometer. Then dividing 313185·6 inches (= 26·0988 × 12·000) by the number of measures (or ratio of the specific gravity of the mercury to that of the air) we should have

Method of calculating Heights.

We may now proceed to the calculation of the vertical height of an object situated within the atmosphere of the earth, the data being the height of the barometer at its summit and base, together with the temperature of the intercepted stratum of air (uniformly at 32° F. and perfectly dry).

To reduce the problem to the greater simplicity, we remark in the first place, that it is immaterial whether the instruments are placed in the same vertical line or not, for every point of the surface of the atmosphere of uniform temperature being at the same distance from the centre of the earth,* and the pressures being directly as the depths below the surface of the fluid, the heights indicated by two or more barometers equidistant from the earth's centre will be precisely the same without regard to their horizontal distance. Secondly, as the pressure exerted by fluids is uninfluenced by their figure, it is unnecessary to have regard to the area of the strata of the atmosphere, increasing (but not in the simple ratio of the height) as we ascend from the surface of the earth.† Thirdly, as the height of the upper barometer exhibits the value of the pressure incumbent on the intercepted stratum of air, and thus affords the datum requisite to ascertain its mean density as far as regards pressure, it would be superfluous even to inquire *what* is the fluid exerting that pressure. Lastly, as the absolute pressure exerted by a fluid is directly as the height multiplied by its mean specific gravity, if we multiply the difference of the heights of the barometer at the two stations by the ratio of the mean specific gravity of the equiponderant intercepted column of air to that of the mercury, we shall have the vertical height of that column; equal to the difference of level of the base and summit of the object.

The heights (or volumes) of equal weights of dry air being reciprocally as the pressures they sustain, and as every stratum of the atmosphere supports the total pressure of those above it, it has been demonstrated that when the altitudes above the lowest station increase in arithmetical progression, the heights of the mercury in the barometer decrease in geometrical progression. Such being the case, it is evident that the difference of some two consecutive terms of the geometrical series will be equal to, or coincide with the difference of any two consecutive terms of the arithmetical one; and that where this equality of differences takes place, the density of the air there will be equal to the mean density of the whole. Or, supposing the density of the column

the height in *inches* of the barometer (if of the syphon construction with branches of equal diameter), without regard to the specific gravity of the mercury.

It is almost superfluous to remark, that at ordinary temperatures the liquid of the barometer should not generate elastic vapours, or the height will be observed in defect; the depression increasing (unequally) with the temperature.

* See § 5, p. 435. † See § 6, p. 435.

of mercury to be equal to the *mean* density of the equiponderant column of air (a supposition which will not at all affect the result), if we divide the two columns, whose heights will now be equal, into the same number of exceedingly small strata, weighing alike, then will some one or other of the strata of the air, varying in depth, be sensibly of the same thickness, or vertical height, as any one of the uniformly deep strata of the mercury. The density of this stratum is evidently equal to the mean density of the column of air, and knowing the number of the strata it is distant from the summit of the column of mercury, we easily ascertain the pressure it sustains by adding the sum of the equal weights of these strata to the height of the barometer at the upper station. With the assistance of a table of logarithms, together with its *modulus*, the mathematician would readily determine that the pressure supported by a volume of air uniformly of a density equal to the mean density of a stratum of dry air intercepted by the pressures of 15·5 inches and 30·5 inches would be 22·1602 inches, and that the height of the object, or of the column of air at 32° F. would be equal to 30·5 *minus* 15·5 = 15 inches of mercury multiplied by $\frac{26\cdot0988}{22\cdot1602}$ of 12,000 inches, or to 17,666 feet. Had the pressure expressive of the mean density been 26·0988 inches, the length of the column of air would have been 12,000 times 15 inches, or 15,000 feet; but as the heights are inversely as the pressures, its altitude must be increased in the ratio of 22·1602 to 26·0988.

But we may calculate the altitude of the object by a process more generally intelligible, and which will have the advantage of demonstrating that the pressures or heights of a barometer carried successively to a series of stations uniformly increasing in their distance from the surface of the earth, will decrease in geometrical progression. Our plan will be to divide the column of *mercury* into a sufficient number of equal parts or heights, to which we must affix the corresponding intercepting pressures. The columns not exceeding one inch in height, the mean pressure, or half the sum of the pressures at the base and the summit, will not materially differ from (exceed) the pressure supported by a stratum of air of which the density, supposed to be uniform, is equal to the mean density of the column of air counterpoising that inch of mercury. As the altitude of a stratum of dry air under the pressure of 26·0988 inches counterpoising an inch of mercury is equal to 1000 feet, and as the heights are inversely as the pressures, we ascertain the altitude in feet corresponding to the different columns of mercury by dividing the constant number 26098·8* by the respective mean pressures. The sum of these altitudes will be equal to the altitude of the object.

* For air containing its mean quantity of moisture, and to include some corrections for gravity, substitute 26210.

(The degree of accuracy of the method increasing with the *number* of the subdivided columns, we may convince ourselves by repeated calculations that it is superfluous to divide the column into lengths less than one inch.)

Example.—Upper barometer 15·5 inches; lower barometer 30·5 inches.

Columns of mercury 1 inch in height between the pressures of (Inches)		Mean	Altitudes of the equiponderant columns of dry air at 32° F. (Feet)	Altitudes above the base of the column (Feet)
29·5 and	30·5	Mean 30	870·0	870·0
28·5	29·5	29	900·0	1770·0
27·5	28·5	28	932·1	2702·1
26·5	27·5	27	966·6	3668·7
25·5	26·5	26	1003·8	4672·5
24·5	25·5	25	1044·0	5716·5
23·5	24·5	24	1087·5	6804·0
22·5	23·5	23	1134·7	7938·7
21·5	22·5	22	1186·3	9125·0
20·5	21·5	21	1242·8	10367·8
19·5	20·5	20	1304·9	11672·7
18·5	19·5	19	1373·6	13046·3
17·5	18·5	18	1450·0	14496·3
16·5	17·5	17	1535·2	16031·5
15·5	16·5	16	1631·2	17662·7
			17662·7	

(Correct altitude 17666·0; Error — 3·3 feet.)

Dividing the stratum of air into any number of sections of equal altitude, we may find by a little further calculation the pressures they sustain, and demonstrate the decreasing geometric progression of the latter; for if we take any two consecutive pressures and divide the consequent by the antecedent one, and any other two consecutive pressures be taken and the consequent of these be divided by their antecedent, the two quotients (or ratios), will be found to be equal to each other, and less than unity, which is the characteristic of a descending geometrical series.

Example.

	Feet.		quotients or ratios.
Pressure at the base . .	—	30·5000 in.	
at an altitude of	4416·5	25·7518	·84432 (nearly)
————————	8833·0	21·7428	·84432
————————	13249·5	18·3579	·84432
————————	17666·0	15·5000	·84432

Having found the altitude of the object, the temperature of the air being 32°, let us now suppose the temperature to have been 80°, or 48 degrees above the freezing point. The volume of the dry air is consequently increased $\frac{48}{480}$, or $\frac{1}{10}$th, and its density, inversely as the volume, diminished in the ratio of 1 to 0·90909.* The given heights of the barometer being, as in the preceding example, 30·5 and 15·5 inches, we must augment the altitude at 32°, or 17666 feet, one-tenth, or in the ratio of 90909 to 100000; equal either way to 19432·6 feet. At this temperature ($12000 + \frac{1}{10}$th or) 13200 cubic feet of dry air supporting the pressure of 26·0988 inches, would be required to balance one of mercury.

The temperature of the air being 24 degrees below 32° F. or at + 8° F. we must reduce the altitude at 32° one-twentieth, or in the ratio of the densities,† viz. of 1·05263 to 1, giving 16782·7 feet as the correct altitude of the object.

We have hitherto supposed the stratum of air to be of uniform temperature; a supposition seldom or ever confirmed by the indications of the detached or exterior thermometers placed in the shade, and freely exposed to the air at the summit and base. Generally speaking, the temperature, especially when the difference of altitude is considerable, diminishes as we ascend. The mean rate of the diminution, which is extremely variable at different times and places, is usually estimated at 1° F. for an elevation of 300 feet. My own numerous observations on altitudes not exceeding 2000 feet, give 230 feet as the mean—a rate of decrement differing little from that deduced from the observations of Mr. Dalton; but as the diminution when within 100 or 200 feet of the summit, particularly when the mountain was an extended ridge, and the thermometers were placed on its leeward side, was frequently double or treble that of the inferior equal sections, the rate is evidently exaggerated, and some correction seems necessary in order to obtain a uniform decrement from observations made on mountains of different altitudes.

Supposing the decrement, whatever the rate, to be but uniform, or in arithmetical progression; that is, granting the differences of temperature indicated by any number of thermometers ranged on the side of the mountain at equal perpendicular distances to be the same, we shall commit no sensible error in the calculation if we consider the mean density of the air the same as that of another stratum *uniformly* of a temperature equal to half the sum of the detached thermometers observed at the base and summit of the mountain.‡

* See § 20, p. 438. † Ibid.

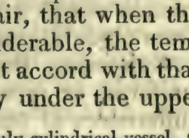
‡ If we expose 440 volumes of air 48 degrees *above*, and 440 volumes 48 degrees *below* the freezing point, to the mean temperature (equal to 32° F.), their volume collectively will be greater by 1-100th part than the sum of their volumes before the mixture took place.

For 400·0 volumes at	32° are equal to	440 volumes at	+ 80°
And 488·9	32	440	− 16
888·9		880	Mean 32

It is to be remarked that any one or more sections of a cylindrical column of air, uniformly of a given temperature, may be heated or cooled without disturbing in the least the pressure of the strata incumbent on that section, or of those on which it rests.* That the density of the lower strata of the atmosphere is occasionally inferior (especially in the middle of a cloudless day declining rapidly in temperature towards morning and evening), to that of the strata immediately superincumbent is proved by the terrestrial refractions being frequently negative when the intercepted arc is inconsiderable.

If we admit the progression of the diminution of temperature as we ascend to be geometrical, and of such a nature that the decrement for the same number of feet is greater at the base than at the summit, the column will evidently contain a greater quantity of air *inferior* than *superior* in temperature to the mean of the detached thermometers at the extremes, and half their sum exceeding the true mean temperature, will introduce an error in excess in the calculated altitude of the object. Still as we have no experiments authorising us to conclude that the diminution of temperature is in geometrical progression; as we are even ignorant whether the rate be greater at the base or summit, and, what is more to the point, being well aware that numerous local and other causes will disturb and render its nature undistinguishable, we may be spared the trouble of calculating any corrections, and content ourselves with considering the mean of the thermometers at the extremes as the proper temperature for calculation.† We cannot, however, too strongly insist that the principal errors of barometrical measurements are the result of an incorrect estimate of the mean temperature. It has also been justly observed by Prof. Playfair, that when the horizontal distance of the barometers is considerable, the temperature of the air at the lower station may not accord with that at the base of the column of air immediately under the upper

* If we increase the temperature of a fluid, confined in a truly cylindrical vessel, to such a degree as to double the volume, the height will be *increased*, and the density *diminished*, in the same ratio, and the pressure at the base will remain unaltered. But if the figure of the vessel be that of the frustum of an inverted pyramid, then will the increment of volume be insufficient, on account of the increasing capacity of the vessel upwards, to augment the depth of the fluid in the same ratio. The absolute pressure being as the height multiplied by the density, there is a consequent *loss* of pressure. Hence it must be admitted that an increase in any ratio of the volume of the entire atmosphere (resulting from an elevation of its temperature throughout its mass) will not cause, on account of its spherical figure, an augmentation of its height in the same proportion; and that the pressures at the base of an atmosphere uniformly according in temperature with our climate would be less in summer and at noon, than in winter and at night. This view of the subject, if correct, may serve to account for the horary oscillations of the barometer.

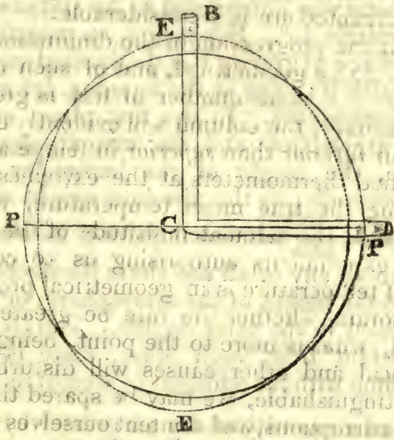


† Dr. Horsley considers the method of taking the mean of the temperatures at the extremes as only an approximation, yet sufficiently exact.—(Phil. Trans. vol. lxiv.)

barometer—a circumstance that cannot fail to vitiate the calculation of the altitude.

Correction for Latitude.

Hitherto we have considered the earth as a sphere at rest, but if we admit that it revolves on its axis, then would its figure, supposing it to have been originally a fluid mass, become that of a spheroid flattened at the poles, and protuberant at the equator. If we conceive the barometer B to be so placed



within the earth that the one branch shall extend from its surface at the equator EE to the centre, and that the other shall protrude to the surface at either pole PP, then will the density of the mercury within the branch at the equator, diminished from the centrifugal force there, be inferior to its density within the branch at the pole. The latter will consequently press on the fluid within the rotating branch, and cause it to ascend until its superior height, compensating its inferior density, produces an equilibrium of pressure between the two columns; their heights now being reciprocally as their mean densities.

The surface of the earth being no longer everywhere equidistant from the centre, we must now define the difference of level of two points not situated in the same vertical line as being equal to the difference of their vertical distances from the surface of the earth, or rather that of the ocean, conforming in figure to that of a spheroid.

The centrifugal force increasing with the length of the radius of rotation, and the intensity of gravity diminishing as the distance from the centre of the earth, it follows that the force of the latter is least at the equator, and that the variation will diminish as we approach the poles in the ratio of the distance of the surface of the earth at any latitude from the nearest point of the axis. Thus it has been demonstrated that if we call the force of gravity in latitude 45° equal to unity, then will its intensity at the equator be diminished $\frac{10}{3515}$: at the poles it will be augmented in the same proportion. For any intermediate latitude, the fraction must be multiplied by cosine of *double* the latitude.

In stating the number of measures of dry air required to counterpoise one of mercury, we remarked that the barometer indicating the pressure was understood to be stationed at the level of the sea in latitude 45°. Supposing, on repeating the experiment, that the force of gravity has diminished in the interim so as to coincide with its value at the equator, let us inquire what will be the consequences. In the first place, as the *absolute* weights of the atmosphere and the mercury are diminished in the same ratio, the height of the barometer will continue unchanged. Secondly, as the absolute weight of each particle of air is diminished $\frac{10}{3515}$ without impairing its elastic force, the original pressure on any or all the strata is enfeebled in the same proportion, and the height of the atmosphere, or of any portion of it, is increased in the same degree. Thirdly, the particles being more distant from each other, there is a consequent increase of volume without addition of weight relative to that of the mercury, and a quantity of dry air greater by $\frac{10}{3515}$ than had been required in the previous experiment, will be necessary to balance the measure of mercury. Had we hermetically closed the vessel containing the air in latitude 45°, and then transported it to the equator, it would still be found to be an exact counterpoise to a volume of mercury equal to that made use of in the former latitude; but on opening the vessel and allowing the included air to communicate with the atmosphere (the barometer there standing at the same height as when at the pole), then would the air, being of an elasticity superior to the *absolute* pressure now incumbent on it, immediately expand, and a portion of it rushing out, the residue would be insufficient to balance the incompressible mercury.

The better to illustrate the variation of the density of air resulting from change of latitude, place a proper weight on a vertical column of elastic wire coiled in the manner of a screw; then if we conceive them to be transported to a lower latitude, the weight will press on the spring with diminished gravity, and the height of the column will be increased.

The calculations being made in the first instance for the latitude of 45°, we must *increase* the altitudes in latitudes nearer the equator, and *diminish* those in parallels approaching the poles, in the ratios proper for the respective latitudes as calculated by the rules just given.* This correction, properly the very last in order, may be readily effected by the fractions given by Biot, in the third volume of his *Traité d'Astronomie*, or more promptly by the annexed table, differing some little from the one by Prof. Littrow, inserted in the Memoirs of the London Astronomical Society.†

Correction for the vertical Diminution of the Force of Gravity.

For the Air.—The force of gravity diminishing as, ascending from the surface, we recede from the centre of the earth as the square of the distance, it is evident that the density of the air at different altitudes will diminish, *cæteris paribus*, in the same ratio. The mean radius of the earth being about 3956 miles, if we call the force of gravity at the level of the sea 1, its value at the several altitudes of

	1,	2,	3,	4 miles
will be equal to 1 × square of	$\frac{3956}{3957}$,	$\frac{3956}{3958}$,	$\frac{3956}{3959}$,	$\frac{3956}{3960}$,
for which we may be allowed to substitute 1 *minus*	$\frac{2}{3956}$,	$\frac{4}{3956}$,	$\frac{6}{3956}$,	$\frac{8}{3956}$:

consequently we must augment the calculated altitudes in proportion to the mean diminution of the density of the intercepted column of air, nearly equal to half the sum of the values of the diminution at the extremities; effected by applying a correction additive found by multiplying the difference of level in feet by the *sum* of the perpendicular distances of the summit and base of the object from the surface of the earth, and dividing the product by 20887680, the mean radius of the earth in feet. The amount of the correction for an elevation of 8000 feet above the sea, being only three feet, it may be safely disregarded within that limit. At greater elevations the value may be readily found by Table IV.

For the Mercury.—Having proceeded in our calculations on the supposition of the force of gravity, as affecting the mercury being constant without regard to the altitude, we must now be conscious that the absolute pressure exerted by the upper strata of the atmosphere on the subjacent intercepted column of air, is no longer correctly exhibited by a barometer situated above the level of the sea. The specific gravity of the liquid as transported

* Admitting the verticals to have the same latitude.

† The upper part of that branch of the syphon barometer containing the shorter column of mercury being filled at the pole with dry air of a certain temperature, and hermetically closed, note the difference of level of the mercury in the two branches. Then in proceeding towards the equator, if we do not change our distance from the surface of the sea, and preserve the temperature of the air and mercury unaltered, the length of the column of air, and the difference of level of the mercury, will continue to augment until we arrive at the equator. The instrument would, therefore, serve to find the latitude.

to different distances from the centre of the earth being diminished in the ratio of the square of those distances, its height in the barometer, when counterpoising the same absolute pressure, or such as would be indicated by a barometer at the level of the sea, will be proportionally augmented. To comprehend the nature of the correction (additive), we remark in the first place, that as it is unnecessary to be acquainted with the specific gravity of the liquid of the barometers,* if alike in both, we have merely to correct the observed height of the column of mercury in *either* barometer in the inverse ratio of their specific gravities. Further, as we may be suffered to express the ratio of the diminution of the force of gravity at the several altitudes above the level of the sea of 1, 2, 3, and 4 miles by the fractions $\frac{1}{1978}$, $\frac{2}{1978}$, $\frac{3}{1978}$, and $\frac{4}{1978}$, increasing uniformly with the altitude, and having equal differences, it is evident that whatever the elevation of the lower barometer above the same level, the observed height of the other, if situated a mile above it, must

* This is sufficiently established by our previous demonstration that a certain number of cubic feet of dry air will balance one of the liquid of the barometer, without regard to its specific gravity, when the observed height is equal to a certain number of inches. However, as Professor Robison asserts, in his Elements of Mechanical Philosophy, that when the mercury of the barometers is not of the same specific gravity as that made use of in the experiments of Sir G. Shuckburgh, calculations conformable to his formula cannot fail to be erroneous, we must so confirm our opposite statement on this important subject as to leave no room for doubt.

If we augment or diminish in the same ratio any two terms of a series of numbers in geometrical progression, the differences of the corresponding terms of the arithmetical will continue the same. It must, therefore, follow, that as the pressures are in geometrical, and the altitudes in arithmetical progression; and as the heights of two columns of mercury supporting the same atmospheric pressure will be constantly, during every variation of the pressure, in the inverse ratio of their specific gravities, the altitudes as computed from one and the same formula will be alike, without regard to the density of the mercury.

We have already found that when the pressures were 15·5 and 30·5 inches, the altitude would be 17666 feet. Supposing we had also observed the pressures with two other barometers containing mercury of a density inferior in the ratio of 11 to 10, the heights would have been noted at 15·5 + $\frac{1}{10}$th, and 30·5 + $\frac{1}{10}$th, or at 17·05 and 33·55 inches. The pressure corresponding to the mean density of the air, computed precisely as before, will be found to be 24·3763 inches, and the altitude equal to 16·5 inches (the difference of the observed heights, or length of the column of mercury balancing that of the air) multiplied by $\frac{26\cdot0988}{24\cdot3763}$ of 12,000 inches, or to 17,666 feet, the same as before. The pressure incumbent on the stratum of air as indicated by the barometers with the rarer liquid, would appear too great by one-tenth, and we might have anticipated (the heights being reciprocally as the pressures) that the altitude would be calculated proportionally in defect; but as the length of the column of mercury counterpoising the air (16·5 inches) is from its inferior specific gravity one-tenth greater than the equiponderant column of mercury in the other barometers (15 inches), the two causes of difference prove to be of an opposite tendency, and equal in value.

An extremely useful alteration in the construction of the Englefield barometer, depending on this contested point, will be proposed when we come to treat of the instruments.

be reduced $\frac{1}{1978}$. Lastly, as the weights of equal (volumes or) heights of air, if so small as to be sensibly of uniform density, are directly as the pressures they sustain, the value of the correction in feet, the mean temperature of the air being 32° F. and containing its usual quantity of moisture, will be constant, and equal to 13·25 feet.

Having proved the correction to be directly in the ratio of the altitude without regard to the elevations of the barometers above the level of the sea, and equal at a temperature of the air of 32° F. to 13·25 ft. per mile, or to $\frac{13\cdot25}{5280\cdot00}$, we may strictly dispense with any reduction of the observed heights of the barometers, or subsequent augmentation of the approximate altitude, if we make our calculations in the first instance on the supposition that $1000 + \frac{1325}{528000}$ feet, or more correctly $1000 + \frac{10}{3984}$ $= 1002\cdot51$ vertical feet of dry air at 32° F. and under the pressure of 26·0988 inches, will counterpoise *one* inch of mercury in the latitude of 45°, or more conveniently if we admit that the inch of mercury will balance 1000 feet of the dry air under a pressure of 26·1643 inches.

When the mean temperature differs from 32° F. the value in feet of $\frac{1}{1978}$ of the height of the barometer will vary, and we must augment (or diminish) the 13·25 feet at the rate of $\frac{1}{480}$ per degree of the difference. To meet this slight correction, we have but to consider the dilatation in volume of dry air as $\frac{1}{479}$ in lieu of $\frac{1}{480}$ per degree.

To prove that the view we have taken of the subject is quite correct, a table is subjoined of the value of the correction at different elevations, and temperatures of the air, strictly calculated on the supposition of the lower barometer being placed at the level of the sea.

Elevations of the Upper Barometer above the Lower one placed at the Level of the Sea.

	Temperature of the Air.				
	0°	32°	52°	72°	92°
1 mile	12·3	13·2	13·9	14·4	15·0 feet.
2	24·6	26·5	27·7	28·8	30·0
3	36·9	39·8	41·6	43·3	45·0
4	49·2	53·0	55·4	57·7	60·0

Gravity diminishing as we ascend above the surface of the earth, it follows that a pendulum clock taken from the level of the sea to the summit of a mountain would have its rate of going retarded. Some experiments of this nature undertaken in

Faussigny with a view to determine the vertical difference of level of the stations proved quite unsuccessful. The probable reason is, that the stations differed materially in latitude, so much so, and in such a manner, that the correction for the latter, had it been attended to, would have exceeded the other, and in an opposite direction.*

Correction for the Attraction of Mountains.

When we consider that the surface of the earth (restricting the term to that part of it coinciding in level with the sea), is generally covered, especially in the scene of barometrical observations, with ponderous mountains, and that the correction for the diminished gravity of the mercury, although reduced one-half, is *subtractive*, and the one for the air (still additive) merely half the amount for *depths* (or differences of level), below the surface, we must be sensible that barometrical measurements made in the midst of a mountainous country must tend to err in excess.

From the attraction of the steep acclivities forming a narrow valley, &c. the particles of air therein are more numerous than in a similar volume of air under the same pressure, &c. taken from a situation unaffected by local attraction. Now as this accession of density does not sensibly extend to the strata of the atmosphere incumbent on those within the valley, the density of the latter is too great for the pressure, and the same depth of air requires a greater column of mercury to form a counterpoise. The error in excess in small differences of level measured in similar situations is incredibly great, as I have had frequent opportunities of verifying by a comparison of the levelled and barometrical altitudes.

When the lower station is an extensive plain, and the upper barometer is placed at the bottom of a deep ravine, or gorge, the increase of the density of the air therein causes the mercury to stand too high, and the computed altitude must fall short of the truth.

Correction for the Difference of Temperature of the Mercury of the Barometers.

Mercury expands with increase of temperature: according to the approved experiments of MM. Dulong and Petit, its volume at 32° F. is altered $\frac{1}{9990}$ for a variation of temperature equal to 1° F. The densities of equal weights of fluids being reciprocally as the volumes, and their pressures equal to their heights mul-

* At the level of the sea, water boils at 212° under a pressure of 30 inches of mercury, but would not the ebullition at the same temperature take place at an elevation of four miles, although the barometer stood there at 30 inches $+ \frac{4}{1978}$?

tiplied by their mean densities,* it follows that the heights of two barometers sustaining the same atmospheric pressure, but exposed to different temperatures, will not coincide; the column of the one having the inferior temperature being shorter than the other in proportion to the increase of density. The observed heights will, therefore, require to be reduced to their value at 32° F. or simply, as the specific gravity of the mercury, if the same in both instruments does not affect the calculation, the length of the one column must be reduced to its height at the temperature of the other. But this reduction, as it supposes the scale of inches, generally of brass, to be unaffected by change of temperature, is of course overrated; were the linear dilatation of the scale equal to that of mercury in volume, the heights of the barometer would not in fact require a correction for difference of temperature. The fraction expressing the reduction must consequently be equal to the expansion of mercury *minus* that of brass, or to $\frac{1}{11153}$ per degree from 32° F.†

To construct a table enabling us to correct the altitude computed from the observed pressures, we have but to find as a basis the value in feet of the correction for a difference of temperature of the barometers equal to one degree, the air being at 32°. Whatever the heights of the columns, provided we reduce them all in the same ratio, or of $\left(\frac{1}{11153} + \frac{1}{11153} \text{ of } 11153 = \right)\frac{1}{11154}$,‡ the heights of the columns of air at 32° F. balancing the *minute* columns of mercury intercepted by the observed and corrected heights will be sensibly equal. The 1000th part of 30 inches is 0·030 in. and the same proportion of 15 inches is but half the quantity, yet as the former supports double the pressure of the latter, its density is greater in the same ratio, and the heights, inversely as the densities, are alike. Under a pressure of 26·208 inches, one vertical foot of air at 32° F. half saturated

* It is quite superfluous to remark, that the increased diameter of the column of mercury from the dilatation of the glass of the tubes does not interfere with the equilibrium of pressure of the atmosphere and the mercury, and must not be regarded.

† Expansion of mercury (in volume)	1	:	1·00010010
Ditto brass (linear)............................	1	:	1·00001044
Difference $\frac{1}{11153}$, or as	1	:	1·0008966

(See a valuable paper on the Expansion of Metals, &c. by the (now) President of the London Astronomical Society, inserted in the second volume of their Memoirs.)

‡ *Rule.*—To reduce the observed height to its length at 32° F. multiply it by the number of degrees of the attached thermometers above 32°, and divide the product by 11153 *plus* the multiplier. The temperature being below the freezing point, multiply by the number of degrees below 32°, and divide the product by 11153 *minus* the multiplier.

For the stationary barometer, generally above 32° in these climates, we might apply a constant divisor that would cause the least errors between 32° and 82°. It would of course exceed the arithmetical mean of 11153 and 11203, the divisors at those temperatures.

with moisture counterpoises 0·001 in. of mercury, allowance being made for the diminution of gravity in the vertical line. Dividing 26208 by $\frac{1}{11154}$, we have 2·35, the correction in feet for one degree of difference of the attached or interior thermometers of the two barometers, subtractive from the calculated height, when, as is generally the case, the instrument at the upper station is inferior in temperature to the other. Substituting $\frac{1}{450}$ for $\frac{1}{480}$ as the mean rate of expansion per degree of humid air, we alter the 2·35 feet in conformity for other temperatures of the air.

The mean value of forty-one degrees of difference of the interior thermometers (the mean of the detached ones being 50°), being equal to 100 feet, were the artist to divide *forty-one degrees* of the scale into 100 equal parts, making the zero of the new scale to correspond with 0° F. and numbering the divisions upwards to designate them as *feet*, we should note at the two stations the number of feet opposite the summit of the column of mercury in the thermometer in lieu of the degrees, and deduct their difference (the upper barometer being coldest), from the altitude calculated with the observed pressures.* When the difference of level of the stations is great, the two detached thermometers may differ considerably, and some little correction should in strictness be applied when the mean temperature of the air differs from 50°. The difficulty of ascertaining the mean temperature of the mercury is, however, so great, and the value of its dilatation so variously given by different experimenters, that it would be mere affectation of exactness to regard it. Its value with the proper signs affixed is subjoined.

Difference of the detached Thermometers.

	Mean Temperature of the Air.			
	+12°	32°	72°	92°
10°	−1·9	−0·9	+1·2	+2·2 feet.
20	−4·0	−1·9	+2·3	+4·4
30	−6·0	−2·9	+3·4	
40	−8·0	−3·9	+4·6	

Deluc imagined that it was necessary to reduce the heights of the mercury, the temperatures of both barometers being the same, to their lengths at one fixed temperature. The error being pointed out, Dr. Maskelyne demonstrated that the correction in feet would be constant for any given temperature of the

* It is to be hoped that our artists will not neglect to substitute for the centigrade scale one of this description, which will enable the observer to dispense with every calculation whatever on account of the unequal temperatures of the barometers. With the tables we shall furnish, the scale of Fahrenheit will be much more convenient than the one with the zero at the freezing point.

air. Ramond (or it may be an error of his translator in reducing metres to feet) states the correction per degree C, at the mean pressure of the atmosphere to be more than 3-100ths of an inch, and equal in elevation to more than 3 feet. The correct quantities are 5-1000ths of an inch, and more than 4 feet. The dilatation of $\frac{1}{5412}$ used by Ramond has been discovered to be incorrect from a mistake in the calculations of the experimenters.

Granting the assigned values of the dilatation of mercury and brass to be correct, if we suppose one of two syphon barometers, alike in every respect in their construction to be placed at any elevation on the side of a mountain, and the other exactly 423 feet above it, the mercury of the instrument at the base being constantly maintained at the freezing point, and that of the superior one at 212° F. then would the observed heights of the mercurial columns (the air being at 32°) be *alike*, without regard to the atmospheric pressures or variation of the pressures they supported.

Correction for the Aqueous Vapour contained in the Atmosphere.

An atmosphere of aqueous vapour uniformly of the same temperature would decrease in density in geometrical progression for equal perpendicular ascents; but as its specific gravity compared to dry air is as 10 to 16, it follows that if its density at an altitude of 16,000 feet should be found to be diminished one-half, a decrease in the same ratio of the density of the dry air would take place at an elevation of 10,000 feet; the pressures of the two fluids at the base being the same, and their temperatures alike.

An equal weight of dry air being mixed with the vapour, the two fluids would exist as distinct atmospheres, the particles of the one not pressing on those of the other,* and consequently maintaining their peculiar arrangement of density undisturbed. At the base the pressure would be double its former value, but as the altitude increased, this ratio, for the reasons assigned, would continue to diminish. Should the temperature of the mixture decline in proportion to the altitude, the diminution of the density of the vapour would be more conformable to that of the air;—the decrement of temperature being sufficiently rapid, it might even exceed it. So numerous and variable are the causes tending to disturb any regular law of the variation of the density, that we may consider the mean density of a stratum of moist air as equal to that of one of dry air under the same pressure, but of a temperature superior to that indicated by the detached thermometers, by the mean of the equations for the observed dew-points at the two stations, computed as pointed

* Dalton.

out at p. 50, vol. x. Substituting these augmented temperatures for those observed, we proceed in our calculations precisely the same as for dry air. When we come to the explanation of the tables, the method will be more fully described.

But what are the equations when the observer has been unfurnished with a hygrometer? Laplace has found on investigation of the observations of Ramond, that the correction for humidity in the mean state of the atmosphere may be made (without regard to pressure) by adding to the *mean* of the detached thermometers *half* the equation for saturated air supporting at that mean temperature a pressure of 30 inches.*

The degree of saturation of a section of the atmosphere may vary from several causes. It will be greater, *cæteris paribus*, in a maritime than an inland situation; and in the midst of mountains, especially if marshy, than when surrounding an arid peak isolated in a level district. When the temperature is low, the probability of the air being more nearly saturated is increased, particularly if the wind should be from a quarter where the quantity of vapour, from the proximity of the sea and the superior temperature, should naturally be considerable. We are also led to infer from the experiments made in different latitudes with the hygrometer of Mr. Daniell, that the correction for humidity should be greater at the same temperature for elevations above the sea not exceeding 5000 feet than for extraordinary altitudes.

Guided by these remarks, when the observer conceives the atmosphere to be unusually damp, let him add to the mean temperature two-thirds of the equations given in the table at p. 50, vol. x. On the other hand, when the air is judged to be in a state of extraordinary dryness, he may consider the degree of humidity as equal to one-third of the maximum quantity, and make his corrections in conformity.

(*To be continued.*)

* The formula of Laplace is incorrect for temperatures a little below 0° C. The augmentation of the dilatation of dry air $\left(\frac{1}{267}\right)$ to $\frac{1}{250}$ in order to cover the increase of humidity at elevated temperatures beyond its mean quantity at 0° (introduced in the general coefficient) has the effect of making moist air at low temperatures appear more dense than dry air.

Article II.

A Synopsis of the Genera of Cirripedes arranged in Natural Families, with a Description of some new Species. By John Edward Gray, Esq. FGS. &c.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *British Museum, June* 9, 1825.

Barnacle and acorn shells first attracted the attention of the older naturalists on account of the fables that they were the origin of the immense flocks of barnacle geese, as described by Gerard and others, but they were afterwards studied zoologically.

Linnæus placed these animals together in a genus under the name of Lepas, considering the animal as similar to his unfigured, and at present unknown, genus Triton.

Their anatomical structure has been displayed by John Hunter, Sir Everard Home (Comp. Anat.), Poli (Test. des deux Siciles), Cuvier (Ann. du Mus.), Savigny, Blainville (Anat. Comp.), and others.

The zoological characters of this group of animals have been much studied by Bruguières, Lamarck, Schumacher, Leach, and Ranzani; but it has been peculiarly unfortunate in having been chiefly attended to by naturalists who appear to disdain to consult and quote the works of others, or even to use their names, as may be observed by the following chronological list of genera, with the synonyma of the subsequent zoologists.

Lister, *Conch.* 1685.

1. *Anatifera.* Pentalasmis, *Hill.* Anatifa, *Lam.* Pentalepas, *Blain.* Lepas, *Brug.*

2. *Balanus.* Monolepas, *Klein,* who divided it into 2 §. 1. *Angipyle,* and 2. *Platipyle.*

Hill, *Gen. Nat. Hist.* 1752.

3. *Pollicipes.* Ramphidoma, *Schum.* Mitella, *Ock.* Pentalepas **, *Blainville.*

Klein, *Ostracologia,* 1753.

4. *Polylepas.* Diadema, *Schum.* Coronula, part. *Lam.* Coronula, *Leach.*

5. *Astrolepas.* Coronula part, *Lam.* Chelonobia, *Savigny, MSS. Leach.* Verruca, *Rumph.*

6. *Capitulum.* Pollicipes, part, *Leach?*

Linnæus, *Syst. Nat.* 1767.

Lepas (for the whole), now abolished, as the other names were used prior, and this means properly a Patella.

Lamarck, *Extrait du Cours.* 1812.

7. *Tubicinella.*

Schumacher, 1817.

8. *Malacota.* Branta, *Ocken.* Otion, *Leach.* Conchoderma, *Olfers.* Auritella, *Blainv.* Gymnolepas, *Blain. Dict.*

9. *Senoclita.* Cineras, *Leach.* Gymnolepas, *Blain.*

10. *Tetraclita.* Conia, *Leach.*

11. *Verruca.* Clisia, *Savigny, MSS.* Ochthosia, *Ranzani.* Creusia, *Lam.*

12. *Diadema.* Coronula, *Leach.*

Leach, *Ency. Brit. Sup.* 1819.

13. *Acasta.* Balanus, *Blainv. Sow.*

14. *Creusia.*

15. *Pyrgoma, Savigny, MSS.* Creusia, *Blain.*

16. *Scalpellum.* Polylepas, *Blain.*

There are several other genera named by this naturalist in the collection of the Museum, but they are without characters.

Say, *Journ. Acad. N. S. Phil.*

17. *Conoplea.* Mesula, *Leach, MSS.* Balanus, *Lam.*

Sowerby, *Genera.*

18. *Lithotrya.* Absia, *Leach, MSS.* Litholepas, *Blain.*

Ranzani, *Mem. di Stor. Nat.*

19. *Chthalamus.*

20. *Cetopirus.* Coronula, part, *Lam.*

21. *Asemus.*

This class of animals has been confounded by most authors, as Cuvier, Dumeril, &c. with the Mollusca; indeed the latter first separated them and the Brachiopodous Mollusca into a group from the rest under the latter name, but they were very properly distinguished from these animals by Lamarck, and considered as a distinct group between the Annulosa and Mollusca. Latreille considered them as Annelides, and Mr. W. S. Mac Leay has lately pointed out their position to be an annectant group between the Crustacea and the Radiata. Their affinity to the former is striking; it is not so apparent with regard to the latter, but this will most likely be more obvious when they become more completely known.

Class.—Cirripedes.

Lepas, Triton? *Lin.* Nematopoda, *Blain.* Cirripoda, *Lam.* Brachiopoda, *Dumeril.* Cirripeda, *Lam. Hist.*

Animal.—Body soft, conical, ending in a slightly ringed tail, inclosed in a fleshy sac, which is open at the hinder or anal extremity. Legs six pair, placed on the side of the tail, each ending in two compressed jointed, horny, and often ciliated appendages. Protected by a shelly case, formed of a certain number of shelly plates, which surround the body more or less completely.

Head not distinct, eyes and tentacula none. Nervous system consisting of a longitudinal series of ganglia united by a double cord, and several scattered ganglia.

Mouth placed at the base (or attached part) of the animal, furnished with three pair of horny jaws. Alimentary canal mostly simple, vent situated at the base of the proboscis-like terminal tube. The gills pectinate, one on each side at the base of the anterior pair of feet.

Hermaphrodite, oviparous, the aperture of the organs of reproduction placed at the end of the proboscis-like tube.

Attached directly, or through the medium of a tendinous tube, to marine bodies. Living on small sea animals which they collect by means of their legs. Growing very rapidly.

Linnæus considered the whole of the Cirripedes as one genus. The French naturalists generally look upon them as an order of mollusca, but Mr. W. Mac Leay has lately proposed, I believe very properly, that they should be regarded as an annectant class similar in rank to the *Annelides*, *Tunicata*, &c. Classes are usually divided into orders, but on account of the small number of genera at present known in this class, I have thought it right to follow the plan used by Mr. W. Mac Leay in his excellent paper on the Tunicata, just published in the Linnean Transactions, and to divide it into Families.

Lister divided this group of animals into two genera, calling them *Anatifera* and *Balanus*. Bruguières followed him, but changed the former name for that of *Lepas*. His genera may be considered as the primary division of the class, and the former appears to be the Normal group.

SYNOPSIS OF THE FAMILIES.

I. *Body compressed, peduncled.* Anatifera, *List.*
Peduncle naked.................... ANATIFERIDÆ.
Peduncle scaly or hairy........... POLLICIPEDIDÆ.
II. *Body coronal, sessile.* Balanus, *List.*
Operculum valves articulated.
Base concave................ PYRGOMATIDÆ.
Base flat or none............ BALANIDÆ.
Operculum valves separate........ CORONULIDÆ.

§ 1. *Normal Group?*—Body oval, compressed; open on the posterior ventral part, and prolonged to be fixed into a fleshy peduncle; shelly valves, five or more, imbedded in the coriaceous tunic, not articulated together, increasing by addition to their whole edge.

Fam. 1. ANATIFERIDÆ, *Gray.*

Body compressed; shelly valves, five or eight; one pair behind, and one or two pair before the legs; one plate on the back (rarely divided across); sheath of the peduncle smooth; destitute of additional scales.

**Body subcompressed; shelly plates small.*

Gen. 1. Malacota, *Schum.* (n. 8.)

Body club-shaped, with two cylindrical fleshy processes behind, just above the posterior shelly plates.

M. bivalvis, *Schum.* Lepas aurita, *Cuvier.*

2. Pamina, *Gray.*

Body club-shaped, with a cylindrical fleshy process behind, between the posterior plates.

P. trilineata, *Gray. Mus. Brit.*

3. Senoclita, *Schum.* (n. 9).

Body club-shaped, attenuated; hinder part, simple.

S. fasciata, *Schum.* Lepas membranacea, *Montague.*

***Body compressed; shelly plates large.*

4. Octolasmis, *Gray.*

Body subcompressed; shelly plates eight small, three lateral pair and two dorsal; the posterior valves linear ovate, with a notch for the end of the linear ventral valve; lateral central valve triangular; dorsal valves two, meeting at the angle of the back

O. Warwickii, *Gray.* Heptalasmis Warwickii, *Leach, MSS.*; but it has certainly eight valves. *Mus. Brit.*

5. Anatifera, *Lister* (n. 1).

Body compressed; shelly plates, five, large, two pair lateral, and one dorsal; lateral valves subtriangular; anterior pair very large; dorsal valve incurved.

† *Valves submembranaceous, dorsal one angulated, peduncle short.* Dosima, *Gray.*

D. fascicularis, *Gray.* Lepas fascicularis, *Montague.*

†† *Valves shelly, furrowed, dorsal one rounded, peduncle short.*

A. sulcata, *Gray.* Lepas sulcata, *Montague.*

††† *Valves shelly, smooth; dorsal one rounded; peduncle long.*

A. vulgaris. Lepas anatifera, *Lin.*

Fam. II. Pollicipedidæ, *Gray.*

Body compressed; shelly valves distinct; peduncle coriaceous, covered with hair or shelly scales.

**Shelly valves smooth, placed one above the other.* Living on wood, or on other marine bodies.

1. Scalpellum, *Leach* (n. 16).

Shelly plates, 13. Lateral plates six pair, subtriangular; dorsal one, linear kneed; peduncle annulated, with shelly scales.

1. S. vulgare, *Leach. Brit. Mus.*

2. Smilium, *Leach,* without character.

Shelly plates, 13. Lateral plates five pair, subtriangular; ventral and dorsal anterior plate, triangular, incurved; dorsal plate, linear, kneed; peduncle pilose.

S. Peronii, *Leach, MSS. Brit. Mus.*

3. Pollicipes, *Hill* (n. 3).

Shelly plates, 33 or 35. The posterior and posterior ventral pair, and dorsal plate large; the rest (14 or 15 pair), small, forming two or three series, the hinder ones largest; peduncle, covered with shelly scales, bald.

1. P. cornucopia, *Leach*. Lepas pollicipes, *Lin.* Ramphidoma vulgaris, *Schum.* P. Smithii is not distinct from this species.

4. Calantica, *Gray*.

Shelly plates, 15. The posterior and posterior ventral pair and the dorsal plate large; with eight smaller scales, forming one series, of which the dorsal and ventral are the largest; peduncle scaly, covered like the shelly plates with hair.

C. Homii, *nob.* Pollicipes tomentosus, *Leach*, (descrip.) P. hispidus, *Leach*, (plate).

I have dedicated this extraordinary species to Sir E. Home, who has most carefully examined the structure of the animals of this class.

5. Capitulum, *Klein.* (n. 6)

Shelly plates, 34. The posterior and posterior ventral pair, large, subarticulated; the lateral medial pair, and the dorsal and ventral plate middle sized, long triangular; with a series of 13 pair of small plates, at the top of the peduncle; peduncle, scaly, bald; shelly plates subsulcated.

This genus appears to be intermediate between this and the next section. I do not know its habits; it may be Lithophagous or a Lithodome. Rumphius is the only author who has figured its peduncle.

C. Mitella, *Gray.* Lepas Mitella, *Gmelin.*

***Shelly valves transversely, acutely, and reversedly sulcated, forming one series.* Forming or living in holes in rocks, shells, &c.

Lithotrya, *Sowerby* (n. 18).

Shelly valves, eight; two pair lateral, one dorsal, one ventral, and a series of minute shelly scales. Peduncle short, thick, reversed conical, with a hole at the anterior part near the attachment; attached to a concave, irregular, shelly valve. Living in holes in rocks, perhaps formed by itself, as the irregular plate, appears to move gradually down its side.

L. dorsalis, *Sow.* Lepas dorsalis, *Ellis.* Absia Lesuerii, *Leach, MSS.*

Mr. Sowerby only describes seven valves.

Ibla, *Leach*, without character.

Shelly valves, four; posterior pair elongated, slightly curved; ventral pair, triangular short; peduncle cylindrical, contracted near the attachment, covered with hair-like processes.

I. Cuvieriana, *Leach, MSS.* Valves transversely annulated, laminæ pointing towards the peduncle. *Mus. Brit.*

Conchotrya, *Gray*.

Shelly plates, five; two pair ventral, and one plate dorsal; peduncle ——— ?

Lives in holes in shells.

C. Valentiana, *Gray*. Shelly plates, thick, transversely lamellated.

Inhabits Red Sea in the valves of *Ostrea Cucullata,* Born; *Lord Valentia*.

Brisnæus, *Leach,* without character.

Shelly plates, seven; three pair lateral, and one valve dorsal. Body cylindrical conical. Peduncle or base ——— ?

Living in holes in stony corals; the holes are clean without any shelly deposit.

B. rodiopus, *Leach*. Shelly valves, all transversely lamellar. *Mus. Brit*.

II. *Annectant Group?* Body conical, cylindrical. Shelly valves, four, or six, or eight, articulated together laterally, and sometimes to a shelly cup, called the support, which closes the front of the shell, the hinder part closed by an operculum formed of two or four valves, which leave an aperture for the passage of the feet; the shelly valves increasing only at their broadest or anterior edge. Attached immediately to or imbedded in marine bodies.

Obs. The operculum appears to represent the posterior and posterior ventral valves of the former group.

Fam. III. Pyrgomatidæ, *Gray*.

Body, four, or six-valved; operculum four-valved, oblique, valves articulated together; base shelly, concave, cup-shaped. Living imbedded in zoophytes.

Each genus of this family appears to be peculiar to a certain genus or group of zoophytes.

**Shelly valves of the body, four, sometimes united together.* Living buried in stony corals.

1. Pyrgoma, *Savigny, MSS. Leach* (n. 15).

Valves of the body of the shell 4, soldered together; sheath of the operculum very small; operculum conical, four-valved, ventral valve linear, posterior valves hooked narrow triangular.

P. cancellata, *Leach*. Shell radiately ribbed.

P. lobata, *Gray*. Shell concentrically striated, deeply lobed.

2. Daracia, *Gray,** Savignium, *Leach,* without character.

Valves of the body of the shell, four, soldered together; sheath

* Although I have been desirous from courtesy to preserve even the manuscript names of Dr. Leach, I have here been necessitated to change one, as it is against the rules of zoological nomenclature.

of the operculum none; operculum convex, two-valved, the ventral and posterior valve of each side being soldered together.

D. Linnæi, *nob.* *Esper Zooph. Madrep.* t. 85.

Obs. Linnæus describes this species as part of his *Madrepora Polygama*, and appears to take the name from his belief that the coral was inhabited by two kinds of animals.—(See *Lin. Sys. Nat.* 1275; *Am. Acad.* iv. 258, t. 3, f. 15; *Esper. Zooph.* i. 29; *Boddaert, Elench. Zool.* 324.)

3. Megatrema, *Leach,* without character.

Valves of the body of the shell, four, soldered together; sheath of the operculum nearly as long as the valves; operculum conical, four-valved, valves subtriangular.

†*Support of the valves immersed.* Valves finely striated.

M. Stokesii, *Gray.* *Mus. Brit.* on Fungia.

††*Valves convex; support of the valves conical exserted;* Adna, *Leach,* no character.

M. A. Anglica; valves and support radiately grooved, and concentrically striated; *Devonshire, Mus. Brit.* on a new species of *Caryophyllia.*

4. Creusia, *Leach* (n. 14).

Valves of the body of the shell 4, distinct; sheath of the operculum nearly as long as the valves; operculum conical, four-valved, valves triangular.

†*Base convex, prominent, sitting on the coral; shell convex.*

C. spinulosa, *Leach. Mus. Brit.*

Dr. Leach describes the valves of the operculum as soldered two and two, but they are not so in the Museum specimens.

††*Base sunk in the coral; body of the shell nearly flat.*

C. Childreni, *nob. Mus. Brit.*

***Shelly valves of the body, six.* Living in or on the surface of horny or barked zoophytes.

5. Conoplea, *Say* (n. 16).

Body short; valves six, elongate, distinct, truncated; ventral, dorsal and lateral dorsal pair large; lateral ventral pair small; operculum conical pointed, four-valved. The base elongate keeled. *Attached to the stems of Gorgoniæ.*

1. C. elongata, *Say.* The base elongated behind. N. America. B. galeatus, *Gmelin.*

2. C. ovata, *nob.* The base ovate. Africa.

6. Acasta, *Leach* (n. 13).

Globular; valves six distinct, long triangular; apex acute; the ventral, dorsal, and lateral dorsal pair, large; the lateral ventral pair, small; operculum conical, acute, four-valved. Base hemispherical. *Imbedded in sponges.*

1. A. Montagui, *Leach.* Lepas spongiosa, *Montague.*

2. A. lævigata, *Gray.* Shell subglobular, yellow, unarmed. Valves finely concentrically striated; the Tropics.

Fam. IV. BALANIDÆ.

Shell of the body, four, six, or eight-valved; operculum four-valved, oblique, valves articulated. Base none, or shelly, imitating the substance to which it is attached. *Attached to all sorts of marine bodies.*

* *Shell of the body six-valved; valves unequal; the lateral ventral pair smaller than the rest.*

BALANUS, *Lister* (n. 2).

Body conical; six-valved; operculum conical acute, four-valved.

B. Tintinnabulum, *Brug.*.

There are two species in the Museum named *Elminius*, by Dr. Leach, which in the dissected specimens have only four valves displayed, by which character the Dr. most probably intended to separate this genus; but on examining the other specimens, the six valves are very distinctly to be seen; so that they are true *Balani*. The opercula appear nearly horizontal; they may be what is intended by the following genus, which I have not seen; this idea is strengthened by one of the species being from Sicily.

CHTHALAMUS, *Ranzani* (n. 20).

Body very depressed; valves six; area very prominent, nearly equal? The internal plate short; base membranaceous; mouth nearly equally four-sided; operculum somewhat pyramidical four-valved, horizontally attached by a membrane to the mouth.

C. stellatus, *Ranz. Poli Moll.* t. 5, f. 12—17.

** *Body, four or eight valved; valves unequal; substance often thick, porous; base none.*

OCTOMERIS, *Sowerby.*

Body depressed; conical; valves eight, thick; operculum subconical; four-valved.

O. Stuchburii, *Gray.* Africa?

I have seen this shell in the shop of Mr. G. Sowerby; who is, I understand, about to describe it under the above generic name.

TETRACLITA, *Schum.* n. 10.

Body, conical; valves four; operculum, four-valved.

†Asemus, *Ranzani; sutures of the valves indistinct.*

1. A. stalactifera, *Ranz. Blain. Ency. Method.* t. 165, f. 9, 10. C. porosa, *Leach?*

††Conia, *Ranz.* (*not Leach*); *sutures very distinct.*

T. C. radiata, *Ranz. Blain. Ency. Method.* t. 164, f. 15. Tetraclita squamulosa, *Schum.* L. Jungites, *Chem.* viii. t. 98, f. 836.

VERRUCA, *Schum.* n. 11.

Body depressed, four-valved, valves oblique, sulcated; operculum convex, four-valved; valves soldered, 2 and 2.

V. Stromii, *Schum.* Balanus striatus, *Pen. Brit. Zool.* Clisia striata, *Leach.* Creusia Stromia? and C. verruca, *Lam. Hist.*

Fam. V. CORONULIDÆ, *Gray.*

Body, conical, or cylindrical; six-valved; valves, cellular; operculum, four-valved, horizontal; valves imbedded in the tunic (not articulated); base none, or membranaceous. *Imbedded in or attached to the organic parts of sea animals, as whales, turtles, crabs.*

1. TUBICINELLA, *Lam.* (n. 7).

Body cylindrical, or front rather contracted; operculum, valves equal.

T. trachealis, *Lam.*

2. POLYLEPAS, *Klein* (n. 4).

Body subdepressed. Mouth nearly circular; valves very thick, outside lobed, inside many cells; operculum, posterior valves largest.

†Diadema, *Schum. Convex, front of the cavity contracted.*

P. D. Kleinii, *Gray.* P. Balænaris, *Klein.* Lepas Diadema, *Lin.*

††Cetopirus, *Ranz. Depressed, front of the cavity scarcely contracted.*

P. C. vulgaris, *Gray.* Coronula Balænaris, *Lam.*

3. PLATYLEPAS, *Gray.* Coronula, *Ranz.* part *Lam.*

Body depressed. Mouth ovate. Valves, outside two-lobed; inside celled, midribbed; operculum, valves nearly equal.

P. pulchra, *Gray.* Shelly valves finely transversely striated; sutures smooth, Corsica. *Mus. Brit.* Chelonobia, species, *Leach.* C. bisexloba, *Ranz.*

This genus is very interesting, as being exactly intermediate between Polylepas and Astrolepas: it has the cells of the former, and the form and operculum of the latter.

4. ASTROLEPAS, *Klein* (n. 5).

Body depressed. Mouth six-sided. Valves thick, subsolid, base toothed, rugose; operculum, valves equal.

1. *A. Testudinaria, Gray.* Shelly valves, radiately substriated; sutures, distinct, simple. Balanus Testudinaria, *Lin.* Coronula Testudinaria, *Lam.*

2. *A. rotundarius, Gray.* Shelly valves, smooth: sutures, wide, transversely pitted. Balanus rotundarius, *Lin.* A. Testudinaria, *Klein.*

3. *A. lævis, Gray.* Shelly valves, smooth; sutures distinct, simple.

Coronula denticula, Say; found on the clypeus of the King

Crab, will most probably form a new genus of this family: it is curious as not being parasitic on vertebrated animals. I have found another exception in *A. lævis,* living on a specimen of *Voluta porcina.*

The affinity which exists between the families of this group is not very apparent at first sight; but upon examination, the passage from one to the other is very striking, and sometimes it is very difficult to decide to which family the genera should be referred, as may easily be imagined when I state that both Dr. Leach and Blainville placed the second section of the *Anatiferidæ* as genera of the family *Pollicipedidæ,* and several of the second section of the latter family are so exceedingly allied to the *Pyrgomatidæ* both in their structure and habits, that I was very doubtful to which family they should be referred, nor was I satisfied till I had reason to believe that *Lithrotrya,* which has many of the characters of *Pollicipes,* was lithophagous, and, therefore, agreed in habits with *Brisnæus* and *Conchotrya,* the genera under consideration.

Thus the transition of the *Pollicipedidæ* of the first group to the *Pyrgomatidæ* of the second, appears very natural, but the genus which forms the junction is yet wanting, for the last genera of the former family may be known from the genera of the *Pyrgomatidæ* by their always appearing to form themselves (by chemical action most probably) the holes which they inhabit, whereas the cells in the corals inhabited by the latter family are caused by the animal raising up its body, and adapting itself to the growth of the zoophyte to which it is attached, which in fact often overruns and destroys it. The passage from the *Pyrgomatidæ* to the *Balanidæ* must be very evident when both Blainville and Mr. Sowerby have placed the genus *Acasta,* and Lamarck the genus *Conoplea,* both of which have evidently the habits of the *Pyrgomatidæ* as species of the genus *Balanus.*

The genus *Tetraclita* of the second section of the *Balanidæ* wants the shelly base, and has the cellular structure of the valves which is so peculiar a character in the *Coronulidæ,* which also has only a membranaceous base.

Another peculiar character of the latter family is that the valves of the operculum are small and distant one from another, and simply imbedded in a membranaceous tunic, which protrudes considerably beyond the mouth of the shelly valves: now, as I have before observed, as there is reason to believe that the valves of the operculum are analogous to the posterior and posterior ventral valves of the *Anatiferidæ,* there must be an evident resemblance in structure between the genera of the first section of the latter family, where these valves are exceedingly small, and the genera of *Coronulidæ.* But this is only an affinity of general structure, the genus intermediate between these families is still a desideratum, for the nearest approximation which I know of at present is between the genera *Malacota* and *Tubicinella*, both of which are subcylindrical, although one evidently belongs to the compressed, and the other to the depressed group of the class.

The families are susceptible of several methods of division, but that used by Lister appears to be most natural: thus, in *Anatiferidæ* and *Coronulidæ*, the base and support of the valves is only a thin naked membrane; while in the other three families, it is more or less shelly; for although it is flexible in *Pollicipedidæ*, its surface is always covered with shelly scales, and in *Pyrgomatidæ* and *Balanidæ*, it is as completely shelly as the valves of the body themselves, and the valves of the operculum are articulated together, and most accurately fit the mouth of the shells.

The older naturalists were inclined to make too few species of this class; but the modern ones, in avoiding this fault, have gone to the other extreme, by making too many. There is a very large collection in the Museum named by Dr. Leach, but nearly all his names being new (and often two or three to the same species), without the slightest reference to those of other authors, they, therefore, cannot be adopted without great examination. The species are not very easy to determine, as the shelly plates of the *Anatiferidæ* and *Pollicipedidæ* are exceedingly apt to vary both in their form and surface, even in the individuals of the same group. The shells of the *Balanidæ* are also greatly altered in their general form by the closeness of the neighbouring specimens; when close they become elongated (thus *B. cylindraceus*), and when scattered they are often depressed and spread out at the base. The surface of the valves is also altered by the structure of the substance to which they are attached thus. I have a Barnacle on a Pecten which is transversely ribbed, and another on a piece of wood where the surface has all the lines of the grain marked on it. The species of the *Pyrgomatidæ* are often overrun by the corals in which they live and are thus destroyed, and rendered almost useless as zoological specimens.

Article III.

Explanation of an Optical Deception in the Appearance of the Spokes of a Wheel seen through vertical Apertures. By P. M. Roget, MD. FRS.* (With a Plate.)

A curious optical deception takes place when a carriage wheel, rolling along the ground, is viewed through the intervals of a series of vertical bars, such as those of a palisade, or of a Venetian window-blind. Under these circumstances the spokes of the wheel, instead of appearing straight, as they would natu-

* From the Philosophical Transactions for 1825, Part I.

rally do if no bars intervened, seem to have a considerable degree of curvature. The distinctness of this appearance is influenced by several circumstances presently to be noticed; but when every thing concurs to favour it, the illusion is irresistible, and, from the difficulty of detecting its real cause, is exceedingly striking.

The degree of curvature in each spoke varies according to the situation it occupies for the moment with respect to the perpendicular. The two spokes which arrive at the vertical position, above and below the axle, are seen of their natural shape, that is, without any curvature. Those on each side of the upper one appear slightly curved; those more remote, still more so; and the curvature of the spokes increases as we follow them downwards on each side till we arrive at the lowest spoke, which, like the first, again appears straight.

The most remarkable circumstance relating to this visual deception is, that the convexity of these curved images of the spokes is always turned downwards, on both sides of the wheel; and that this direction of their curvature is precisely the same, whether the wheel be moving to the right or to the left of the spectator. The appearance now described is represented in Plate XXXVI. fig. 1.*

In order to discover a clue to the explanation of this phænomenon, it was necessary to observe the influence which certain variations of circumstances might have upon it; and the following are the principal results of the experiments I made for this purpose.

1. A certain degree of velocity in the wheel is necessary to produce the deception above described. If this velocity be gradually communicated, the appearance of curvature is first perceptible in the spokes which have a horizontal position: and as soon as this is observed, a small increase given to the velocity of the wheel, produces *suddenly* the appearance of curvature in all the lateral spokes. The degree of curvature remains precisely the same as at first, whatever greater velocity be given to the wheel, provided it be not so great as to prevent the eye from following the spokes distinctly as they revolve: for it is evident, that the rapidity of revolution may be such as to render the spokes invisible. It is also to be noticed that, however rapidly the wheel revolves, each individual spoke appears, during the moment it is viewed, to be at rest.

2. The number of spokes in the wheel makes no difference in the degree of curvature they exhibit.

3. The appearance of curvature is more perfectly seen when the intervals between the bars through which the wheel is

* The appearance in question has been noticed by an anonymous writer in the Quarterly Journal of Science (vol. x. p. 282), who gives, however, no explanation of the phenomenon. It would have been impossible, indeed, to reconcile the facts as they are there stated, with any theory that could be imagined for their solution.

Fig. 1.

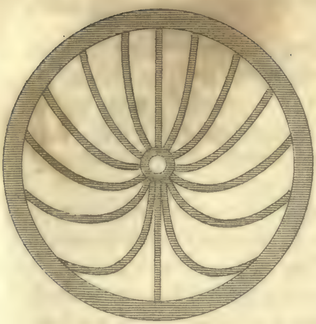


Fig. 2.

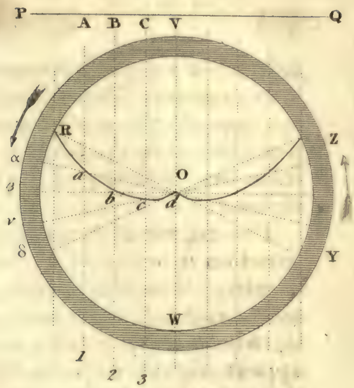


Fig. 3.

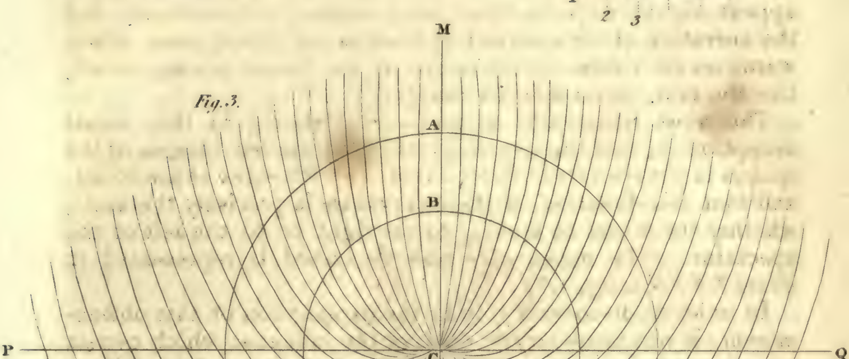


Fig. 4.

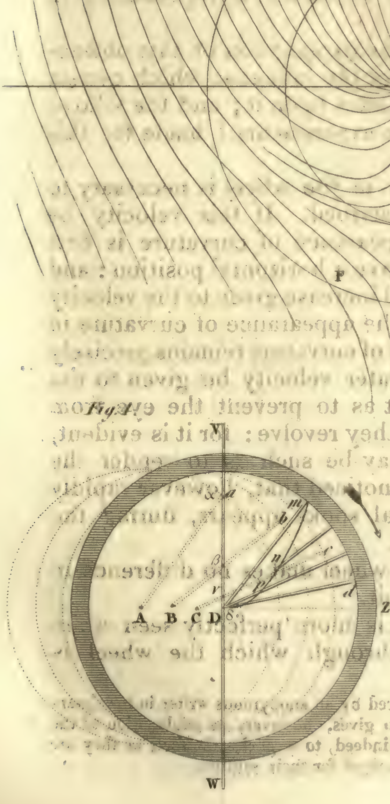
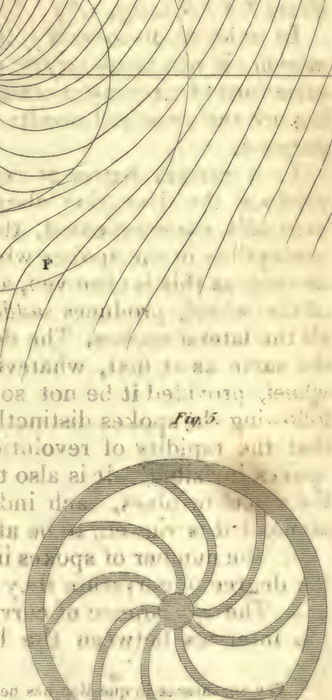


Fig. 5.



viewed, are narrow; provided they are sufficiently wide to allow of the distinct view of all the parts of the wheel in succession, as it passes along. For the same reason, the phænomenon is seen to the greatest advantage when the bars are of a dark colour, or shaded, and when a strong light is thrown upon the wheel. The deception is, in like manner, aided by every circumstance which tends to abstract the attention from the bars, and to fix it upon the wheel.

4. If the number of bars be increased in the same given space, no other difference will result than a greater multiplication of the curved images of the spokes; but if a certain relation be preserved between the angles subtended at the eye by the whole intervals of the bars, and of the extremities of the spokes, this multiplication of images may be corrected. The distance of the wheel from the bars is of no consequence, unless the latter are very near the eye, as in that case the apertures between them may allow too large a portion of the wheel to be seen at once.

5. If the bars, instead of being vertical, are inclined to the horizon, the same general appearances result; but with this difference, that the spokes occupying positions parallel to the bars, are those which have no apparent curvature; while the curvatures of the other spokes bear the same relations to these straight spokes, and to each other, that they did in the former case. When the inclination of the bars is considerable, however, the images become more crowded, and the distinctness of the appearance is thereby diminished. The deception totally ceases when the wheel is viewed through bars that are parallel to the line of its motion.

6. It is essential to the production of this effect, that a combination should take place of a progressive with a rotatory motion. Thus, it will not take place if, when the bars are stationary, the wheel simply revolves on its axis, without at the same time advancing: nor when it simply moves horizontally without revolving. On the other hand, if a progressive motion be given to the bars, while the wheel revolves round a fixed axis, the spokes immediately assume a curved appearance. The same effect will also result if the revolving wheel be viewed through fixed bars by a spectator, who is himself moving either to the right or left; because such a movement on the part of the spectator produces in his field of vision an alteration in the relative situation of the bars and wheel.

It is evident from the facts above stated, that the deception in the appearance of the spokes must arise from the circumstance of separate parts only of each spoke being seen at the same moment; the remaining parts being concealed from view by the bars. Yet since several parts of the same spoke are actually seen in a straight line through the successive apertures, it is not so easy to understand why they do not connect themselves in the imagination, as in other cases of broken lines, so as to convey the impression of a straight spoke. The idea at first suggests itself that the portions of one spoke, thus seen separately, might possibly connect themselves with portions of the two adjoining spokes, and so on, forming by their union, a curved image made up of parts from different successive spokes. But a little attention to the phænomena will show that such a solution cannot apply to them: for when the disc of the wheel, instead of being marked by a number of radiant lines, has only one radius marked upon it, it presents the appearance, when rolled behind the bars, of a number of radii, each having the curvature corresponding to its situation; their number being determined by that of the bars which intervene between the wheel and the eye. So that it is evident, that the several portions of one and the same line, seen through the intervals of the bars, form on the retina the images of so many different radii.

The true principle, then, on which this phænomenon depends, is the same as that to which is referable the illusion that occurs when a bright object is wheeled rapidly round in a circle, giving rise to the appearance of a line of light throughout the whole circumference; namely, that an impression made by a pencil of rays on the retina, if sufficiently vivid, will remain for a certain time after the cause has ceased. Many analogous facts have been observed with regard to the other senses, which, as they are well known, it is needless here to particularize.

In order to trace more distinctly the operation of this principle in the present case, it will be best to take the phænomenon in its simplest form, as resulting from the view of a single radius, fig. 2, OR of the wheel VW, revolving steadily upon its axis, but without any progressive motion, and seen through a single narrow vertical aperture which is moving horizontally in a given direction PQ. Let us also assume that the progressive motion of the aperture is just equal to the rotatory motion of the circumference of the wheel. It is obvious that if, at the time of the transit of the aperture, the radius should happen to occupy either of the vertical positions VO or OW, the whole of it would be seen at once through the aperture, in its natural position; but if, while descending in the direction VR, it should happen to be in an oblique position RO, terminating at any point of the circumference at the moment the aperture has, in its progress horizontally, also arrived at the same point R, the extremity of the radius will now first come into view, while all the remaining part of it is hid. By continuing to trace the parts of the radius that are successively seen by the combined motions of the aperture and of the radius, we shall find that they occupy a curve R *a b c d* generated by the continued intersection of these two lines. Thus, when the aperture has moved to A, the radius will be in the position O α; when the former is at B, the latter will be at O β, and so on.

Again; let us suppose that when the aperture is just passing the centre, the radius should be found in a certain position on the other side OY, and rising towards the summit. Then tracing, as before, the intersections of these lines in their progress, we shall obtain a curve precisely similar to the former. Its position will be reversed; but its convexity will still be downwards.

If the impressions made by these limited portions of the several spokes follow one another with sufficient rapidity, they will, as in the case of the luminous circle already alluded to, leave in the eye the trace of a continuous curve line; and the spokes will appear to be curved, instead of straight.

The theory now advanced is in perfect accordance with all the phænomena already detailed, and is farther confirmed by extending the experiments to more complicated combinations.

It readily explains why the image, or spectrum, as it may be called, of the spoke, is at rest, although the spoke itself be revolving: a circumstance which might escape notice, if the attention were not particularly called to it.

Since the curved appearance of the lines results from the combination of a rotatory, with a progressive motion of the spokes, in relation to the apertures through which they are viewed, it is evident that the same phænomena must be produced if the bars be at rest, and both kinds of motion be united in the wheel itself. For, whether the bars move horizontally with respect to the wheel, or the wheel with respect to the bars, the relative motion between them, and its effects, in as far as concerns the appearance in question, must be the same. The attention of the spectator should in both cases be wholly directed to the wheel, so that the motions in question should be referred altogether to it. Thus, in fig. 4, the real positions, at successive intervals of time, of the spoke A *a*, when the wheel is rolling on the ground in the direction AZ, are expressed by the lines A*a*, B*b*, C*c*, and D*d*. While the spoke is in these positions, the portions of it really seen through the fixed aperture VW, are the parts α, β, γ, δ, the impressions of which, being retained upon the retina, and referred to the wheel when in its last position, form the series of points *m*, *n*, *p*, and *q*, in the curved spectrum *m* D.

That the attention may the more easily follow the wheel in its progression, it is necessary that its circumference be distinctly seen, and its real situation correctly estimated. Hence, although it be true, that by a sufficient exertion of attention the phænomenon may be exhibited by means of a single aperture, it is much more readily perceived, when the number of apertures is such as to allow the wheel to be seen in its whole progress. For this reason the phænomenon is very distinct in the case of a palisade.

Each aperture produces its own system of spectra; and hence, when the apertures occur at short intervals, the number of the spokes is considerably multiplied; but if the intervals be so adjusted as to correspond with the distances between the spokes at the circumference of the wheel, the images produced by each aperture will coalesce, and the effect will be much heightened.

A mathematical investigation of the curves resulting from the motion of the points of intersection of a line moving parallel to itself, with another line revolving round its axis, will show them to belong to the class of Quadratrices, of which the one which touches the circumference of the inner generating circle is that which is known by the name of the Quadratrix of Dinostrates. Such a system of curves is represented in fig. 3, where MC, CN, are the generating radii, A the outer, and B the inner generating circles, and PQ the common axis of the curves.

All these curves have the same general equation, namely,

$$y = (b - x \,.\, \text{tang.}\, x.$$

where the co-ordinates are referred to the axis at right angles to the vertical generating radii, and passing through the centre of their revolution: the basis b being measured on the axis from the point of its intersection with the curve to the centre: and x being the arc of the inner generating circle, as well as the abscissa.*

A wheel simply rolling on its circumference exhibits, when seen through fixed bars, only those portions of the curves which are contained within the inner circle; but when its motion of revolution is more rapid than its horizontal progression, as when it is made to roll on an axle of less diameter on a raised rail-way, then the remaining portions of the curves will be seen, and others, on the lower part of the wheel, having a contrary flexure, will also make their appearance. These are seen at FF in fig. 3.

If the spokes, instead of being straight, be already curved, like those of the Persian water-wheel, their form, when viewed through bars, will undergo modifications, which may readily be traced by applying to them the same theory. Thus, by giving a certain curvature to the spokes, as in fig. 5, they will at one part of their revolution appear straight, namely, where the optical deception operates in a direction contrary to the curvature.

The velocity of the apparent motion of the visible portions of the spokes is proportionate to the velocity of the wheel itself; but it varies in different parts of the curve; and might therefore, if accurately estimated, furnish new modes of measuring the duration of the impressions of light on the retina.

* This equality between the arc and the abscissa is a necessary consequence of the progressive motion of the wheel being equal to the rotatory motion of its circumference: the former motion producing the increments of the abscissa; and the latter those of the arc of the circle. The equation $y = (b - x)$. tang. x. is deduced from a simple analogy of the sides of similar triangles.

Article IV.

Astronomical Observations, 1825. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Siderial Time.

1825.	Stars.	Transits.		
June 28.	α Ophiu.	17h	04′	41·07″
28.	39 Ophiu.	17	07	02·78
28.	Moon's First or West Limb	17	08	22·88
28.	θ Ophiu.	17	11	21·17
28.	ε² Ophiu.	17	20	50·11
28.	*d* Ophiu.	17	32	50·09
July 1.	*f* Sagitt.	19	35	13·21
1.	57 Sagitt.	19	42	05·98
1.	*g* Sagitt.	19	47	45·42
1.	Moon's Second or East Limb	20	05	15·83
1.	π Capric.	20	17	22·14
1.	ρ Capric.	20	18	57·26
1.	τ Capric.	20	29	32·86

Article V.

On the Preparation of Acetate of Soda: &c. By Mr. N. Mill.

(To R. Phillips, Esq. FRS. &c.)

SIR, *Addington-square, Camberwell.*

In commenting on Dr. Hope's observations upon your strictures on the Edinburgh Pharmacopæia, *Annals of Philosophy*, N. S. ii. p. 23, you give a process for procuring acetic acid by the double decomposition of acetate of lead and sulphate of soda; and Dr. Henry, in the last edition of his Chemistry, in quoting your paper, has added in parenthesis, that 4½ ounces of acetate of lime might be used instead of the acetate of lead. In the proportions there stated (nor indeed in any other proportions), have I been able to effect a *perfect* decomposition of sulphate of soda by acetate of lime.

200 grains of acetate of lime, dried at a temperature of 430° or 440° Fahr. were decomposed by 400 grains of crystallized sulphate of soda, the solution evaporated, and crystals obtained. These crystals when dissolved in water and tested with the muriate of barytes gave a copious precipitate of sulphate of barytes, but neither sulphuric nor oxalic acid occasioned any precipitate of lime. These crystals were not, therefore, *acetate*

of soda, but a compound salt, consisting of *sulphate of soda* and *acetate of soda*. The mother water when tested gave precipitates *of both* sulphate of lime by sulphuric acid and sulphate of barytes by muriate of barytes, thereby proving that *acetate of lime* and *sulphate of soda* are incompatibles only to a *certain extent;* for they may and do exist at the same time in the same solution. If acetate of lime be added to the mother liquor *ad infinitum*, the sulphate of soda will not be totally decomposed; nor, on the contrary, if sulphate of soda be added to the mother water instead of the lime, will the lime which existed in the liquid disappear, for oxalic acid still occasions a copious precipitate.

Crystals procured from either of these last solutions, whether acetate of lime or sulphate of soda be in excess, still give as large a precipitate with muriate of barytes as heretofore, thereby indicating that the sulphate of soda is not *wholly* decomposed, and that a *perfect* acetate of soda *cannot* be obtained through the medium of sulphate of soda.

I am also of opinion, that most of the acetates are deficient in the power of totally decomposing the sulphates, which opinion is strengthened by Dr. Thomson's experiments to discover the atomic weight of acetic acid, *Annals of Philosophy*, ii. p. 142, N. S. He found acetate of lead to be nearly in the same situation as acetate of lime; for he states, that "acetate of lead does not possess the power of throwing down the *whole* of the sulphuric acid from the solution of a sulphate." If this be the case, the process you have given for procuring acetic acid from the double decomposition of the acetate of lead and sulphate of soda must be defective, inasmuch as this, that the acetic acid is not procured from acetate of soda (which should result from the perfect decomposition) *in toto*, but from a compound salt of *acetate of lead* and *acetate of soda*. In order to ascertain the proportions of sulphate of soda in the crystallized salt before alluded to, I dissolved 100 grains of the crystals in water, and added muriate of barytes so long as any deposition took place, the precipitated sulphate of barytes was then collected, dried, and weighed 10 grains, which is equivalent to 14·7 grains of the crystallized sulphate of soda. This salt is, therefore, composed of (in 100 parts)

Crystallized acetate of soda	85·3
Crystallized sulphate of soda. ..	14·7 = 100.

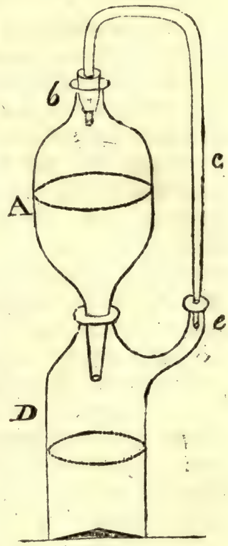
As the pyroligneous acid manufacturers commonly decompose acetate of lime by sulphate of soda to procure acetate of soda, it must be of some importance to them to know that, independently of the loss of the salts left in the mother water by this process, they also procure an impure article.

Your obedient servant, Nicholas Mill.

Article VI.

Description of an Apparatus for filtering out of Contact with the Atmosphere. By M. Donovan, Esq.*

We promised in our last to give a description of Mr. Donovan's apparatus for filtering substances liable to be affected by the contact of the atmosphere, as solutions of caustic alkali, &c.



An inspection of the wood-cut annexed will at once explain the nature of the instrument. It consists of two glass vessels, the upper one A has a neck at *b*, which contains a tight cork, perforated to admit one end of the glass tube *c*. The other end of the vessel A terminates in a funnel pipe, which fits into one of the necks of the under vessel D, by grinding or luting, or by a tight cork. The vessel D has also another neck *e*, which receives the other end of the tube *c*, the junction being secured by a perforated cork, or by luting. The throat of the funnel pipe is obstructed by a bit of coarse linen loosely rolled up, and not pressed down into the pipe. The solution is then to be poured in through the mouth at *b*, the cork and tube having been removed, and the first droppings are to be allowed to run to waste, and not received into the under vessel D. The parts of the apparatus are now to be joined together, and the filtration may proceed at the slowest rate, without the possibility of any absorption of carbonic acid by the alkali.

The mode of action of this simple but ingenious apparatus is too obvious to require any explanation; but Mr. Donovan observes, that it should be made of green glass in preference to white, the former being much less acted on by fixed alkalies than the latter. He states that a white glass bottle containing a solution of caustic potash will often be cracked by it in every direction, and in a singular manner.

This apparatus is useful for filtering liquids, to which access of the carbonic acid, or moisture of the atmosphere, would be injurious, as well as for the filtration of volatile substances, as alcohol, ethers, ammoniacal fluids, &c. If a stratum of coarsely pulverised glass or flint be substituted for the roll of linen, it may be employed for filtering corrosive acids, which would be weakened by absorbing water from the atmosphere.

* Abridged from the Dublin Philosophical Journal.

Article VII.

On Fluoric Acid, and its most remarkable Combinations. By Jac. Berzelius.

(*Continued from* vol. ix. p. 131.)

Decomposition of Silicated Fluoric Acid by Potassium.

The description of the experiments made by the French chemists leads unavoidably to the conclusion, that they had succeeded in decomposing fluoric acid; and when I undertook, therefore, to repeat them, my sole object was to acquire more accurate information regarding the composition of the reduced products. In my first trials I obtained the same results which they had already described, with this single exception, that the product of the decomposition did not become white by ignition in oxygen gas, but retained its original brown colour almost unaltered. Expecting that this calcined mass would contain silicated fluate of potash, I poured over it concentrated sulphuric acid; but no trace of fluoric acid was disengaged, nor was the slightest alteration produced upon it by evaporating the mixture to dryness. All other acids, with the exception of the fluoric, proved equally inactive. This acid extracted a quantity of silica, and left behind a darker brown-coloured matter, which was insoluble in acids, and incombustible when ignited. Was this the radical of the fluoric acid or of the silica, or a combination of both?

To obtain a larger supply of this substance, I ignited some potassium in a suitable apparatus in contact with an atmosphere of silicated fluoric acid gas. The metal gradually darkened in colour, until it finally became as black as coal; soon after, it took fire, and burned with a large dark red coloured flame, which, however, was by no means intense, and the gas was at the same time rapidly absorbed. The product of the combustion was a hard, porous, dark brown-coloured mass, unaltered by exposure to the air, but imparting, when touched or breathed upon, that peculiar odour of hydrogen gas, which is observed when we handle metallic manganese. On being thrown into water, it occasioned a copious disengagement of hydrogen gas, and much fluate of potash passed into solution. By degrees, the evolution of gas became less and less considerable, and the mass disintegrated to a powder. When the action in the cold appeared to have ceased, the alkaline solution was replaced by fresh water, and the mixture was heated to ebullition. No additional disengagement of hydrogen was produced by this treatment, but the liquid now became strongly acid, and proved to be a saturated solution of silicated fluate of potash. The powder was now repeatedly boiled in water until it was rendered completely free from all soluble matter: it was then collected upon a filter and dried.

To determine the alteration which this substance would undergo by combustion, I ignited it in a current of oxygen gas. It instantly took fire, and burned with some intensity, a pale blue-coloured flame being at the same time visible over its surface. The gaseous product of the combustion produced an abundant precipitate in barytes water, and this precipitate proved on examination to be pure carbonate of barytes, unmixed with silicated fluate. The calcined mass had diminished greatly in volume, but retained its original colour almost unaltered, and the increase of weight which it had sustained scarcely amounted to a half per cent. No corrosion by fluoric acid, and no deposition of silica, could be observed in any part of the apparatus; consequently fluoric acid did not form one of the products of the combustion. I was therefore disappointed in my expectation of ascertaining by this means the composition of fluoric acid; yet the result was not the less interesting, for it appeared to me that I had succeeded in isolating the radical of silica, and that the brown pulverulent body was in reality silicium. That it should undergo combustion in oxygen gas and give off carbon without becoming heavier, was easily intelligible; because the same circumstance is observable when we calcine the quadricarburets of most of the metals whose oxides contain three atoms of oxygen. The presence of so much carbon appeared at first to be somewhat unaccountable: I speedily ascertained, however, that it had previously existed in a state of chemical combination with the potassium. This potassium had been prepared by Brunner's process, in which a mixture of carbonate of potash and charcoal is strongly ignited in a retort of malleable iron; and by redistilling it in a glass vessel, I found that it left behind a carbonaceous matter which took fire when exposed to the air, and which in water caused the disengagement of hydrogen gas; whilst, at the same time, potash was formed, and a large quantity of carbon deposited. On repeating the experiment with potassium purified in this manner by distillation, the powder which I obtained was not so dark brown-coloured, and when burned in oxygen gas it increased in weight 40 per cent. without giving off any carbonic acid. Its colour, however, was scarcely altered by the combustion. This circumstance may be explained by supposing, either that silicium possesses a lower degree of oxidation which is produced by calcination, or that (as happens with boron) the portion oxidized at the commencement of the process prevents the perfect oxidation of the remainder. The residue of the combustion was treated with fluoric acid. The silica was by this means separated, escaping in the state of silicated fluoric acid gas, and the colour of the undissolved powder became much deeper. Being now washed and dried, it constituted silicium in a state of purity.

Silicium obtained in this manner has a dark nut-brown colour, and is wholly destitute of metallic lustre. When rubbed upon a polishing stone, it does not communicate a shining streak. It is incombustible both in atmospheric air and in oxygen gas; and it appears to be highly infusible, for it undergoes no change in the flame of the blowpipe. This circumstance appears contradictory of what I have already stated respecting the easy combustibility of the silicium which is obtained immediately after its reduction by potassium. The difference between its properties in these two states is indeed highly remarkable; and I have fully ascertained that it is not occasioned, on the one hand, by the adherence of potassium, or, on the other, by the subsequent digestion in fluoric acid. Most probably, the combustibility of the silicium proceeds from its being combined with a small quantity of hydrogen, for if it be burned in oxygen gas, even after having been ignited in an atmosphere of hydrogen gas or in vacuo, there is invariably formed a certain quantity of water, although its amount indeed is inconsiderable when compared with the high saturating capacity of silicium. The silicium obtained by treating the reduced brown-coloured mass with water is therefore a hydruret. The reduced mass is originally a siliciuret of potassium, which is decomposed by the water; the potassium converted into potash, passes into solution; the greater part of the hydrogen separates in the state of gas, and a smaller portion enters into combination with the silicium. In the incoherent condition in which it is separated from potassium by the action of water, silicium may be compared to the loose tinder (a hydruretted carbon) prepared from linen, which may be easily kindled by a spark from steel; but after having been exposed to an elevated temperature, it may be compared to coke, which is, by itself, quite incombustible.

Silicium has an extreme tendency to soil even when in a state of dryness, and it adheres strongly to the glass vessel in which it is kept. It is a non-conductor of electricity.

The incombustible silicium is not altered by ignition with chlorate of potash. In nitre it does not deflagrate until the temperature has been raised so high that the acid undergoes decomposition, when the affinity of the disengaged alkali co-operates with that of the oxygen. In carbonate of potash it is oxidized with great readiness, and with intense ignition; carbonic oxide is at the same time disengaged, and the mass assumes a black colour in consequence of the reduced carbon. This property gives occasion to a very paradoxical phænomenon. If the incombustible silicium be moderately heated with nitre, no action ensues between the two substances; but if a bit of anhydrous carbonate of soda be now introduced, the silicium at the instant of contact is oxidated at its expense, and deflagrates in the midst of the nitre. The cause why silicium in lower temperatures is more easily oxidized at the expense of the carbonate than of the nitrate of potash undoubtedly exists in the circumstance, that the affinity of potash for silica is necessary to dispose it to combustion, and in the case of nitre, this co-operation is not obtained, except in the elevated temperature in which the acid of the salt undergoes decomposition.

Silicium deflagrates with brilliant ignition in the hydrates of the fixed alkalies; and the deflagration takes place as soon as the hydrate begins to fuse, and far below a red heat. It deflagrates also, but with less intensity, in the hydrates of barytes and lime. With the acid fluate of potash, it deflagrates in the low temperature necessary to produce the fusion of the salt; in melted borax it remains unaltered.

If silicium be heated in the vapour of sulphur, it catches fire and burns, but with much less brilliancy than in oxygen gas. The product is a white earthy looking substance, which, in contact with water, instantly dissolves, giving off at the same time sulphuretted hydrogen gas. The silicium is here converted into silica, which is taken up by the water; and if the quantity of that liquid be small, the resulting solution is so concentrated, that it gelatinizes after a very slight evaporation. In the open air this sulphuret gives off a strong odour of sulphuretted hydrogen gas, and rapidly loses the whole of its sulphur: in an artificially dried atmosphere, it may be preserved unaltered. When torrefied, it is converted, but not so rapidly as might have been anticipated, into sulphurous acid and silica.

It is certainly a most remarkable property that silica, at the instant of its formation in the humid way, should be so abundantly soluble in water, and that by evaporation of the solution it should lose this property so completely, that in the analysis of minerals, it is justly regarded as insoluble. This high degree of solubility enables us to understand the copious crystallization of silica in drusy cavities, where, not unfrequently, the volume of water could not possibly have greatly exceeded that of the crystals which it deposited.

I did not succeed in forming a phosphuret, by passing the vapour of phosphorus over ignited silicium.

When silicium is heated in a current of chlorine, it catches fire, and is rapidly volatilized. The product of the combustion condenses into a liquid, which appears to be naturally colourless, but which has a yellowish colour when it contains an excess of chlorine. This fluid is very limpid and volatile, and evaporates almost instantaneously in the form of a white vapour when exposed to the open air. It has a suffocating odour, not unlike that of cyanogen. It re-acts as an acid on litmus paper. This fluid is analogous to the combinations of the other electronegative substances with chlorine. It constitutes the second known example of a volatile compound of silicium. No combination ensued when silicium was heated in the vapour of iodine.

Silicium is neither dissolved nor oxidized by the sulphuric, nitric, or muriatic acids, nor even by aqua regia. While still combustible, it is slowly dissolved by fluoric acid, but even in this acid, it loses its solubility after having been ignited. On the contrary, it is readily dissolved in the cold by a mixture of the fluoric and nitric acids.

Silicium, after it has been insulated, possesses very little tendency to form alloys with metals. Copper, silver, lead, and tin, may be fused with it before the blowpipe, and the alloys, when dissolved in acids, leave behind an inconsiderable portion of silica. The alloy of copper leaves a skeleton of silica which retains the original form of the metal.*

Composition of Silica.—Having now succeeded in isolating the basis of silica, it was natural for me to investigate its composition by the direct synthetical process. With this view, 100 parts of pure silicium were ignited with carbonate of soda: the mass was treated with muriatic acid, evaporated to dryness, and the residue strongly ignited. Digested in water, this left a grey coloured silica, which, after washing and ignition, assumed a snow white colour, and weighed 203·75 parts. The liquid filtered from the silica was again evaporated to dryness, and the saline mass ignited. By dissolving the fused salt in water, there separated an additional quantity of silica, weighing, after ignition, 1·5 part. Consequently 100 parts of silicium had combined with 105·25 parts of oxygen. On repeating the experiment with a portion of silicium over which I had previously evaporated to dryness some fluoric acid, the augmentation of weight amounted to 108 per cent. According to these two experiments, silica is composed of

Silicium	48·72	48·08
Oxygen.	51·28	51·92

Both results indicate a larger proportion of oxygen than has been hitherto supposed to exist in silica. According to my earlier experiments, in which I deduced the composition of silica by determining its capacity of saturating saline bases, the quantity of oxygen was only 50·3 per cent.

The saturating capacity of silicium may be calculated also from the composition of the salts which contain fluate of silica. Of these, the most suitable for this purpose is the silicated fluate of barytes. 100 parts of this salt, fused with twice its weight of oxide of lead, lost 0·85 part of moisture. 100 parts, weighed out at the same time, yielded 82·933 parts of sulphate

* The method of preparing silicium, by decomposiug the double fluate of silica and soda, has been already described in the *Annals*, N. S. vol. viii. p. 122.

of barytes, equivalent to 54·428 parts of barytes. Now, I have already proved that in the double silicated fluates, the base is associated with thrice as much acid as in the neutral salt. Consequently, the silicated fluate of barytes which I analyzed was composed of

Barytes	54·428
Fluoric acid	22·836
Silica	21·886
Moisture	0·850
	100·000

These 54·428 parts of barytes were saturated by 7·612 parts of fluoric acid; the remaining 15·224 parts of this acid had been therefore combined with 21·886 parts of silica; or the fluate of silica is composed of

Fluoric acid	41·024	100
Silica	58·976	143·76

But 100 parts of fluoric acid imply the existence of 74·7194 parts of oxygen in the base by which it is saturated. Consequently, this quantity of oxygen must be contained by 143·76 parts of silica, and silica must be composed of

Silicium	48·025	100
Oxygen	51·975	108·22

This number corresponds very closely with that of the last synthetical experiment. If we suppose silica to contain three atoms of oxygen, the atomic number of silicium will be, according to the above analysis = 277·2, and according to the synthetical experiment = 277·8.

This number exceeds by $1\frac{2}{3}$ per cent. the number which has been hitherto adopted, and which corresponds so well with the most exact and the most recently performed analyses of pure minerals, that the present one, if made the basis of the calculation, would necessarily indicate in them an excess of silica: I must here mention, however, that we rarely find a mineral to whose constitution silica is even altogether foreign, which does not contain it to the amount of from one-half to upwards of two per cent. in the state either of quartz or of some other siliceous mineral; and this mechanical intermixture of silica is still more likely to exist in those minerals in which it constitutes at the same time an essential ingredient.

The *number* of atoms of oxygen contained by silica is still left undecided by the foregoing experiments. The circumstance that its carburet does not alter in weight when calcined affords indeed a presumption that silica belongs to the class of oxides which contain three atoms of oxygen; but our knowledge of the crystalline forms of bodies will require to be much farther extended, before it will be possible to deduce unequivocal conclusions respecting the number of atoms of oxygen which exist in oxides. The supposition that silica is constituted of an atom of each of its elements is unquestionably the simplest, and the most convenient for the purpose of expressing the composition of silicates by formulæ; but this view obliges us to admit the improbable existence of silicates in which the silica contains six times the oxygen of the base; as, for example, in apophyllite, in which one atom of potash would be combined with 12 atoms of silica.

B. *Fluoboric Acid.*

The characteristic properties of a strong and corrosive acid which the compound hitherto styled fluoboric acid possesses, have caused it, from our earliest knowledge of its existence, to be regarded as a double acid, which, with bases, has a tendency to form double salts, containing *two* acids and *one* base. This property indeed it possesses in a far higher degree than the silicated fluoric acid; but, as is the case with that compound, its most distinguishing tendency is likewise to produce double salts containing *one* acid and *two* bases, the boracic acid invariably constituting one of the latter. I shall hereafter demonstrate that it thus forms a class of salts, which are constituted in obedience to the same laws with the corresponding salts of silica.

Gay-Lussac and Thenard, and J. Davy, have stated, that fluoboric acid is absorbed by water without decomposition. This however is inaccurate; for I have ascertained that when the acid gas is passed into water, it deposits a considerable quantity of boracic acid, just as silicated fluoric acid deposits one third of its silica. If the liquid acid be cooled or very slowly evaporated, an additional quantity of boracic acid separates; but if it be concentrated in an elevated temperature, it volatilizes without leaving any residue, a proof that, in a certain degree of concentration, the compound which had been decomposed by water, is reproduced.

It would be impossible by direct analytical experiments to ascertain the composition of the gaseous fluoboric acid, or to determine the quantity of boracic acid which is disengaged by dissolving the gas in water, and indeed, without a previous knowledge of silicated fluoric acid, the composition of fluoboric acid, and the proportions of its combinations with other bodies, would present two of the most difficult problems which still remain to be resolved by chemical analysis; but in consequence of the complete analogy which subsists between the properties of these two substances, the simplest experiments suffice to demonstrate, that, with the exception of mere proportions, they are both constituted according to precisely the same laws.

Boracic acid has a more energetic affinity than silica for fluoric acid; nevertheless, it is incapable of producing a perfect decomposition of the fluate of silica. The gas obtained by J. Davy's process is invariably contaminated with fluate of silica; and as my attempts to precipitate the silica from it by means of boracic acid proved unsuccessful, I always prepared the fluoboric acid which I employed in my experiments by dissolving boracic acid to saturation in pure fluoric acid.

The methods which first occurred to me, as promising to disclose the composition of fluoric acid, were, to decompose the silicated fluate of potash or soda by boracic acid; to mix a solution of borax with fluoric acid, in the expectation of converting the whole quantity of the borax into a double salt; or to combine fluoboric acid directly with saline bases; but in all of them I experienced a total failure. It only remained for me to attempt the direct combination of a fluate with boracic acid; and by this synthetical process, I was fortunate enough to attain my object.

Borofluate of Potash.—This salt falls as a gelatinous precipitate when fluate of potash is mixed with a solution of borate of potash. By desiccation it assumes the form of a fine, mealy, white coloured powder. Its taste is weakly bitter, but not at all acid, and it does not redden litmus paper. It is anhydrous. 100 parts of cold water dissolve 1·42 part of the salt: boiling water dissolves it in considerably larger quantity. It is slightly soluble also in boiling alcohol. When ignited, it fuses and gives off fluoboric acid gas; but for complete decomposition it requires a much longer continued and more violent heat than the corresponding salt of silica. This salt is soluble in boiling hot solutions of the alkalies and their carbonates, and as the liquids cool, it crystallizes again unaltered.

Borofluate of soda is more soluble in water than the acid and neutral fluate of soda, and by slow cooling it crystallizes in large, transparent, four-sided rectangular prisms. This salt has a bitterish and weakly acid taste, and strongly reddens litmus paper. It contains no water of crystallization. It is sparingly soluble in alcohol.

Borofluate of lithia may be prepared by precipitating the borofluate of barytes with sulphate of lithia. It is very soluble in water, tastes like the salt of soda, and crystallizes in large prisms. In a moist atmosphere it deliquesces and runs to a liquid.

Borofluate of Ammonia.—When boracic acid is introduced into a solution of neutral fluate of ammonia, it is instantly dissolved; *ammonia is at the same time disengaged*, and may be detected by the smell. If no excess of acid had been employed, the salt obtained by evaporating the solution is the borofluate of ammonia. *It is undoubtedly remarkable that in these circumstances the boracic acid should be capable, like a base, of displacing ammonia;* but such is the operation of the combined affinities.

The dry salt may be sublimed without undergoing decomposition. Its taste resembles that of sal-ammoniac, and it reddens litmus paper. It is largely soluble both in water and in alcohol.

This salt is of a different constitution from the compounds which are produced by the neutral condensation of fluoboric acid and ammoniacal gases.

Borofluate of barytes is most readily obtained by adding carbonate of barytes in small quantities at a time to dilute fluoboric acid, until it ceases to be dissolved. By spontaneous evaporation of the solution, it crystallizes in four-sided rectangular prisms. This salt posseses an acid reaction, but its taste resembles that of the barytic salts in general, and is not in the least degree acid. In a temperature above 104°, it effloresces, and loses its water of crystallization. Alcohol decomposes it into a soluble acid salt and an insoluble pulverulent compound, whose composition I have not examined. The crystals contain 10·34 per cent. of water, whose oxygen is therefore double that of the barytes.

Borofluate of Lime.—A gelatinous mass, which has an acid taste, and reddens litmus paper.

Borofluate of magnesia is very soluble in water, and shoots during evaporation in large prismatic crystals. Its taste is bitter, like that of the other salts of magnesia.

Borofluate of alumina and *borofluate of yttria* are only soluble in water when assisted by an excess of acid; and by slow evaporation of the solution, they may be obtained in crystals.

Borofluate of oxide of lead shoots by spontaneous evaporation in short, four-sided, apparently rectangular prisms or tables, resembling the crystals of the borofluate of barytes. Its taste is at first sweet and astringent, but it finally leaves an impression of acidity. Both water and alcohol decompose it into an acid and a sub-salt.

Borofluate of oxide of zinc may be prepared by dissolving zinc filings in fluoboric acid. It is uncrystallizable and deliquescent.

Borofluate of oxide of copper is very soluble in water, and yields by evaporation a mass of bright blue-coloured acicular crystals, which are excessively deliquescent.

I come now to the investigation of the constitution of these compounds; for which, however, a knowledge of the saturating capacity of boracic acid is indispensable.

In my chemical tables, I have estimated the oxygen of boracic acid at 74·17 per cent., and its saturating capacity at 37·085. These numbers are founded upon my analysis of borate of ammonia, and of the crystallized hydrous boracic acid. The more recent analysis of L. Gmelin and Arfwedson, led me to distrust the accuracy of these determinations, and I attempted to reproduce a borate of ammonia, similar in constitution to the one which I originally analyzed. But all my trials with this view proved unsuccessful, and I suspect therefore that some error had been committed in determining the weight of the specimen employed in my first analysis.

To determine the composition of borax, I dissolved it in a mixture of the fluoric and sulphuric acids, and evaporated the solution to dryness; 2·634 grammes of the fused salt, decomposed by this process, yielded 1·853 gramme of sulphate of soda. 100 parts of borax contain therefore 69·173 parts of boracic acid and 30·827 parts of soda. The crystals, by fusion, lost 47·1 per cent. of water. According to these experiments, the crystallized salt is composed of

Boracic acid	36·59		
Soda	16·31	oxygen	4·1715
Water	47·10		41·889

The oxygen of the water is obviously 10 times that of the base. The proportion of the soda would probably be obtained most accurately by computation from that of the water, both because the latter is susceptible of a more rigid analytical determination than the former, and because any error in the quantity of the water would induce a corresponding error of only one-tenth the amount upon that of the soda. The composition of borax, according to this calculation, would be

Boracic acid	36·5248	100·
Soda	16·3753	44·8336
Water	47·1000	

These 44·8336 soda contain 11·4684 oxygen, which number indicates the saturating capacity of boracic acid in all the salts proportional with borax. The acid also must contain oxygen in some multiple of 11·4684.

M. Arfwedson analyzed no fewer than three distinct combinations of boracic acid and ammonia. His results were as follows:—

	(1)	(2)	(3)
Boracic acid	64·0	63·34	55·95
Ammonia	7·9	12·88	21·55
Water	28·1	23·78	22·50

In these salts the boracic acid is combined with quantities of ammonia which are equivalent, in other bases, to 5·734, 11·468, and 17·202 of oxygen:—numbers whose respective ratios are 1 : 2 : 3. In the borate of ammonia which I originally analyzed, the acid appeared to be combined with a quantity of base representing 34·4 of oxygen, which is six times the lowest degree of combination.

From the analysis of *native borate of magnesia*, M. Arfwedson deduced the saturating capacity of the acid to be 16·83, that is, very nearly 17·2. In the crystallized borate of potash, prepared from boracic acid and carbonate of potash, the saturating capacity of the acid proved to be 5·7, and when anhydrous boracic acid was fused with carbonate of potash, and the loss of weight in carbonic acid determined, it was found that 100 parts of boracic acid had combined with 139 parts of potash, whose oxygen amounts to 23·51. By a similar experiment with carbonate of soda, it was found that 100 parts of acid had combined with 135·5 parts of soda, which contain 34·66 of oxygen. These experiments therefore gave the following saturating capacities:

5·734	in the biborates of potash and of soda.
11·468	in borax, and in neutral borate of ammonia.
17·202	in boracite, and in borate of ammonia.
22·93	in subborate of potash.
34·40	in subborate of soda and of ammonia.

On comparing these numbers, we find that they are multiples of the lowest by 2, 3, 4, and 6.

Two methods presented themselves for a determination of the composition of boracic acid; namely, either an investigation of the relative proportions in which it combines with fluoric acid, or direct synthesis by the oxidation of boron. For the first of these, the analysis of the borofluates of barytes and of potash appeared to me most suitable.

100 parts of the salt of barytes yielded 10·5 parts of water, and 67·2 parts of sulphate of barytes = 44·10 per cent. of barytes. 150 parts of the salt of potash yielded 103·8 parts of sulphate of potash = 37·417 per cent. of potash.

My direct experiments on the oxidation of boron (to be related hereafter) rendered it probable that boracic acid contains 68·81 per cent. of oxygen; and this composition would correspond accurately with the analysis of the double salts, if we suppose them to be constituted in such a manner that the fluoric acid contains four times, and the boracic acid three times, the oxygen of the base, or, in other words, that the boracic acid is combined with thrice as much fluoric acid as the alkaline base.

To verify this composition, I dissolved in water 250·6 parts of crystallized bicarbonate of potash and 154·66 parts of crystallized boracic acid (the quantities which, according to the above supposition, represent an atom of each of the two substances), and added to the mixture as much fluoric acid as rendered it slightly acid. The solution, after having been concentrated, was found to contain no excess either of potash or of boracic acid, and when evaporated in a water-bath, it yielded to the last drop borofluate of potash. This was, therefore, the real constitution of the double salt.

We are now entitled to deduce the following conclusions with respect to the composition of the boracic and the fluoboric acids, and of the fluoborates.

Boracic acid contains six times as much oxygen as the soda with which it is combined in borax, that is, 68·8104 per cent. It is capable of combining with bases in such proportions, that its oxygen amounts to 12, 6, 4, 3, and 2 times the oxygen of the bases, and as in these combinations the multiples 12 and 6 occur far more frequently than any of the others, it appears highly probable that boracic acid contains 6 atoms of oxygen, and that the salts whose constitution is proportional with that of borax, are *neutral borates.* On this supposition, an atom of boron weighs 271·96, and an atom of boracic acid 871·96. The numerical composition of boracic acid is

Boron	31·1896	100·00
Oxygen	68·8104	220·62

The crystallized boracic acid contains, according to my early experiments, 44 per cent. of water, of which it loses one half when exposed to a temperature above 212°, and the second half when combined with a different basis. It follows from this that boracic acid is capable of combining in two proportions with water; and that in the first of these compounds the water contains as much oxygen as the acid; but in the second, only half that quantity. The numerical composition of these two hydrates is

Boracic acid	1 atom	72·1;	1 atom	56·38
Water	3	27·9;	6	43·62

Fluoboric acid, on the hypothesis that fluoric acid is an oxygen acid, is so constituted, that the two acids contain equal quantities of oxygen; that is, it consists of an atom of boracic acid and 3 atoms of fluoric acid. Its numerical composition is

Fluoric acid	47·942
Boracic acid	52·058

When fluoboric acid gas is absorbed by an excess of water, one-fourth of the boracic acid is disengaged: the compound thus formed consists of an atom of hydrous fluoric acid and an atom of fluoboric acid.

The *borofluates* are produced when in this compound the water is replaced by any other base, and they are so constituted, that *the base is associated in them with four times as much fluoric acid as in the neutral salt, and with a quantity of boracic acid, which contains thrice as much oxygen as the base.* These salts, therefore, contain an atom more of fluoric acid than the corresponding combinations of silica.

I now proceeded to give a final confirmation to these conclusions, by determining the quantity of oxygen which is absorbed by boron during its acidification; but after having made trial of various processes, I found it impossible to prepare boron in a state of such absolute purity, that the composition of boracic acid could by means of it be determined with more precision than by the indirect methods already described. The most successful experiment which I was able to make was one in which 0·035 gramme of boron was converted by ignition in a current of oxygen gas into 0·091 gramme of boracic acid, and according to which boracic acid ought to contain 61·5 per cent. of oxygen; but the boron here employed was contaminated with carbon, whose volatilization during the combustion would necessarily cause the apparent augmentation of weight to fall short of the truth.

Boracic Acid, and Fluate of Silica.—Silicated fluoric acid gas is not affected by dry boracic acid, but it is instantly absorbed by the crystallized hydrate. The product is a true chemical combination, in which the boracic and fluoric acids are so proportioned that they contain equal quantities of oxygen, and in which the fluoric acid is divided equally between the boracic acid and the silica. This substance does not smoke when exposed to the air, as would happen, were it a mere mixture of fluoboric acid and silica. Water decomposes it, and disengages about three fourths of the silica.

Fluoborates.—By this appellation I propose to designate the double salts, in which a *single* base exists in combination with boracic and fluoric acids. They appear to be produced when the foregoing compounds are saturated with the base, and to be capable of existing in a variety of proportions between the fluate and borate. I have not examined any of them minutely; *at present,* they possess too little interest, to induce one to encounter the difficulties which would attend an exact determination of the proportions of the two acids.

Boron.—The easiest and most economical method of preparing boron, is to decompose an alkaline borofluate by potassium. Boracic acid, even by protracted fusion, cannot be completely deprived of water, and it absorbs an additional quantity during pulverization; this is the cause why the reduction of boracic acid is accompanied by a rather violent detonation, and why a portion of the mixture is in general projected from the crucible. On the contrary, when the borofluate of potash has been sufficiently dried, the sound at the instant of the reduction is scarcely audible, and for every atom of potassium expended we obtain the corresponding quantity of boron. The only inconvenience attending this operation is the tedious edulcoration which is requisite in order to remove the undecomposed borofluate of potash: perhaps this disadvantage might be obviated by employing sodium and the borofluate of soda. The boron must be washed with a solution of sal-ammoniac, and finally with alcohol; because when pure water is employed for this purpose, a considerable portion passes in a dissolved state through the filter.

Sulphuret of Boron.—Boron is capable of forming a sulphuret, but, contrary to what has been hitherto supposed, no combination takes place between the two substances except in a temperature greatly exceeding the boiling point of sulphur. It takes fire and burns, when strongly ignited in the vapour of sulphur. The sulphuret is a white opaque mass. When put into water, it is rapidly converted into sulphuretted hydrogen gas and boracic acid; the liquid at the same time becomes more or less milky, in consequence of the precipitation of sulphur. I am disposed to think, from the observations which I have made, that boron is capable of combining in several distinct proportions with sulphur.

Chloride of Boron.—Sir H. Davy ascertained that boron even without the application of heat takes fire spontaneously in chlorine gas and undergoes brilliant combustion; but he did not examine the product of the combination. I have confirmed Davy's statement; if, however, the boron be very pure, and if it has been previously ignited moderately in vacuo, no combination takes place until heat is applied. The product of the combustion is *a new gas,* which, in contact with atmospheric air, smokes as strongly as fluoboric acid gas. It must be collected over mercury, which absorbs the excess of chlorine. This gas is colourless, and, in consequence of the formation of muriatic acid at the expense of the atmospheric humidity, it has a strong suffocating odour. It is rapidly, but not instantaneously, absorbed by water, and when the proportion of the water is small, a quantity of boracic acid is deposited upon its surface. Alcohol also dissolves it, and acquires the same odour of ether, as when it has absorbed muriatic acid gas.

Chloride of boron, when mixed with ammoniacal gas, condenses and forms a salt, which may be sublimed unaltered, but which is less volatile than sal-ammoniac. If the salt be moistened previously to sublimation, there remains behind a quantity of boracic acid. One volume of the gas condenses $1\frac{1}{2}$ volume of ammoniacal gas. Chloride of boron is composed of

Chlorine	90·743
Boron	9·257

Fluoric acid, unless aided by nitric acid, neither oxidates nor dissolves boron.

It has been affirmed that boron is dissolved in the dry way by *alkalies,* and that when the fused mass is treated with water, the boron is taken up by the alkaline liquid, and forms with it a yellow coloured solution. This is incorrect. When boron is ignited with an alkaline carbonate, it detonates at the expence of the carbonic acid ; and when it is ignited with the hydrate of a fixed alkali, hydrogen gas is disengaged with effervescence, and boracic acid is formed.

In the properties which have now been brought under review, boron possesses so close a resemblance to silicium, that the two substances may be associated with one another, in the same manner as we have been accustomed to associate arsenic with phosphorus and selenium with sulphur. The affinities of boron, however, are stronger, and in the lower temperatures, more active than those of silicium; thus, it detonates with nitre in a low red heat with such energy, that the explosion may be almost compared to that of gunpowder.

(To be continued.)

Article VIII.

On Forge Scales. By M. P. Berthier.*

When pieces of iron are heated to whiteness, in order to draw them out into bars, or roll them into plates, they become covered with a coat of oxide, which flakes off in scales by the blow of the hammer, or the pressure of the rollers. These are called by the workmen forge scales.

The thickness of the forge scales is proportionate to the time that the masses of iron on which they are formed have remained in the fire, but commonly it is from one to two millimetres ; (from $\frac{4}{100}$ to $\frac{8}{100}$ of an inch); they are of a shining black colour, with a semimetallic lustre; their structure is crystalline and presents intersecting laminæ, perpendicular to the surface of the scales. They are said to have been observed distinctly crystallized, in regular octohedra.

They are usually composed of two parallel laminæ, the outer one granular and blebby, the inner compact and crystalline. This structure forbids a doubt of their being liquified at a cer-

* From the Annales de Chimie.

tain period of their formation: nevertheless, although they become soft by an intense heat, we are unable to fuse them completely. It is probable that their fusion is effected by the local heat which is developed at the moment when the incandescent iron combines with the oxygen of the air, and which must necessarily be very intense, but as it is quickly dissipated, the matter soon becomes solid, and assumes a crystalline structure, if not cooled too suddenly. A similar phenomenon is seen in the combustion produced by striking fire with the flint and steel, in cupellation, and in several other instances.

The forge-scales are very magnetic. When reduced to grains of the size of a pin's head, they adhere to a magnet like metallic iron. Their specific gravity is 3·5; but, as they always contain some blebby cavities, this must be too low. Their powder is of a dull greyish black colour.

This oxide has hitherto been supposed to be the same as the native magnetic oxide, and that obtained by passing aqueous vapour over iron heated to redness. Having had occasion for some very pure oxide of iron, for some experiments on the silicates of that metal, I used the forge-scales, supposing them to be so; but I soon perceived that they do not contain so much oxygen as the magnetic oxide, which at present is considered as a *deutoxide.* For instance, when, for the purpose of preparing a proto-silicate of iron, I employed calculated proportions of forge-scales and iron filings, a certain portion of the metal always remained unoxidated; and when I reduced forge-scales by cementation in a black lead crucible, I constantly obtained a heavier button than the pure native oxide, similarly treated, would have given. I was therefore induced to inquire into the true composition of these scales; and the results of my experiments demonstrate that they are a new oxide, which, from the quantity of its oxygen, ranks between the protoxide and the native magnetic oxide.

This oxide does not produce any peculiar salt; it is decomposed by the acids into protoxide and peroxide, exactly like the deutoxide, and this property affords a very simple method of analyzing it, and is the one which I adopted. I treated the forge-scales with pure muriatic acid, in which they dissolve very readily, even in the cold, if the acid be concentrated, and the temperature of the liquid becomes considerably raised. I diluted the solution with water, and threw down the peroxide, by gradually pouring in carbonate of ammonia 'till the liquid was discoloured. This process is not attended with any difficulty; it gave me from 0·34 to 0·36 of peroxide, according to the scales I employed, and which I obtained from different forges, both from the tilt-hammer and the flatting-mill. The purest gave the largest quantity of peroxide. When I only obtained ·34, I always observed, at the moment of solution, a slight disengagemeut of hydrogen gas, which lasted only a few seconds, and evidently proceeded from some minute grains of metallic iron, accidentally mingled with the forge scales. The same scales assayed in the dry way with one-fifth of their weight of earthy glass, gave metallic buttons whose weight varied from 0·75 to 0·78. If we compare these results with those which an oxide composed of two atoms of protoxide, and one atom of peroxide would give, we find an almost perfect identity: for such an oxide would contain

Protoxide	0·642	— $2f^2$;	or Iron,	0·745	— 100
Peroxide	0·358	— F^3;	or Oxygen,	0·255	— 0·344·

I believe this therefore to be the true composition of the forge-scales.

According to these results, we must henceforth reckon four oxides of iron, in which the quantities of oxygen combined with the same quantity of iron are respectively : : 6 : 7 : 8 : 9.

This oxide is formed whenever iron is in contact with a more oxygenated oxide at a white heat, or when heated in contact with the air so as not completely to oxidate it.

It is necessary to observe that some forge-scales give, on analysis, much less than 0·35 of peroxide; but in that case they are not pure, but contain a mixture of scoriæ, which is discovered by the jelly it produces with concentrated acids. As these scoriæ are silicates of protoxide of iron with a great excess of base, the presence of from 0·02 to 0·05 of silica may diminish the proportion of peroxide nearly one-fifth. Perhaps it may be objected to my hypothesis of the composition of forge-scales, that a mixture of deutoxide and metallic iron, or its protoxide, would give the same analytical results as those which I obtained with my presumed new oxide; but if the scales be such a mixture, it is very extraordinary that the ingredients should always be found in the same proportion; I shall, however, obviate these objections by the detail of some facts, which in other respects also I think are not uninteresting.

If the forge-scales were a mixture of deutoxide of iron and metallic iron, they would contain 0·09 of the latter; but then their specific gravity would be much greater than it really is, since that of the deutoxide is more than 4·7, and that of iron 7·5. Moreover, when we treat a mixture of very fine iron filings and the pulverised deutoxide or peroxide, with muriatic acid, the iron dissolves before the oxide with evolution of hydrogen gas, and we find in the solution quite as much peroxide as there would have been without the admixture of the metallic iron. Hydrogen gas does not reduce this peroxide; now, since the forge-scales contain one half less of peroxide than of protoxide, we should admit, from that circumstance, that they contain half their weight of metallic iron, which it is impossible to suppose, since, when pure, they give no sensible quantity of hydrogen by the action of acids. Besides, if they contained half their weight of iron, they would give 0·85 of fused iron by the assay, which is far beyond what we obtain.

It remains to be ascertained if the forge-scales can be a mixture of protoxide and deutoxide. If that were so, since the protoxide is very greedy of oxygen, they should have a great tendency to combine with that body; whereas, not only are they wholly unalterable by exposure to the air, but are acted on even by concentrated and boiling acid only very slowly and with great difficulty. I endeavoured to determine their composition by this method, estimating the quantity of oxygen absorbed by the increase of weight; but I was unable to convert them entirely into peroxide. It is, moreover, very doubtful, if the protoxide of iron can exist in a free state; for being a base which has such attraction for oxygen, that it decomposes water, it is very difficult to obtain it absolutely uncombined. The dry way appearing to be the only means by which we can hope to succeed, I made several trials after that manner, but without success. The following process seemed the most likely to accomplish the object in a direct manner.

I took several black lead crucibles lined with charcoal, and placed 100 grammes (1544 grains) of pulverised and finely sifted forge-scales, in each; I then filled the crucibles with charcoal, and closed their mouths with covers, carefully luted on, and exposed them in a wind furnace to a heat of about 70° of the pyrometric scale. I took them out of the fire in succession—the first in half an hour, and the last in three hours, and compared the results. All the buttons had become solid, without changing their form or diminishing in volume; they were covered with a coating of metallic iron, and the oxide in the centre was neither fused nor altered; it gave the same relative proportion of peroxide and protoxide by analysis viâ humidâ, as at first. The thickness of the metallic coat was proportionate to the time the crucible had remained in the fire; the maximum thickness was five millimetres (nearly ·2 of an inch.) It has a peculiar aspect; its surface is dull, and fracture granular; its colour is grey, inclining to olive; it takes a brilliant polish by being rubbed against hard substances; it may be cut with a knife, and reduced, in that manner, to a very fine powder; it is as soft as lead, and has no elasticity; it flattens by a blow, and retains the mark of the hammer; its specific gravity, at the utmost, does not exceed one-third that of forged iron; it is, in fact, pure iron, minutely divided, and in a state analogous to that of spongy platina.

If the cementation has been continued for a considerable time, the section of the button presents, from the surface to the centre, first, a very thin layer of metalliic iron of a deep blue or black colour; secondly, a thick layer of iron, of an uniform colour, inclining to olive; thirdly, a layer with shades of black and olive, which soon passes to the pure, and slightly metallic black, of the scales. I examined the olive-coloured part, with the idea that it might probably contain a mixture of metallic iron and protoxide; but I found it composed wholly of reduced iron of the utmost purity, and there is every reason to think that it is even perfectly free from carbon. When treated with muriatic or sulphuric acid, it dissolves without leaving any residuum, and hydrogen gas is disengaged to the last. The last portions dissolved have the same aspect as the whole mass. When fused in a black lead crucible, either alone or with the addition of an earthy glass, instead of losing weight, as would happen if it contained protoxide, it increases from 0·01 to 0·02. The portion with shades of black and olive behaves like a mixture of metallic iron and forge-scales; in the moist way, red oxide is always found in it. This fact proves that metallic iron exerts no action on the oxide of the scales, and consequently that it is impossible to obtain the protoxide by heating any oxide with iron. The bluish coat of the buttons seemed to me to be steely iron, or to have passed to the state of steel, by the absorption of a certain quantity of carbon; but I have not positively ascertained the fact.

The cementation of the peroxide of iron presents as interesting, and more varied results, as the cementation of the forge-scales. If the mass be not very large, as long as any red oxide remains in the centre, no metallic iron is produced at the surface, but only black oxide. When the heat has been kept up a sufficient time, we find in the centre only magnetic oxide, and we may observe towards the surface, as in the cementation of the forge-scales, the bluish steel layer, the layer of olive-coloured iron, and the layer shaded with olive and black. The magnetic oxide in the centre is variable in its composition; in one experiment I found in it 0·48 of peroxide, and 0·52 of protoxide, and in another 0·60 of peroxide and 0·40 of protoxide. Since the native magnetic oxide contains 0·69 of peroxide and 0·31 of protoxide, it is obvious that the oxide in question must be a mixture, in variable proportions, of the magnetic oxide of the forge-scales, and native magnetic oxide.

It appears, from what we have seen above, that the peroxide of iron is changed by cementation, first, into an oxide similar to the native magnetic oxide, and that as soon as this change has been effected, its reduction begins from the surface to the centre, the process going on in such a manner that, in proportion as metallic iron is produced at the surface, the deutoxide of the forge-scales is formed in the interior of the mass, to its centre; but these proportions diminish from the surface to this point. Lastly, when the cementation is very far advanced, the button becomes covered with a layer of steely iron of appreciable thickness.

How does it happen in these experiments, that the oxide of iron is reduced without being in contact with carbon, and even when several centimetres (1 centimetre = 0·39 inch) distant from it? This is a question which in the present state of our knowledge imperiously demands an answer, and deserves to be considered. We might suppose that the effect is produced by the inflammable vapours from the furnace, which penetrate all porous substances; but it is easy to satisfy oneself that this is not the efficient cause, at least of the reduction of the oxides of iron into metallic iron. In fact, if we fill a crucible with red oxide of iron, placing a layer of charcoal below it at the bottom of the crucible, or if we place the oxide at bottom and cover it with charcoal; or lastly, if we introduce charcoal into the centre of a mass of oxide of iron, and heat it for an hour or two, we shall find that metallic iron is formed only in that part of the mass which was next the charcoal, and that there is not the slightest trace of it at the surface of the button in the other parts, although those parts were exposed, like all the rest, to the inflammable gases of the furnace.

The formation of the forge-scales on the surface of iron is quite as inexplicable as the reduction of the oxides by cementation. The oxidation of hot iron by the air is a gradual process, for the crust of the scales is much thicker on large masses, which require a long time to be heated, than on thin bars or plates, which heat much more quickly: now, as soon as a certain quantity of oxide is formed, it covers the iron like a varnish, and prevents its contact with the air; it must therefore attract its oxygen through the oxides, just as the oxides attract the carbon through the metallic iron.

These effects must have certain limits, which it would be important to ascertain, as they may perhaps furnish an explanation of the phenomena.

Article IX.

On the Specific Gravity of Hydrogen Gas, as modified by the Presence of Moisture. By Mr. Harry Rainy.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Glasgow, July* 9, 1825.

Dr. Thomson, in his First Principles of Chemistry, recently published, has adduced various new experiments in proof of the doctrine that the atomic weights of all substances are multiples by integer numbers of the atomic weight of hydrogen.

One of the most important of these experiments is related in vol. i. p. 67 to 76; and is intended to prove that the specific gravity of *dry* hydrogen is exactly $\frac{1}{16}$ of that of *dry* oxygen; but Dr. Thomson appears to me in this case to have been led into a very considerable error, by under-rating the quantity of vapour in the hydrogen. The hydrogen was disengaged at temperature 49°, at which, according to Dalton's table, the tension of vapour = 0·363 inch. Dr. Thomson supposes the specific gravity of vapour at 49° to be ·00533 compared with dry air at 60°, and under a pressure of 30 inches. It is easy to show, however, that ·00533 is nearly the specific gravity of vapour at *temperature* 212°, and pressure 0·363 inch; and that the specific gravity of vapour at *temperature* 49° and pressure 0·363, is considerably greater.

If the specific gravity of dry air at temperature 60° and barometer 30 = 1; the specific gravity of *vapour* at temperature 212° and barometer 30 will be 0·481, and the specific gravity of vapour at 212° and barometer 0·363 will be $0·481 \times \frac{0·363}{30} =$ 0·00582. To find from this the specific gravity of vapour at tension 0·363, and *temperature* 49°, we must consider that the vapour, if reduced in temperature from 212° to 49°, will, without condensing into a liquid, be reduced in bulk, like any of the gases, from 660 parts to 497;* and consequently its specific gravity will increase from 0·00582 to $0·00582 \times \frac{660}{497} = 0·00772$, *which is the true specific gravity of vapour at temperature 49°, and at the corresponding tension* 0·363.

Both Dr. Apjohn and Dr. Thomson have given erroneous formulæ for calculating the specific gravity of aqueous vapour, founded on the supposition, that if we take vapour at 212° and barometer 30 as the standard, the density of vapour *at any other temperature* is exactly proportional to the pressure which it supports, without any reference to the temperature.† This opinion is quite inconsistent with the properties of vapour, as is evident from the illustration which I have just given; it is also inconsistent with Gay Lussac's theory of volumes. Several years have now elapsed since that gentleman has shown, that a volume of aqueous vapour (*of any tension and temperature*) consists of one volume of hydrogen and half a volume of oxygen at the same tension and temperature. This is true at 212°, but it is equally true at 49° or any other temperature. The specific gravity of aqueous vapour is to the specific gravity of atmospheric air always as 0·625 to 1, the temperature and pressure

* Dalton and Gay Lussac have shown that 480 volumes of a gas at temperature 32°, expand to 497 at 49°, and to 660 at 212°. The same is true of vapours if not in contact with their liquids.

† *Annals of Philosophy*, N. S. vol. iii. p. 305 and 386.

being the same. We cannot indeed have vapour at temperature 49° and barometer 30 to compare with air at 49° and barometer 30; but we can have air at 49° and barometer 0·363 to compare with vapour at that temperature and pressure.

I consider it to follow as a necessary consequence from Gay Lussac's experiment on vapour, and his theory of volumes, that the following is the true formula for the specific gravity of aqueous vapour. Let the specific gravity of dry air at temperature 60° and barometer 30 = 1, and its volume = V; and let V′ be the volume of air, and p the tension of vapour at any other temperature, then the specific gravity of vapour at that temperature will be

$$1 \times \frac{V}{V'} \cdot \frac{p}{30} \times 0{\cdot}625.$$

If in this formula we substitute for V and V′ the numbers 508 and 497, which are the relative volumes at 60° and 49°; and if for p we substitute 0·363, which is the tension of vapour at 49°, we shall find the specific gravity of vapour of maximum tension at 49° $= 1 \times \frac{508}{497} \times \frac{0{\cdot}363}{30} \times 0{\cdot}625 = 0{\cdot}00772$, as by the former method.

If in Dr. Thomson's calculations, we substitute the number 0·00772 instead of 0·00533, which he adopts as the specific gravity of vapour at 49°, we shall find that 100 cubic inches of dry hydrogen weigh 2·0537 grains; and 100 cubic inches of oxygen weigh 33·915 grains at temperature 60° and barometer 30; and if we admit that in these circumstances 100 cubic inches of dry atmospheric air weigh 30·5 grains, we shall have the specific gravity of hydrogen = 0·0673, the specific gravity of oxygen = 1·111, and consequently *the specific gravity of hydrogen to the specific gravity of oxygen as* 1 *to* 16·54.

From this it follows, that if Dr. Thomson's experiment is correct (and of this we can scarcely doubt from the care and attention with which it was performed), it disproves the hypothesis that the specific gravities of all the gases are multiples by integer numbers, of the specific gravity of hydrogen. It is true that 16·54 does not differ from 16 by more than about $\frac{1}{32}$ of the whole, and that a very slight change in the number adopted for the specific gravity of hydrogen would account for the difference; but this merely shows how difficult it is to make any experiment sufficiently accurate to decide on the truth of the hypothesis.

Article X.

Analyses of Books.

An Attempt to establish the First Principles of Chemistry by Experiment. By Thomas Thomson, MD. FRS. Regius Professor of Chemistry in the University of Glasgow, &c. In Two Volumes, 8vo. 1825.

This work may be considered under two different points of view; first, as a collection of the principal facts upon which the important doctrine of definite proportions or atomic theory is founded; and, secondly, as containing numerous experiments confirming those which had been previously made, or supplying the deficiency which existed as to the atomic weights of various bodies, both simple and compound.

After a preface and advertisement, we are presented with an historical introduction of the atomic theory, occupying twenty-eight pages: in this sketch we think the author has fairly allotted to each philosopher the portion of merit due to him; there are, however, some statements which call for observation; and especially with respect to the substance by which the atomic unit is preferably represented—whether by hydrogen or oxygen. Dr. Thomson remarks, p. 14, " Mr. Dalton made choice of the atom of hydrogen for his unity; and in this he has been followed by Dr. Henry, of Manchester, and by one or two chemical gentlemen in London. But this method has been rejected by almost all the British chemists, and by all the chemists, without exception, in Europe and America." Has Dr. Thomson forgotten, that Sir H. Davy, in his Elements of Chemical Philosophy, has adopted hydrogen as unity? We shall not follow the author in all his arguments for preferring oxygen; the strongest of these, and in our opinion indeed the only one which possesses any weight, and that but little, is that " we see at once by a glance of the eye the number of atoms of oxygen which enter into combination with the various bodies." This fact the Doctor has illustrated by reference to that case which of all others best proves it, viz. the six oxides of manganese, for no other body forms so many oxides; but this facility is, we think, more than counterbalanced by the difficulty of seeing at a glance whether the proposed atomic weight of a body is probably the true one, by determinining whether it is a multiple of the atom of hydrogen by a whole number; thus oxygen being unity, fuming sulphuric acid, is represented by 11·125: now it requires an operation to discover that this is equivalent to $0{\cdot}125 \times 89$, and 89 is, in our opinion, a much more convenient number, and more likely to be retained in the memory than 11·125, unless by some process which we have not discovered, decimals are more easily remembered than whole numbers, and numerous figures than few.

With respect to the decimals included in those numbers which result from the adoption of oxygen as unity, Dr. Thomson observes: "Now surely it will not be said that the fractional numbers are more unwieldy or more unmanageable than the whole numbers; while in all cases of whole numbers, the advantage on the side of the latter method is very great. Thus if hydrogen be unity, the atom of uranium is 208, while if oxygen be unity, it is only 26."

Let it be granted for a moment that provided we have to remember a certain number of figures, it is indifferent whether they are decimals or whole numbers; and let us then examine in which mode the greater facility is obtained. Dr. Thomson's third table contains 408 substances hydrogen; being 1, 319 of these are represented by two figures, and 89 by three, and consequently no one by any greater number; but oxygen being unity, we have 58 bodies represented by one figure, 104 by two, 99 by three, 143 by four, and 4 by five figures. It will also appear that of 246 substances represented by three, four, or five figures, oxygen being unity, 200 are represented by two figures only, when hydrogen is the standard of comparison. The sixth table contains the atomic weight of 646 bodies; of these, 262 are represented by four, five, and 1 by six figures, of which not one would exceed three figures, hydrogen being 1.

These statements are, we think, sufficient to settle the question of facility. But in conversation, let any chemist inquire of another by what number he represents any given substance—let it be nitrate of manganese; if oxygen be unity, the answer will be 19·125. If hydrogen be the standard, the answer will be 153. Now this is not an extreme case; there are many such as will be readily imagined from what has been stated.

The author proceeds to the consideration of the relative and absolute weight of oxygen and hydrogen gases, and the composition of water. He discusses the question whether that fluid, according to the views of Sir H. Davy and Professor Berzelius, is a compound of 1 atom of oxygen and 2 atoms of hydrogen, as indicated by their respective volumes, or constituted of one atom of each of its elements. We need hardly observe that the author coincides with the views of most other chemists in adopting the latter opinion.

Several well imagined and executed experiments are related, all of which tend to confirm the opinion that an atom of hydrogen being 1, that of oxygen is 8, and of water consequently 9. This part of the subject had indeed been so completely settled in our opinion by the previous researches of the author and others, that it was scarcely requisite to perform fresh experiments to decide it. Indeed no difference whatever is to be found between the specific gravities of any gases either simple or compound in the author's present work, and those contained in the last edition of his System, except in five cases of the latter.

The recent experimental researches detailed are not founded upon more obvious or easy principles, but with the increased difficulty of complexity. Of this the last method employed to ascertain the atomic weight of ammoniacal and azotic gases offers an abundant proof. After having determined the atomic weight of azotic gas by the analysis of atmospheric air, nitric acid, protoxide and deutoxide of azote and nitrous acid, and having proved its weight to be 1·75, and having arrived at the same conclusion from the analysis of ammonia, the Doctor offers what he allows to be "a redundancy of evidence," as to the composition of ammonia, and consequently the atomic weight of azote. We shall give this as a fair specimen of very complex analysis, executed, we think, with great skill, and as offering confirmatory evidence of the facts which the operations are intended to illustrate.

"1. Oxalate of ammonia is a neutral salt, which crystallizes in beautiful transparent prisms. It is not very soluble in water. Its constituents I have found, by a careful analysis, to be

1 atom oxalic acid	4·5
1 atom ammonia....................	2·125
2 atoms water	2·25
	8·875

8·875 grains of this salt were dissolved in a small quantity of distilled water. 6·25 grains of pure calcareous spar (equivalent to 3·5 grains of lime) were dissolved in muriatic acid: the solution was evaporated to dryness, and the dry residual salt, constituting muriate of lime, was redissolved in a little water. The two solutions being mixed, a double decomposition took place, and oxalate of lime subsided to the bottom. As soon as the supernatant liquid had become quite clear, it was tested by oxalate of ammonia, and by muriate of lime; but was not rendered muddy by either of these reagents,—showing that it contained no lime nor oxalic acid. From this it is obvious, that 8·875 grains of oxalate of ammonia contain just the quantity of oxalic acid requisite to saturate 3·5 grains of lime. Now, 3·5 being the atomic weight of lime, the oxalic acid in 8·875 grains of the oxalate must be the equivalent of an atom, or 4·5; for it will be shown afterwards that 4·5 is the atomic weight of oxalic acid.

"The liquid from which the oxalate of lime had precipitated was neutral; hence the muriatic acid in the muriate of lime was just capable of saturating the whole ammonia in the 8·875 grains of oxalate of ammonia. Now, this muriatic acid weighed exactly 4·625 grains. And it will be shown in the next paragraph, that 4·625 grains of muriatic acid just saturate 2·125 grains of ammonia. This, therefore, is the quantity of ammonia in 8·875 grains of oxalate of ammonia.

"We have thus determined the weight of acid and ammonia in 8·875 grains of oxalate of ammonia. The surplus weight being undoubtedly water, it is obvious that the constituents of oxalate of ammonia are

1 atom oxalic acid	4·5
1 atom ammonia....................	2·125
2 atoms water	2·25
	8·875

The atomic weight of ammonia in this salt is undoubtedly 2·125.

"2. Sal-ammoniac, when newly sublimed, or when dried for some time upon the sand bath, is an anhydrous salt. It is neutral; and, therefore, a compound of one atom muriatic acid and one atom ammonia.

1 atom muriatic acid.	4·625
1 atom ammonia....................	2·125
	6·75

6·75 grains of pure dry sal-ammoniac were dissolved in water; 21·5 grains of pure anhydrous nitrate of silver were dissolved in another portion of water, and the two solutions were mixed. A double decomposition took place, and chloride of silver precipitated. As soon as the residual liquid had become clear, it was tested by nitrate of silver and common salt. Neither of these salts produced any effect, if we except an almost imperceptible opalescence which appeared when the common salt was added; but there was no precipitate whatever, even after the liquid had stood a week. From this experiment it is obvious, that 6·75 grains of sal-ammoniac contain just 4·625 grains of muriatic acid; for that is the quantity necessary to saturate the 14·75 grains of oxide of silver present in 21·5 grains of nitrate of silver. Hence the other constituent of the salt, the ammonia, must weigh 2·125, because that is the weight wanting to make up the full quantity of sal-ammoniac employed; and, as sal-ammoniac is neutral, and 4·625 the atomic weight of muriatic acid, 2·125 must be the atomic weight of ammonia.

"3. 13·5 grains of dry sal-ammoniac were wrapped up in blotting paper, and dropped into a retort filled with dichloride of lime (Mr. Tennant's bleaching powder), made into a thin paste with water. The whole retort and beak was then filled with water, and the beak of the retort was plunged into a water trough, under an inverted graduated jar, filled with water. As soon as the paper round the sal-ammoniac was sufficiently softened to allow the dichloride to come in contact with the salt, an effervescence took place, and azotic gas was disengaged. This is just the effect always produced when chlorine and ammoniacal gas come in contact. The lime which was in excess in the salt decomposed the sal-ammoniac; and the ammonia, as it was evolved, came in contact with chlorine, and was decomposed; the hydrogen uniting with the chlorine, and the azote being disengaged in the gaseous state. The action is so violent, if the dry sal-ammoniac be dropped at once into the retort, that it is difficult to collect the whole gas; but when the salt is wrapped in paper, the action is slow, and the gas may be all collected with the greatest facility. The azotic gas obtained in this process was 11·7 cubic inches, at the temperature of 47°, and when the barometer stood at 29·93 inches. This is equivalent to 11·853 cubic inches of dry gas, of the temperature 60°, and under a pressure of 30 inches mercury.

"This constitutes the whole amount of the azotic gas in 4·25 grains of ammonia, the quantity contained in 13·5 grains of dry sal-ammoniac. Now, 11·853 cubic inches of azotic gas weigh 3·5147 grains. Hence it follows, that the weight of the other constituent, the hydrogen, is 0·7353 grain. Consequently, ammonia is composed of

Azote	1·7573	or 1 volume
Hydrogen	0·3676	2·94
	2·1250	

The small excess of azote in this experiment was owing to a small admixture of common air with the azote, in consequence of the gas standing 24 hours over the water.

"The experiment was repeated seven times, in various ways, and the mean of the whole came exceedingly near 11·8 cubic inches of dry azotic gas from 13·5 grains of sal-ammoniac. This weighs 3·4993 grains, giving us the composition of ammonia as follows:

Azote	1·74965	or 1 volume
Hydrogen	0·37535	3·0028
	2·12500	

This analytical result of direct experiment comes within less than $\frac{1}{1000}$th part of the theoretical estimate; and, taken together with the preceding facts, can leave no doubt of the composition of ammonia."

At p. 150, in treating of the compounds of carbon and hydrogen, a statement occurs which both surprised and amused us. The author mentions his belief that no fewer than five different gases or vapours exist composed of one volume of carbon vapour and one volume of hydrogen gas. He says, "the first consists of

$$\left.\begin{array}{l}\text{1 volume carbon vapour}\\ \text{1 volume hydrogen gas}\end{array}\right\}\text{condensed into 1 volume.}$$

"Its specific gravity is 0·4861. One volume of it requires for complete combustion $1\frac{1}{2}$ volume of oxygen gas. After the combustion there remains one volume of carbonic acid gas.

"This peculiar gas has not yet been met with by chemists; but I see no reason to doubt its existence."

Now, in the name of the first principles of chemistry as established by experiment, we protest against the admission of so vague a conjecture in a work decidedly of a practical kind; and it is venturing much too far to state the specific gravity of a gas which exists only in the imagination; and equally objectionable is it to state the quantity of oxygen which it *does* require for combustion, instead of that which it *would* require if we could "*first* catch it." Under this head Dr. Thomson states his analysis of napthaline, by which it appears to consist of

$1\frac{1}{2}$ atom carbon.	1·125
1 atom hydrogen	0·125

In determining the atomic weights of silicon, an experiment is related, which we confess ourselves at a loss to comprehend: it is the following:

"About the middle of May, 1823, I fused a quantity of silica, with thrice its weight of anhydrous carbonate of soda, and digested the fused mass in a small quantity of water, till the silica assumed a flocky appearance. The whole was then thrown upon a filter; and the silica was washed repeatedly with distilled water, till no traces of soda could be found in the washings. In two days the filter with the silica became dry enough to be handled. I placed the filter on several folds of blotting paper, on a table in the middle of my laboratory, where it was allowed to remain for six weeks, without being disturbed. It may be necessary to mention, that the weather during the whole time was uncommonly cold; and I have reason to believe, that the temperature of the room scarcely ever exeeeded 60°, if it amounted to so much. When I returned to Glasgow, on the 24th of June, the thermometer in my laboratory stood at 57°. The silica, to the eye and the feel, appeared perfectly dry; it weighed 43·23 grains. By exposure to a red heat, it lost 10·55 grains, and was reduced to 32·68. Now, 32·68 : 10·55 :: 4 : 1·2913 = water combined with 4 silica. This exceeds 1·125 by 0·1663, which is rather more than one-seventh of an atom. This may be considered the greatest amount of the excess which ever remains."

Now the question which arises is this:—Can silica be separated from the soda with which it has been fused by the agency of water, and without the action of an acid? If the fact be so, it is quite new to us; but we cannot help thinking that some part of the operation is omitted to be stated.

The atomic weights of the fixed alkalies, alkaline earths, and their metallic bases, are all given as in the author's System, so that his experiments which merely confirm former statements call for no particular observations. Indeed with respect also to the metals the greater number, and all the more important ones, agree with those given in the System. There are, however, some variations and important additions in these bodies; such are the atomic weights of palladium, iridium, titanium, tungsten, and uranium, and of many compounds containing them. With respect to uranium, it is a curious fact that it has a strong disposition to form sesquisalts; with these additions, we may now consider the atomic weights of the metals as settled, except that of osmium.

While adopting the composition of the oxides of copper as usually given, Dr. T. has, and, we think, without sufficient reason, altered his opinion of their atomic constitution; he now considers the black oxide as a compound of an atom of oxygen 1, and one of copper 4; and the red oxide as a suboxide constituted of two atoms of copper 8, and one atom of oxygen 1.

It is remarked by the way, that such suboxides as that of copper "in general are incapable of constituting permanent salts with acids." Now as far as we know, no such suboxides exist, the only unquestionable one being the suboxide of silver formed by Mr. Faraday, and which consists of three atoms of silver and two of oxygen. It is indeed true that two suboxides of manganese have been mentioned, but their existence is much too problematical to serve as the basis of a general law; one of them indeed is stated to be incapable of combining with acids, and no proof has, we believe, been offered that the other unites with them.

When treating of the salts of copper, vol. i. p. 420, note, Dr. Thomson says, "It was the consideration of the salts of copper that induced me to adopt 4 for the atomic weight of copper; for if we represent the atom of copper by 8, all the salts of copper, without exception, will be bisalts, or will contain 2 atoms of acid united to 1 atom of oxide." This assertion appears to be of too sweeping a nature, unless the *soluble* salts only of copper be meant, and even then we question its accuracy. Among those which would consist of one atom of each of its constituents are those which Dr. Thomson calls disalts, there are the disulphate, dicarbonate, verdigris, and probably also the insoluble muriate and nitrate. If, however, the fact were so, that all the salts of copper were insoluble except the bisalts, this circumstance does not appear to be a sufficient cause for making so glaring an exception to the general rule, that when two oxides of a metal exist, the oxide which contains least oxygen is admitted to consist of one atom of each of its elements; and that which contains double, of two atoms of oxygen and one of base. By the method which Dr. Thomson has adopted, we have also the anomaly of a protoxide represented by a higher number than a peroxide. Thus while protoxide of mercury is represented by 208, and the peroxide by 216, the protoxide of copper is 72, and the peroxide 40.

It was our intention to have offered some observations respecting the number by which Dr. Thomson represents alumina, but we have extended this article to so considerable a length, that we have room only for one quotation more, and is that which forms the conclusion of Dr. Thomson's work, respecting an empirical law of Berzelius.

"Before concluding these general observations," observes Dr. T. "I may say a few words respecting Berzelius' law, that 'in all salts the atoms of oxygen in the acid constitute a multiple by a whole number of the atoms of oxygen in the base.' This law was founded upon the first set of exact analyses of neutral salts which Berzelius made. Now, as neutral salts in general are combinations of an atom of a protoxide with an atom of an acid, it is obvious that the atoms of oxygen in the acid must in all such salts be multiples of the atom of oxygen in the base; because every whole number is a multiple of unity. Neutral salts, therefore, are not the kind of salts by means of which the precision of this supposed law can be put to the test.

"Even in the subsalts, composed of 1 atom of acid united to 2 atoms of base, it is obvious enough, that the law will hold whenever the acid combined with the base happens to contain 2 or 4, or any even number of atoms; because all even numbers are multiples of 2. Now, this is the case with the following acids:

Phosphoric,	Nitrous,	Antimonic,	Citric,
Carbonic,	Titanic,	Manganesic,	Saclactic,
Boracic,	Arsenious,	Molybdous,	Chromous.
Sulphurous,	Selenic,	Uranitic,	

Consequently, the law must hold good in all combinations of 1 atom of these acids with 2 atoms of base.

"In the case of all those acids which contain only 1 atom of oxygen, all the subsalts composed of 1 atom of the acid united to 2 atoms of base, the law will also in some sort hold; for the atoms of the oxygen in such acids being 1, this number will always be a submultiple of 2, the number of atoms of oxygen in 2 atoms of base. This is the case with the following acids:

Silicic,	Hyposulphurous,
Phosphorous,	Oxide of tellurium.

It is only in the subsalts of acids containing an odd number of atoms of oxygen, that exceptions to the law can exist. It is to them, therefore, that we must have recourse when we wish to determine whether this empyrical law of Berzelius be founded in nature or not. Now, there are thirteen acids, the integrant particles of which contain an odd number of atoms of oxygen. The following table exhibits the names of these acids, together with the number of atoms of oxygen in each.

	Atoms of oxygen.		Atoms of oxygen.
Sulphuric acid	3	Acetic acid	3
Arsenic	3	Succinic	3
Chromic	3	Benzoic	3
Molybdic	3	Nitric	5
Tungstic	3	Tartaric	5
Oxalic	3	Hyposulphuric	$2\frac{1}{2}$
Formic	3		

"Now, although the number of subsalts which I have examined is exceedingly small, because my object was not to investigate the truth of Berzelius' law, but to determine the quantity of water of crystallization which the salts contain, yet there occur several which are inconsistent with Berzelius' law. This is the case, for example, with the disulphate of alumina, the atoms of oxygen in the base being 2, and those in the acid 3. The following subsalts are precisely in the same predicament:

	Atoms of oxygen in base.	Ditto in acid.
Dinitrate of alumina	2	5
Trisnitrate of alumina	3	5
Diprotarseniate of iron	2	3
Dinitrate of lead	2	5
Diacetate of lead	2	3
Diacetate of copper (verdigris)	2	3
Dinitrate of bismuth	2	5

These examples comprehend not only nitric acid, which Berzelius has recognised as an exception to his law; but likewise, sulphuric acid, arsenic acid, and acetic acid.

"It would certainly be a most remarkable circumstance if 2 atoms of any protoxide were incapable of combining with 1 atom of any of the 13 acids in the preceding list. I have given seven examples of such combinations; and am persuaded that many more will be discovered whenever the attention of chemists is particularly turned to the subsalts.

"There is another kind of saline combination in which exceptions to the law of Berzelius may also be looked for; I mean those salts which I have distinguished by the epithet *sesquisalts* or *subsesquisalts*. In the sesquisalts, $1\frac{1}{2}$ atom of acid unite with

1 atom of base; or, which comes to the same thing, 3 atoms of acid unite with 2 atoms of base. In the subsesquisalts, $1\frac{1}{2}$ atom of the base unite with 1 atom of the acid; for example, the *sesquicolumbate* of barytes is composed of

3 atoms columbic acid,	containing 3 atoms oxygen.	
2 atoms barytes	2	

Here we see, that the oxygen of the acid is not a multiple of that in the base.

"When the acid contains 2 atoms of oxygen, and the base 1 atom, it is plain that the sesquisalts must all come under Berzelius' law; because $1\frac{1}{2}$ atom of acid will contain 3 atoms of oxygen, and 3 is, of course, a multiple of 1; but in acids containing 1 or 3 atoms of oxygen, the law of Berzelius cannot hold.

"With respect to the subsesquisalts they will all come under Berzelius' law when the acid happens to contain 3 atoms oxygen, and the base only 1 atom; but they will deviate from it whenever the acid contains 1 or 2 atoms of oxygen.

"Upon the whole, though the subsalts and sesquisalts have not been sufficiently investigated to enable us to decide upon the point with perfect certainty; yet from what we do know, there appears sufficient evidence that Berzelius' rule cannot be considered as a general chemical law; and that we run the risk of falling into most egregious mistakes, if we make use of such a law in calculating the atomic weight and chemical constitution of the acids or bases. I pointed out some remarkable examples of this error when treating of uranium, to which it is merely necessary to refer the reader."

In concluding our remarks, we may observe, that we have freely expressed our differences of opinion with the author on certain subjects; to this we are sure he will not object, more especially as they are mostly matters of opinion, from which we have withheld our assent. His method of experimenting appears to us liable to exception in very few cases; the work must form a part of every chemical library, and will be referred to as a standard by those who wish to acquire information as to the atomic weights of bodies, or a knowledge of the experimental means of ascertaining them.

Article XI.

Proceedings of Philosophical Societies.

LINNEAN SOCIETY.

April 19.—The reading of the Rev. Messrs. R. Sheppard's and W. Whitear's Catalogue of the Birds of Norfolk and Suffolk was continued.

May 3.—Prof. F. A. Bonelli, and Mons. C. S. Kunth, were elected to fill the two vacancies in the list of Foreign Members of the Society; and the reading of the Catalogue of Norfolk and Suffolk Birds was concluded. Annexed to this catalogue was a table of the times of migration of various birds, as observed at several places in the above counties during a séries of years.

May 24.—The Anniversary Meeting of the Society was held this day at one o'clock, Sir J. E. Smith, *President*, in the Chair; when the following members were chosen Officers and Council for the ensuing year.

President.—Sir J. E. Smith, Knt. MD. FRS.

Vice-Presidents.—Samuel, Lord Bishop of Carlisle, LLD. VPRS.; A. B. Lambert, Esq. FRS.; W. G. Maton, MD. FRS.; Edward, Lord Stanley, MP. FHS.

Secretary.—J. E. Bicheno, Esq.

Assistant-Secretary.—Richard Taylor, Esq. MAS.

Treasurer.—Edward Forster, Esq. FRS.

Council.—Edward Barnard, Esq. FHS.; Robert Brown, Esq. FRS; H. T. Colebrooke, Esq. FRS.; Edward Horne, Esq.; Charles Konig, FRS; Daniel Moore, Esq. FRS.; Rev. T. Rackett, MA. FRS.; and J. F. Stephens, Esq.

The Society afterwards dined at the Freemasons' Tavern, where the presence of Sir J. E. Smith, in improved health, added much to the enjoyment of the day. Addresses on subjects interesting to the cultivators of natural history were delivered by various members and other men of science: amongst others, by the venerable Bishop of Carlisle, Lord Stanley, the Rev. Dr. Fleming, and the respective Presidents of the Horticultural and Geological Societies. Numerous expressions of respect and cordial esteem were called forth towards the late Secretary of the Society, Alexander Mac Leay, Esq. FRS. on the occasion of his quitting this country for a time, to occupy the important station of Colonial Secretary in New South Wales.

June 7.—Some communications were read from Lieut. J. H. Davies, and Charles Willcox, Esq. relative to a species of *Mitylus*, stated by them to be *M. bidens*, found in great quantity adhering to the bottom of his Majesty's ship Wellesley, built at Bombay, and which has been lying in Portsmouth Harbour ever since 1816. It seems to be quite naturalized there, and to propagate abundantly. A paper was also read on the Crepitacula and Organs of Sound in Orthopterous Insects; and particularly in the *Locusta camellifolia*, a description of which is subjoined; by the Rev. Lansdown Guilding, BA. FLS.

June 21.—The following papers were read:—A Descriptive Catalogue of the Australian Birds in the Cabinet of the Linnean Society; by Thomas Horsfield, MD. FLS. and N. A. Vigors, Esq. FLS.: communicated by the Zoological Club of the Linnean Society. In the introductory remarks to this Catalogue, most of the species described in which are of great interest, the writers express their confident expectation that the deficiency of our knowledge of the habits of the birds of Australia will be in great measure supplied by the exertions of Mr. A. Mac Leay, during his future residence in that interesting country.—A Notice on a peculiar Property of a Species of Echinus; by E.T. Bennett, FLS.: communicated by the Zoological Club.

The Society then adjourned to the 1st of November next.

GEOLOGICAL SOCIETY.

May 6.—A paper was read, entitled "A Brief Description of an extensive Hollow or Fissure, recently discovered at the Quarries near the Extremity of the Western Hoe, Plymouth; by the Rev. Richard Hennah."

In this communication the author describes an extensive hollow or cave in the limestone rocks near Plymouth, in which no remarkable bones have yet been discovered, but in which stalactites are particularly abundant. Mr. Hennah offers some remarks on the various causes and circumstances which have contributed to give to these stalactites their different shapes and compositions.

A paper entitled "On a Dyke of Serpentine cutting through Sandstone in the County of Forfar;" by Charles Lyell, Esq. Sec. GS. was read in part.

May 20.—The reading of Mr. Lyell's paper was concluded.

In the former part of this paper, the rocks which are exposed on the left bank of the Carity, a small river in Forfarshire, which descends from the mica-schist district of the Grampians into Strathmore, are described. The first of these is a claystone porphyry, next to it is a conglomerate containing quartz pebbles, and then strata of fine grained micaceous sandstone and shale, dipping to the south, and which are suddenly cut off at an angle by the serpentine. These strata of sandstone and shale form part of a great series which overlies the clay slate to which it immediately succeeds, and is older than the great conglomerate of the old red sandstone which lies immediately upon it. The serpentine is vertical and is well characterized. It contains in part veins of asbestos, and in parts diallage, and a large mass of hypersthene.

On the other side of this dyke of serpentine, which is 90 yards thick, fine grained sandstone and conglomerate again appear and dip away from the serpentine towards the S. Next to these a mass of serpentine is seen mixed with dolomite, and at its side altered sandstone and a conglomerate in which the quartz pebbles are split and re-united by ferruginous matter. Lastly, at a short distance a dyke of greenstone parallel to the serpentine occurs flanked on both sides by vertical masses of sandstone and conglomerate much altered and indurated, and charged with brown spar.

Mr. Lyell next describes the rocks on the right bank of the river, which resemble those on the left with one exception, viz. that the great dyke of serpentine seems to be connected with the mass of dolomitic serpentine, a thin bed of fine grained greenstone alone intervening, and the sandstone and conglomerate which appeared between them on the opposite side being absent.

In conclusion the author traces this dyke of serpentine pursuing, its course in a direct line to the north-east and south-west of the locality in which it occurs on the Carity. It is found recurring at intervals for the space of at least 14 miles from the bridge of Cortachie to Bamff, near Alyth, in Perthshire.

It is always unconformable to the strata through which it passes, and its course is never interrupted by any other rock.

A notice was then read "On the Serpentine of Predazzo;" by J. F. W. Herschell, Esq. Sec. RS.

In this communication, the author mentions that at Canzocoli, near Predazzo, in the Tyrol, where a junction is seen of a granitiform sienite with dolomite, a layer of serpentine is found to intervene between the sienite and the dolomite.

The dolomite dips at an angle of 50° or 60° beneath the sienite, and near the junction an alteration takes place in its mineralogical character; as it presents, instead of its usual highly crystallized saccharine structure, a flaky and very talcose appearance. The incumbent sienite is no less affected. Its grain is smaller, and it is intersected with innumerable veins parallel to the plane of junction of a white mealy substance, which partly dissolves with effervescence, and partly gelatinizes with nitric acid. In the midst of this white substance occurs the thin lamina of serpentine, which is extremely well characterized.

The whole of the transition from the sienite to the dolomite takes place within a thickness of about 18 inches or two feet.

A notice was read on Carbonate of Copper, occurring in the Magnesian Limestone at Newton Kyme, near Tadcaster; by W. Marshall, Esq. MGS.

The green carbonate of copper, found by the author in a large quarry of magnesian limestone near Tadcaster, runs through the limestone in thin veins dipping to the west; the dip of the limestone being in the same direction, but at a less angle. At Farnham, a small village two miles north-west of Knaresborough, which is also in the magnesian limestone, a considerable quantity of copper was formerly obtained, and these are the only two instances in which Mr. Marshall has heard of any of the ores of copper having been found in the magnesian limestone.

Article XII.

SCIENTIFIC NOTICES.

Magnetism.

1. *Queries respecting Animal Magnetism.* By a Correspondent, in a Letter to Mr. Children.

MY DEAR SIR, *Cambridge, July* 18, 1825.

Though many experiments, and some of them of very recent date, have been made on the gymnotus electricus and other fishes having similar powers, yet I am not aware that it has as yet been ascertained whether, and to what extent, they may be possessed of electromagnetic properties. If this *animal* electricity be similar to *common* electricity, it is to be expected that it will be capable of magnetising a needle inclosed in a spiral, but not of causing deviation in the galvanoscope; if it resemble galvanism, we may expect both effects. From the experiments made by Mr. Cavendish in reference to the Raia Torpedo, it appears that its electricity was most nearly imitated by that of a large extent of coated surface charged to a very low intensity; that "the quantity of electricity was extremely great," and that "it was gradually transferred from one side to the other." I should, therefore, anticipate from the torpedo, magnetic action resembling that from galvanism; and by analogy, similar effects may be expected from the Gymnotus electricus, Silurus electricus, Tetraodon electricus, and Trichiurus Indicus.

Should any of your readers have the opportunity to resolve these questions, they will, I hope, consider them sufficiently interesting to deserve their attention.

Believe me, my dear Sir,

Very truly yours, J. C.

Zoology.

2. *On the Anatomical Difference between Helix Hortensis and H. Nemoralis.* By J. E. Gray, Esq.

There has been a difference of opinion among the various English and Continental zoologists respecting the permanency of the distinction between *Helix Hortensis* and *H. Nemoralis*, which certainly at first sight appear very distinct, both on account of the small size, thinness, and more polished surface, as well as the white lip of *Helix Hortensis;* but no one has yet taken any notice that there exists a difference in the form of that part of the generative organs of the shell called *visicula multifida* by Cuvier in his dissection of *Helix Pomatia;* in one (*H. Nemoralis*) it is much more lobed than in the other; Cuvier's name for this organ is bad, as in several of the Helices it is singly-forked, in others doubly-forked, and rarely many cut. This variety existing in the several organs is curious, as I am informed by a friend, whose experiments I hope will be shortly published, that the two species will breed together. Pioret perhaps had the knowledge of this fact when he named the brown mouth (variety of Helix Nemoralis) as a species, with the name of *H. Hybrida.*

3. *On Siren Lacertina.*

Rusconi having observed that the lungs of the *Siren Lacertina* were extended to the end of the abdomen, and that these organs only did so in the larva of the salamander, used this fact as an argument that the *Siren* was only a larva; but Mr. Grauenhorst has weakened the position by observing that the lungs of the perfect salamander are sometimes similarly extended.—(*Iris*, 1824, 673.)

4. *On the Animal of Argonauta.*

It has been a matter of considerable dispute amongst the modern zoologists to know if the animal usually found in the *Paper Nautilus* described by Aristotle and Pliny, was the real former of the shell, or only a parasitical inhabitant similar to the soldier crab, &c. *Dr. Leach, Mr. Say,* and *M. Blainville* were of the latter opinion, apparent with great reason, *Cuvier* and *Dumeril* combated their opinions; and lately *Baron Ferussac, M. Ranzani,* and the celebrated Sicilian naturalist, *Poli,* has supported the opinion of the latter authors. The strongest fact brought forward in the support of their position, is that both Mr. Duvernoy and Poli have discovered the existence of the shell on the embryo found in the eggs attached to the animals, which are said to be the true inhabitants of the shell. Sir E. Home in his paper (in the *Phil. Trans.*) appears to refer to the observations of the former, when he observes, that the yolk must have been mistaken for the shell.

Poli agrees with Aristotle and Dr. Leach that the animal has no muscular attachment to the shell; which was the chief argument used by the latter, that it was not its real builder, and indeed the latter, on the authority of the late Mr. Cranch, states that they sometimes swim about without their shells, and even exchange them for others. The fact with regard to the egg requires to be verified. Would it not be a fit subject for the pencil of Bauer?

The want of the muscular attachment of the animal to the shell is an anomaly amongst Mollusca, as is also a truly external and celled shell amongst Cephalissodes. Indeed the form and structure of the shell gives reason to believe that its former is more nearly allied to the genera *Carinaria* and *Firola*.—J. E. G.

5. *On the Animal of Calyptræa.*

Messrs. Deshayes, (*Annals Sci. Nat.*) and Deslonchamps, (*Rev. Encycl.*) have lately examined the animal of the genus *Calyptræa* of Lanark, (Patella China noistain) and found it very similar to that of the genus *Crepadula* dissected by Cuvier; indeed it only differs slightly in the position of the gills and abdominal viscera caused by the more orbicular form of the shell. Their account agrees with the dissection I made three years ago, and proves that the two above-named genera are exceedingly allied.

6. *On the Genus Plagiostoma.*

M. De France has lately divided the genus of Plagiostoma as established by Mr. Sowerby into two genera. The first, for those species found in the chalk (as P. spinosa and P. Hoperi, *Sow.*) which he conceived to be allied to Terebratula; he has given the name of *Pachyta* with the following characters: shell bivalve, regular; hinge, toothless; the cardinal edge of one valve straight: of the other deeply cut with a triangular sinus, for the passage of the tendenous pedicle. Secondly, for those found in the more ancient strata he keeps the name Plagiostoma, and gives for the genus the following character; shell bivalve, inequilateral, slightly eared; cardinal edge, transverse, straight, umbones rather distant. Hinge toothless, with a conical ligament cavity situated under the umbones. These shells are usually very thin, and M. De France considers them to have lived in the glush of the sea-shore, as they are usually filled with a fine paste; the genus appears to be allied to Lima, and consequently to the Family *Pectenidæ*.—J. E. G.

7. *On Fossil Elks.*

Dr. Hilbert has given two interesting papers on the Fossil Elk discovered in the marsh pits of the Isle of Man; in which he attempts to prove that the bones are post diluvian, and that these animals mostly died by a natural death; and that one of the chief reasons of their extermination is the gradually filling up the lakes they formerly inhabited. He is apparently not aware that the *Irish Elk* had twice before been described as distinct from the common one, under the name of *Cernus giganteus* by Blumenbach, and *C. Hibernicus* by Desmarest; as he proposed to designate that species, which he considers distinct from the Isle of Man one, under the name of *C. Euryceros*, thinking it may be the *Euryceros* of Appian.

If the Manse Elk should be distinct from the Irish species, it ought to have a new specific name.

8. *Fossil Crocodile from Whitby.*

The Rev. Mr. G. Young has given a description of a specimen of crocodile found in the alum shale in the neighbourhood of Whitby, by Mr. Brown Marshal, which was purchased by the Whitby Literary and Philosophical Society.

The length of the animal, which is a species of Gavial, is 14 feet 6 inches following the curvature of the spine, but when it was alive it must have been more than 18 feet long.

A head of the same species has been figured as an *Ichyosaurus* in the *Geological Survey of the Yorkshire Coast*, p. 16, f. 2.—(Edin. Phil. Jour. 1825. 76.)

Miscellaneous.

9. *Mr. Herapath on the Author of an Erroneous Solution of* $\psi^n x = x$.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Cranford, July* 16, 1825.

In your *Annals* for November, 1824, p. 323, I have mentioned Mr. Herschell as the author of an erroneous solution of

$$\psi^n x = x,$$

extracted from Mr. Babbage's paper, Philos. Transac. 1815. I came to this conclusion from Mr. B.'s observations in the 9th and 10th problems of his paper, and an allusion with Mr. Herschell's name in the 19th problem. Having, however, received a letter from Mr. Herschell in which he informs me that he is not the author, I beg you will have the goodness to say so in your next. I am, Gentlemen, your humble servant,

J. Herapath.

10. *Luminous Snow Storm on Lochawe.*

Towards the latter end of March, in the year 1813, a shower of snow fell on Lochawe, in Argyleshire, which alarmed or astonished those by whom it was witnessed, accordingly as they were influenced by curiosity or superstition. Some gentlemen who had crossed the lake in the morning, had a good opportunity of marking the phænomenon. All had been calmly beautiful during the day, and they were returning homewards from Ben Cruachan when, the sky becoming suddenly gloomy, they rowed more smartly towards the shore in order to avoid the threatened storm. In a few minutes, however, they were overtaken by a shower of snow; and immediately after, the lake, which was of glassy smoothness, with their boat, clothes, and all around, presented a luminous surface, forming one huge sheet of fire. Nor were the exposed parts of their bodies singular in this respect, for to the eye they all seemed to burn, although without any feeling even of warmth. When they applied their hands to any of the melting snow, the luminous substance adhered to them as well as the moisture, and this property was not lost by the snow for twelve or fifteen minutes. The evening became again mild and calm, but lowering and very dark. The natives had not witnessed any similar appearance before; and many of them believed it the forerunner of some dire calamity that was to befal their mountain land. *Rev. Colin Smith.*—(Edin. Phil. Jour.)

11. *Mr. Mackintosh's Process for rendering impervious to Water and Air all Kinds of Cloths; also Leather and Paper, &c.*

This very valuable process, which we owe to the ingenuity of our countryman Mr. Charles Mackintosh, consists in joining the surfaces of two pieces of cloth by a flexible varnish, made of caoutchouc dissolved in the naptha obtained from the distillation of coal. The caoutchouc, after being cut into thin shreds, is steeped in the varnish composed of twelve ounces of caoutchouc to one wine-glass full of the oil. Heat may be applied, and the thick varnish must be strained through a sieve of wire or horsehair. The cloth is stretched on a frame, and then covered by means of a brush with a coat of the elastic varnish. When the varnish has become sticky, another piece of similar cloth, similarly varnished, is laid upon the first, the surfaces being placed face to face; and to promote the adhesion, they are pressed between a pair of plain rollers, and then dried in a warm room. This cloth, of which we have now several very fine specimens before us, besides being used for outer garments to keep off rain, will be found highly useful for various purposes in the arts and sciences.—(Edin. Jour. of Science.)

Article XIII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

A Treatise on Volcanoes, and their Connexion with the History of the Globe. By G. P. Scrope. 8vo.

A Course of Studies in Plane Geometry. By T. S. Davies, Private Teacher of Mathematics, Bristol.

Materia Indica, or some Account of those Articles which are employed by the Hindoos and other Eastern Nations, in their Medicine, Arts, Agriculture, and Horticulture. By Whitelaw Ainslie, MD. MRAS. late of the Medical Staff of Southern India. 8vo.

JUST PUBLISHED.

An Historical and Descriptive Narrative of Twenty Years' Residence in South America; containing Travels in Arauco, Chile, Peru, and Columbia; with an Account of the Revolution. By W. B. Stevenson. 3 vols. 8vo. *2l. 2s.*

Practical Remarks upon Indigestion, particularly as connected with Bilious and Nervous Affections of the Head, &c. Illustrated by Cases. By John Howship, MRCS. &c. 8vo. *7s.*

Rennie on Gout. 8vo. *5s. 6d.*

Welbank on Syphilis. 8vo. *7s. 6d.*

The Theory and Practice of Warming and Ventilating Public Buildings, &c. 20 Plates. 8vo. *18s.*

Lynn's Nautical Tables. Royal 8vo. *2l. 2s.*

Article XIV.

NEW PATENTS.

J. Fox, Plymouth, Devonshire, rectifying distiller, for an improved safe to be used in the distillation of ardent spirits.—May 14.

C. Macintosh, Crossbasket, Scotland, for a new process for making steel.—May 14.

J. Badams, Ashted, near Birmingham, chemist, for a new method of extracting certain metals from their ores, and purifying certain metals. —May 16.

I. Reviere, Oxford-street, gunmaker, for an improved construction, arrangement, and simplification of the machinery by which guns, pistols, and other fire-arms are discharged.—May 20.

W. H. James, Coburg-place, Winson-green, near Birmingham, engineer, for certain improvements in apparatus for diving under water, and which apparatus is also applicable to other purposes.—May 31.

J. H. Sadler, Hoxton, Middlesex, machinist, for an improved power loom for the weaving of silk, cotton, linen, wool, flax, and hemp, and mixtures thereof.—May 31.

J. F. Ledsam, merchant, and B. Cook, brass-founder, both of Birmingham, for improvements in the production and purification of coal gas.—May 31.

J. Crowder, New Radford, Nottingham, lace net manufacturer, for improvements on the Puslew bobbin net machine.—May 31.

J. Apsdin, Leeds, bricklayer, for a method of making lime.—June 7.

C. Powell, Rockfield, Monmouthshire, for an improved blowing machine.—June 6.

A. Bernon, Leicester-square, merchant, for improvements in fulling mills, or machinery for fulling and washing woollen cloths, or such other fabrics as may require the process of fulling.—June 7.

M. Poole, Lincoln's Inn, for the preparation of certain substances for making candles, including a wick peculiarly constructed for that purpose.—June 9.

J. Burridge, Nelson-square, Blackfriars-road, merchant, for improvements in bricks, houses, or other materials, and for the better ventilation of houses and other buildings.—June 9.

J. Lindsay, of the island of Herme, near Guernsey, for improvements in the construction of horse and carriage ways of streets, turnpike and other roads, and an improvement or addition to wheels to be used thereon.—June 14.

W. H. James, Coburg-place, Winson-green, Birmingham, engineer, for improvements in the construction of boilers for steam-engines.—June 14.

J. Downton, Blackwall, shipwright, for improvements in water closets.—June 18.

W. Mason, Castle-street, East, Oxford-street, axletree manufacturer, for improvements on axletrees.—June 18.

C. Phillips, Upnor, Kent, for improvements in the construction of a ship's compass.—June 18.

G. Atkins, Drury-lane, and Henry Marriott, Fleet-street, ironmonger, for improvements on, and additions to, stoves or grates.——June 18.

E. Jordan, Norwich, engineer, for a new mode of obtaining power applicable to machinery of different descriptions.—June 18.

J. Thompson, Vincent-square, Westminster, and the London Steel Works, Thames Bank, Chelsea, and John Barr, Halesowen, Birmingham, engineer, for improvements in producing steam applicable to steam-engines, or other purposes.—June 21.

T. Northington, the younger, and J. Mulliner, both of Manchester, small-ware manufacturers, for improvements in the loom, or machine, used for the purpose of weaving or manufacturing of tape, and such other articles to which the said loom, or machine, may be applicable.—June 21.

Ross Corbett, Glasgow, merchant, for a new step, or steps, to ascend and descend from coaches, and other carriages.—June 21.

P. Brookes, Shelton, in the Potteries, Staffordshire, engraver, for improvements in the preparation of a certain composition, and the application thereof, to the making of dies, moulds, or matrices, smooth surfaces, and various other useful articles.—June 21.

J. F. Smith, Dunston Hall, Chesterfield, for improvements in machinery for drawing, roving, spinning, and doubling cotton, wool, and other fibrous substances.—June 21.

J. J. Saintmare, Belmont Distillery, Wandsworth Road, Surrey, distiller, for improvements in distilling.—June 28.

D. Redmund, Old-street Road, Middlesex, engineer, for improvements in building ships, houses, &c.—June 28.

G. Thompson, Wolverhampton, for improvements in the construction of saddles.—June 28.

J. Heathcoat, Tiverton, lace-manufacturer, for improvements in manufacturing thrown silk.—July 6.

W. Heycock, cloth-manufacturer, Leeds, for improvements in machinery for dressing cloth.—July 8.

J. Biddle, Dormington, Salop, glass manufacturer, for his machinery for making, repairing, and cleansing roads and paths, &c.—July 8.

Lieut. Molyneux Shieldham, Brampton Hall, Wrangford, Suffolk, for improvements in setting, working, reefing, and furling the sails of vessels.—July 8.

W. Furnival and J. Craig, both of Anderton, Cheshire, salt-manufacturers, for improvements in the manufacturing of salt.—July 8.

J. Day and S. Hall, Nottingham, lace-manufacturers, for their improvement on a pusher twist or bobbin-net machine.—July 8.

W. Hancock, King-street, Northampton-square, Middlesex, for improvements in the making of pipes for the passage of fluids.—July 16.

W. and H. Hurst, Leeds, for improvements in scribbling and carding sheep's wool.—July 16.

H. Hurst, manufacturer, and G. Bradley, machine-maker, both of Leeds, for improvements in looms for woollen cloths.—July 16.

T. W. Stansfeld, merchant, W. Prichard, civil engineer, and S. Wilkinson, merchant, Leeds, for improvements in looms and in the implements connected therewith.—July 16.

T. Saddler, Devizes, for improvements in collars for horses and other animals.—July 16.

M. I. Brunel, Bridge-street, Blackfriars, for mechanical arrangements for obtaining powers from fluids, and for applying the same to various useful purposes.—July 16.

T. Sitlinton, Stanley Mills, engineer, for improvements in machinery for shearing or cropping woollen or other cloths.—July 16.

J. Farey, Lincoln's-inn Fields, civil engineer, for improvements in lamps.—July 16.

T. R. Williams, New Norfolk-street, Strand, for an improved lancet. —July 16.

Lieut. T. Cook, Upper Sussex-place, Kent Road, for improvements in the construction of carriages and harness for the greater safety of persons riding.—July 16.

J. Cheseborough, dyer, Manchester, for a method of conducting to and winding upon spools, or bobbins, rovings of cotton, flax, wool, or other fibrous substances.—July 16.

W. Hirst, and J. Carter, cotton spinner, Leeds, for an apparatus for giving a new motion to mules or billies.—July 16.

J. P. De la Fons, George-street, Hanover-square, dentist, for improvements in extracting and fixing teeth.—July 16.

J. Downton, Blackwall, Middlesex, shipwright, for improvements in machines or pumps.—July 19.

ARTICLE XV.

METEOROLOGICAL TABLE.

1825.	Wind.	Barometer. Max.	Min.	Thermometer. Max.	Min.	Evap.	Rain.
6th Mon.							
June 1	S W	30·49	30·21	72	50	—	09
2	S W	30·21	29·89	68	54	—	—
3	N W	29·89	29·74	62	50	—	05
4	S W	29·74	29·60	65	43	—	15
5	N W	30·12	29·60	55	38	—	—
6	N W	30·12	30·04	64	52	·86	
7	S W	30·11	30·03	71	51	—	
8	S W	30·21	30·11	75	49	—	
9	S W	30·40	30·21	75	45	—	
10	W	30·40	30·36	79	46	·79	
11	E	30·36	30·29	81	44	—	
12	E	30·29	30·27	84	48	—	
13	N E	30·40	30·27	81	49	—	
14	N E	30·43	30·40	80	48	·88	
15	N E	30·40	30·32	82	43	—	
16	N E	30·32	30·29	85	45	—	
17	N E	30·33	30·31	73	38	—	
18	N E	30·32	30·20	75	39	—	
19	S	30·20	29·93	77	40	—	
20	N W	30·02	29·93	67	37	·74	07
21	N E	30·19	30·02	62	35	—	
22	N W	30·24	30·19	74	42	—	
23	N W	30·23	30·15	75	42	—	
24	S W	30·15	29·96	75	44	—	
25	S E	29·93	29·87	77	47	·98	05
26	S W	29·94	29·93	70	44	—	03
27	S W	29·94	29·93	72	50	—	08
28	S W	29·93	29·86	74	52	—	08
29	N W	29·88	29·87	73	54	—	02
30	N W	29·90	29·88	72	55	—	06
		30·49	29·60	85	35	4·25	·68

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Sixth Month.—1. Fine. 2. Fine, with occasional clouds 3. Fine. 4. Rainy. 5. Showery. 6, 7. Fine. 8. Cloudy. 9—19. Fine. 20. Cloudy: showers. 21—24. Fine. 25. Showery. 26. Fine: showery. 27. Fine, with slight showers. 28. Showery. 29. Cloudy. 30. Showery.

RESULTS.

Winds: NE, 7; E, 2; SE, 1; S, 1; SW, 10; W, 1; NW, 8.

Barometer: Mean height

For the month.................................... 30·112 inches.

Thermometer: Mean height

For the month.................................... 59·516°

Evaporation.................................... 4·25 in.

Rain.................................... 0·68

Laboratory, Stratford, Seventh Month, 26, 1825. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

SEPTEMBER, 1825.

Article I.

On Naval Improvement. By Col. Beaufoy, FRS.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Bushey Heath, near Stanmore, Aug.* 1, 1825.

The building of three experimental vessels for the improvement of naval architecture having excited much attention in the public mind, not only from the peculiarly interesting nature of the science under inquiry, but from the professional abilities of the different projectors, the individual success of each ship has been observed with an anxiety commensurate with the importance of the object in view.

If, notwithstanding the skill of the constructors, neither of these men of war showed decided superiority in sailing, the failure must be attributed to our ignorance of the resistance bodies meet with when opposed to the impulse of water. Our knowledge of this branch of physics is nearly as limited as our acquaintance with the laws which govern the motion of the fixed stars; but here the parallel must end: the accumulated industry of ages alone will probably detect the cause which produces change of place amongst these heavenly bodies, whereas the advancement of hydrodynamics is within the influence of the present generation.

If strength, durability, and efficiency, be all that is required in our floating fortresses, these characteristics have already been combined by the talent of Sir Robert Seppings.

It appears that much uncertainty existed in the sailing of the experimental vessels: sometimes one had the advantage, sometimes another; the distinction resting mainly on the quantity and stowage of the ballast, alterations in the masts, yards, &c. The requisiteness of these changes is a proof that the highest genius is incapable of correctly anticipating either the qualities or the sailing powers of a ship prior to her going to sea.

One great point has been gained by building these vessels, in showing that the synthetical process is inadequate to obtain the end in view. Is it not similar to a chemist who, desiring to analyse metals, of which some were known and others unknown, first mixed them altogether, and then, after great pains, labour, and expence, discovered the impossibility of arriving at any accurate conclusion in regard to their respective properties; whereas had he, in the first instance, separately examined each, the result would have proved less fatiguing, less costly, and more satisfactory? In all complex cases, scientific, or mechanical, the most easy and natural way for well understanding the subject is to resolve it into the component parts.

In the construction of ships, the great and leading features are stability and fast sailing; the theory of the former is sufficiently known, but our acquaintance with the resistance of non-elastic fluids may be termed yet in its infancy. The ablest builder is at present ignorant of the curves best adapted for dividing the water; and working thus in the dark, it is no wonder that the aggregate of slow sailers so far exceeds those that are fast.

If it be deemed desirable to persevere in building experimental vessels, the plan proposed in the *Annals of Philosophy*, for Oct. 1817, may not be unworthy of notice.

The importance of discovering the curve of least resistance is not confined alone to vessels moved by the power of wind. Constructors of steam boats are deeply interested in the fact. If a packet with an engine of forty horse power be driven nine knots in an hour, it will require an effort of nearly sixty-one horses to increase the speed to ten. Could this additional mile be gained by giving the hull a more advantageous form for cleaving the water, many substantial benefits would accrue. The original cost of the engine would be lowered from the inferior size required, expenditure in fuel and stowage would be saved, and less risk incurred of the melting of the grate bars. In short, from the waterman who plies upon the Thames to the captain commanding the largest ship in the British navy, all are interested in finding the solid of least resistance; the first by diminishing the labour of the oar, and the latter by out-sailing, coming up with, and capturing the enemy's ship.

Ships have been aptly compared to bridges connecting the whole world together; a slow sailing vessel, therefore, is a bridge longer than necessary. It is not improbable that the Carthaginian and Roman builders surpassed the moderns in the form they gave their men of war for cleaving the water, because, being frequently impelled by oars, to lighten the fatigue of the rowers, must have been a matter of the greatest moment.

It is highly gratifying to observe the pleasure that several of the nobility and gentry take in maritime concerns. The Royal Yacht Club, by building vessels, and bestowing prizes on the best sailers, enjoy the patriotic and praiseworthy consciousness that money so expended encourages some of the most useful classes of society, and creates a spirit of emulation among the different branches of artificers connected with nautical affairs. This institution, by introducing for trial new and expensive machinery, is capable of performing services which few individuals could undertake; and it is submitted for the consideration of the body whether considerable improvement in the science of sailing might not result from the following experiments.

Lug sails are usually thought preferable to others in turning to windward, and such as are taunt and narrow are deemed more effective than those that are low and square; but this phrase of taunt and narrow is extremely indefinite. In the first instance it is proposed that a vessel rigged as a lugger shall sail with others, and most likely one amongst them will be found either a company keeper, or whose rate is nearly on a par. In the next place, let canvass be taken from the breadth of the sails and added to the hoist, and a second comparison made; thus subjecting the sails to repeated alterations and trials, until the maximum of the length to the breadth be obtained. This fact established, the next suggestion is to convert the lugger into a cutter, observing the necessary precaution that the mainsail, foresail, and jib, expose the same surface to the action of the wind, as the sails of the lugger. The third trial consists in changing the same vessel into a schooner, scrupulous regard being paid that the quantity of sail is equal in the three cases, and that no variation in the weight, quantity, or stowage of the ballast be permitted either in the boat of comparison, or experimental vessel.

Rigid adherence to these points is essential to the success of the experiments, inasmuch as it is the action of sails, and not the best trim of the hull, which forms the object of the present inquiry. A vessel of size is for several reasons desirable; one of 14 feet beam, and 57 on deck, might prove sufficiently large; but the beams of the deck should be so disposed as to require no removal in the subsequent alterations of the masts for the various modes of rigging. It is also recommended that the body be clencher built; vessels so constructed generally excel in sailing such as are carvel made, and this superiority will obtain so long as the resistance of water to curved lines shall be involved in obscurity.

It is somewhat paradoxical that constructors of boats for contraband trade should possess such decided advantage over the builders employed by the revenue as to call forth an Act of

Parliament regulating the extent and the fixing of the bowsprit, and limiting the proportions which the breadth of a vessel must bear to its length. Such legislative interference is detrimental to science: experience teaches us that attempts to run goods will continue so long as high duties create the temptation; and the boat restrictions, instead of mitigating the evil, have but caused the removal of the capital and skill of the constructors from our own coasts to those of Holland. If a smuggler build a lugger 13 feet beam, 96 from stem to stern, and the bowsprit 60 feet long, why not launch a custom vessel of 100 feet in length: the smuggler, if chased, would use his best endeavours to escape; the revenue officer, actuated by duty and stimulated by hope, would exert his utmost to make a seizure, and the relative success of either party would soon determine the most effective limits of length to breadth.

Let not these remarks be misconstrued into an advocation of illicit trade. Taxes must be raised, and consequently any person who by smuggling evades paying his individual share, commits a fraud on the rest of the community by binding on others the obligation of his own debts. My sole wish is that naval science may not be injured by legal enactments. On the same principle that laws are made for building and fitting of vessels, why should not others pass, restricting residents on the coast suspected of contraband addictions to the services of none but lame horses; whereas all such as are fleet shall be devoted to the use of those engaged in the collection of the revenue.

I remain, Gentlemen, your obliged

Mark Beaufoy.

Article II.

Explanation of the Theory of the Barometrical Measurement of Heights. By Mr. Nixon.

(*Concluded from* p. 96.)

Calculation by Logarithms.

We have seen that when the *differences* of the pressures sustained at the summit and base of the strata of an atmosphere of dry air are the same, the weights of those strata will be equal, and their heights, granting them to be so thin as to be sensibly of uniform density, will be reciprocally as the pressures they support; consequently as the difference of the logarithm of any given number and that of another, greater by an indefinitely small quantity, compared to the logarithmic difference of any other given number, and one greater by the *same* difference will be reciprocally as the two given numbers,* it is evident that the natural numbers of a table of common logarithms will represent the pressures or heights of the barometer, and the difference of their corresponding logarithms, the difference of level, or altitude of the strata (or stations), *possibly* in feet, but at all events in some constant ratio of the scale of the barometer.

We have demonstrated that when the pressures, as we ascend a series of stations, are observed to diminish in geometrical progression, the difference of level of the stations will be equal; and we shall find that if we affix to a series of numbers in geometrical progression their corresponding logarithms, then will the differences of the latter continue equal; thus confirming the propriety of substituting logarithms in our calculations, for the more tedious and less accurate method made use of in the first instance.

Example.

		Differences.
Logarithm of 32	= 1·5051500	
16	= 1·2041200	·3010300
8	= 0·9030900	·3010300
4	= 0·6020600	·3010300
2	= 0·3010300	·3010300
1	= 0·0000000	·3010300

The vertical height of a stratum of dry air of the temperature of 32° F. intercepted by the pressures of 43·42945 inches, and 43·42946 inches, is equal to ·0060095 foot; but as the logarithmic difference of the pressures is only ·0000001, we must multiply the difference of the logarithms of the heights of the barometer at the two stations by 60095, and the product will be equal to their difference of level in feet.

Example.

Lower barometer 30·5 in.	Log.	1·4842998
Upper barometer 15·5		1·1903317
	Difference	0·2939681 × 60095 = 17666 ft.

When the mean temperature differs from 32°, we must alter the multiplier of the logarithmic differences (termed the constant coefficient) in the ratio of the variation of the volume of the air; or we may multiply the altitude at 32° by the difference of that temperature and the mean of the detached thermometers, and

* Log. of	15·00	1·1760913	Log. of	30·00	1·4771213
	15·01	1·1763807		30·01	1·4772660
Differences	00·01	0·00028	Diff.	00·01	0·0001447

Hence 2894 is to 1447 in the inverse ratio of 15 to 30.

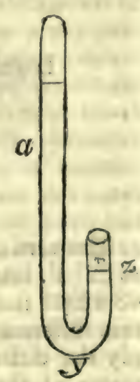
dividing the product by 480, add or subtract the quotient according as the mean temperature is above or below 32°.

In order to introduce the correction for the diminished specific gravity of the quicksilver in the vertical line, we must augment the constant coefficient $\frac{10}{3984}$, or to 60246, and call the dilatation of air $\frac{1}{479}$ per degree.

The indices of the logarithmic differences being sometimes negative as well as affirmative, it will be advisable to disregard them altogether, considering the tabular logarithms of the pressures as decimals.

Construction of the Barometer.

The *syphon* barometer consists of a glass tube bent in the form of an inverted syphon, filled with very pure mercury freed from air by carefully boiling it when in the tube.* Each branch being furnished with a scale of inches having their zeros coinciding in level, the difference of the observed heights of the summits of the (perpendicular) mercurial columns will be equal to the height of the barometer or pressure of the atmosphere. In order to dispense with the shorter scale, the index of the longer branch is so constructed as to slide down to the level of, and extend horizontally to the summit of the mercury in the other branch. In some barometers the zero of the scale is placed anywhere above the shorter leg as at *a*, and the inches are numbered upwards and downwards so that the *sum* of the two measurements, instead of their difference, exhibits the pressure. An augmentation in the pressure of the atmosphere having taken place, a depression of the mercury in the shorter branch, and an equal elevation in the longer one, of half the quantity of the variation restore the equilibrium of pressure. To obviate the trouble of measuring the difference of level of the two columns, the shorter one only has been provided with a scale, having the half inches numbered as whole inches;—a method which renders it impossible to make a proper correction for the variation of temperature of the mercury. (We might inquire why the syphon itself, being laterally confined within cylindrical rings, might not be raised or depressed by means of a screw fixed below *y*, so that the summit of the shorter column might always coincide in level with the zero of the scale fixed at *z*?)

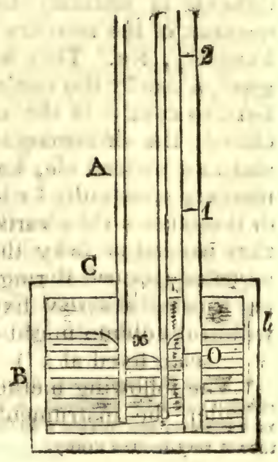


The following methods have been resorted to with a view to render the instrument portable. 1. The two branches being

* It has been proposed as an improvement to fill the tube in a vacuum.

connected by means of a little apparatus furnished with a cock, the whole of the mercury may be confined within the longer branch. 2. The two branches being fixed within a leathern bag containing mercury, a stopper is inserted in the shorter branch, and the mercury forced to the summit of the interior of both branches by means of a screw pressing against the bag. The screw serves further to bring the shorter column of mercury, under every variety of pressure, to the zero of the scale. 3. The syphon being filled with mercury in the usual manner, the shorter branch is hermetically closed, and a capillary opening made a little above x to admit the atmospheric air, but too small to permit the mercury to escape. To break the force of the shocks to which the instrument may be exposed, the longer tube is bent out of a straight line, or both branches are contracted in bore somewhere near the summit.

If we fill a number of strait glass tubes of different interior diameters with mercury, and immerse them (inverted) in a basin of the same fluid placed in a vacuum, it will be observed that the mercury within the tubes will descend below the level of the fluid in the basin; the depression being most considerable within the tube having the smallest bore. The atmospheric air being admitted to press on the mercury in the basin, it will be found that the heights of the mercurial columns (exhibiting the pressures) will fall short of the elevation of the column within an extremely wide tube, and that the discrepancies will be equal to the depressions below the surface observed before the admission of the air. Hence it is clear that when the interior of the syphon barometer is not of the same diameter in both branches within the *range* of the summits of the mercurial columns, a correction for capillarity (otherwise unnecessary) will be required: the height of the mercury within the narrower tube must be augmented by the *difference* of the corrections proper for the respective tubes.



It has been objected to the syphon barometer that the adhesion of the mercury taking place through the length of two tubes as well as in the part connecting them, the settling of the fluid will be more uncertain than in the single tube of the cistern barometer; and that two measurements are more liable to error in noting the pressure than when one only is required.

The cistern barometer consists in its simplest form of a strait tube A hermetically closed at one end, filled with mercury,* and having the other immersed in a cylindrical ivory or wooden cistern B, containing a sufficient quantity of the same fluid. The tube and cistern are connected by the cover C, having a small aperture for the admission of air.

To render the instrument portable, the cover is constructed without the aperture, but can be opened for the ingress of the air, or closed to prevent the escape of the mercury by means of an ivory screw. To counteract the oscillations of the mercury within the tube, the central part of the bottom of the cistern is formed of leather having a screw fixed beneath it capable of forcing the mercury to the summit of the tube.

At whatever height of the mercury within the cistern the artist may have adjusted the scale of inches so that its zero shall coincide in level with the surface of the fluid, it is obvious, that as the pressure increases (or diminishes), the mercury will subside below (or rise above) the level of zero. This constant source of error is remedied in the barometer of Ramsden by raising or depressing the leathern bag sustaining the mercury until the mark made on a piece of ivory floating in the cistern coincides in level with some fixed point equally elevated with the mark above the zero of the scale. The adjustment of the float to the gauge being considered as difficult, a superior plan has been adopted by Troughton, consisting in having the vertical sides of the cistern formed of glass cased in brass. Near the top of the case is a horizontal slit, and another exactly opposite, both having their upper edges accurately of the height of the zero of the scale. The adjustment is effected by turning the screw of the leathern bag until the surface of the mercury seen through the horizontal slits just excludes the transmitted light.

Barometers of this description are extremely cumbrous difficult in ordinary hands to adjust, and objectionable on account of the mercury being exposed during an observation to humidity, &c. They have in consequence been superseded for general use by the one invented by Sir H. Englefield. Its peculiarities consist in the cover of the cistern being permanently closed, the air forcing its way through the pores of the wood; and that instead of an adjustment to bring the surface of the mercury constantly to the level of zero, the computer increases or diminishes the observed heights of the columns, according as they exceed or fall short of the height at which the scale was adjusted (termed the *neutral point*) by a quantity found by dividing those differences by the ratio of the area of the mercury within the tube to its area within such part of the cistern as

* When the barometer, filled with mercury at ordinary pressures, is carried to the summit of a lofty mountain, a third, or even one-half of the mercury within the tube may subside, and mixing with that in the cistern will render the boiling of it within the tube of very limited utility.

surrounds that tube. This ratio is termed (for brevity) the *capacity.*

If we suppose the zero 0 (see last figure) of the scale of an Englefield barometer placed in a vacuum to coincide with the surface of the mercury x within the tube, depressed from capillarity some little below its level l in the cistern, it is evident that the height of the mercurial column on the admission of the atmospheric air will always be observed in defect, and in one *constant* ratio. Admitting the bore of the tube to be uniform, it is unnecessary to be acquainted with this ratio (equal to that of the capacity); we have merely to suppose the mercury increased in density in some unknown degree, and the calculations will give at once the true altitude without the previous troublesome corrections for capacity (and capillarity).

Example.

Correct pressures.

30·600 in..................	Log.	·4857214
20·400....................		·3096302
Differences		·1760912

Observed pressures (capacity $\frac{1}{50}$).

30·000 in..................	Log.	·4771213
20·000....................		·3010300
Differences...............		·1760913

When a variation of temperature occurs, the heights of the columns augment and diminish, not only without interfering with the level of the cistern, but the height of the mercury therein is itself subjected to a simultaneous elevation and depression in proportion to its varying depth.* Setting aside this latter cause of error, as being too trivial to be regarded, it must, however, be admitted that the reduction of the columns for temperature should be made on their observed heights augmented in the ratio of the capacity, or that the expansion per degree $\left(\frac{1}{11153}\right)$ should be proportionally increased. The capacity being rarely greater than one-fortieth, the error in a difference of 16 degrees of the attached thermometers will be no more than one foot.

The adjustment in a vacuum being scarcely practicable, let the artist measure the height of the column of mercury (as usual) from the level of the cistern, and subtract the correction for capacity *minus* that for capillarity. The scale being after-

* Admitting the tube and cistern to preserve their diameters unaffected by temperature.

wards so affixed as to exhibit the height of the column at the reduced, in lieu of the measured quantity, the calculations may be made as before without introducing any correction whatever for capacity or capillarity.

Calculation.

Measured height above the level of the mercury		30·000 in.
Correction for capacity $\left(\frac{1}{50}\right)$	·600	
Capillarity	·100	
		·500
Height to be indicated by the scale.		29·500

The correction for capacity is in the ratio of the *square* of the interior diameter of the tube to the *square* of the diameter of the cistern *minus* that of the exterior diameter of the tube.* As an incorrect estimate of the capacity will influence the adjustment of the scale in the ratio of the height of the column, the artist should make the measurement at as low a pressure as practicable.

The Englefield barometers as at present constructed have their neutral points marked at or about 30 inches, so that the computer has frequently to make the correction for the lower barometer additive, and the one for the instrument at the upper station subtractive. These corrections may indeed be shunned by increasing the calculated altitude in the ratio of the capacity,† but the method is only an indifferent approximation, capable of introducing an error (in defect, when the mean of the two pressures falls below the neutral point) equal to the 100th part of the altitude.

When the absolute pressure is required, it may be readily found with the zero adjusted as proposed by increasing the observed pressures in the ratio of capacity, and then correcting them for temperature. To adjust the zero of a scale already attached to the barometer, raise the scale, or lower the cistern with the connected tube by a quantity equal to the height of the neutral point divided by the fraction indicating the capacity *minus* the correction for capillarity.‡

* When the tube and cistern are not formed of materials expanding alike from change of temperature, the ratio of capillarity will be (slightly) variable.

† To reduce the errors of this approximative method, the neutral point should be equal to the *mean* of the pressures likely to occur in general barometrical observations, for instance, 26 or 27 inches.

‡ Were we to immerse in the cistern of a *stationary* barometer a vertical rod of glass of the same diameter as the mercurial column within the tube, and so connected with the index as to move with it in a vertical line; but in an *opposite* direction, an exact compensation would be effected for the drainage and filling of the cistern at pressures differing from the neutral point. Should the length, equal to the range of the pressure,

Mr. Newman has remarked that when the cover of the cistern of the Englefield barometer is sufficiently porous to admit the air with freedom, the pressure of the screw forcing the mercury to the summit of the tube cannot fail to expel some portion of the fluid through the cover, thus disturbing the adjustment of the zero; and that on the other hand a cover made sufficiently strong to prevent the escape of the mercury will so obstruct the free admission of the air that a very considerable time must elapse before the barometer exhibits the correct pressure of the atmosphere. To remedy these evils, Mr. Newman constructs the cistern of cast-iron having a cover of very porous wood. The instrument being sufficiently inclined, the mercury ascends to the summit of the tube, and is retained therein by a screw capped with leather, which, passing upwards through the bottom of the cistern, presses against the orifice of the tube. Provided this novel method of rendering the instrument portable shall be proved to preserve the column of mercury free from the admixture of atmospheric air, there can be no doubt that the iron cistern will be found preferable to one of wood and leather, varying in tension, &c. with the hygrometric state of the atmosphere.

General Observations.

Two barometers filled with mercury of the same specific gravity, placed near each other with their cisterns on the same level, will have their columns (corrected for capillarity, capacity, and inequality of temperature) observed at the same height under every variety of pressure: otherwise, the corrected heights will be inversely as the specific gravities. Hence if two barometers compared at the base of a mountain stand at 30 in. and 30·2 in. respectively, we must increase the pressures exhibited at the upper station by the instrument having the denser mercury in the ratio of 30 to 30·2; in lieu of adding to it, as is more generally the case, the discrepancy observed at the base. It will be in vain to attempt the determination of the difference of level of places distant from each other, by means of the stationary barometer, unless the mercury is known to be of the same specific gravity in all the instruments.

If we incline a barometer out of a vertical line the difference of *level* of the cistern and summit of the column will remain unaltered, yet as the scale laterally attached to the tube is equally inclined with it, we measure the height of the column in excess in the ratio of radius to the secant of the angle of inclination. Nevertheless if the barometers at the two stations

require a cistern of an inconvenient depth, the rod might be double or treble the area of (a horizontal section of) the bore of the tube, but made to describe a vertical space, inferior in the same ratio to the one passed over by the index; easily effected by a proper arrangement of the teeth of a wheel attached to the glass rod and the endless screw of the index.

deviate from the perpendicular by the *same* angular quantity, the calculations may be correctly made on the supposition of the mercurial columns having been truly vertical. It is merely to imagine the mercury specifically too light in the ratio of the secant of the angle of inclination to radius. Should the degree of obliquity be considerable, the friction will be materially increased, and the mercury will not settle so correctly as in a vertical tube.

To verify the adjustment of zero to the level of the cistern, note the pressures (when very low) with the instrument first in a vertical, and afterwards in an inclined position. Then if the pressure when inclined does not exceed the other in the ratio of radius to secant of the angle of inclination, the zero dips within the mercury, and the heights will be observed in excess. Or we may find the correct pressure, and consequently the error of adjustment, by measuring the height of the longer column of a wheel barometer (freed of its pulleys) above a horizontal line drawn from the summit of the shorter column. The mercury in the two instruments must, however, be of the same specific gravity.*

Errors.—When the pressure is incorrectly measured at *one* of the stations, which may arise from an inequality in the divisions of the scale; from parallax in reading off the vernier; from want of horizontal parallelism in the movement of the index; from the barometers differing in inclination; from the varying adhesion of the mercury to the sides of the tubes; or from the inexpertness of the observer in adjusting the notch, &c. of the vernier in a tangent to the convex summit of the mercury,—the value of the error when equal to ·001 in. will be generally rather more than one foot of altitude; or more correctly to

0·785 foot, the temperature being	0° F.	Pressure 31 inches.
0·976 ————————	102	
1·747 ————————	32	Pressure 15 inches.
1·623 ————————	0	

When the zero of the scale is improperly placed, or the allowance for capillarity is incorrect, so that the pressures are always observed in excess or defect by some constant quantity, the calculated altitude will be to the true elevation *inversely* as the half sum of the observed pressures is to that half sum augmented or diminished as the pressures are in defect or excess by the constant error. The subjoined scale exhibits the value in feet of the error at different pressures, the altitude being 1000 feet.

* An Englefield barometer in my possession (remarkable for the constancy with which its mercurial columns would settle to the same height on being disturbed, and there is a most material difference in the instruments in this respect) had its zero placed too high by nearly half an inch.

Constant error.		·1 in.	·2 in.	·3 in.	·4 in.	·5 in.
Mean pressures.	30 in.	3 ft.	7	10	13	17
	25	4	8	12	16	20
	20	5	10	15	20	25
	15	6	13	20	26	33 feet.

The corrections are additive or subtractive according as the pressures are observed in excess or defect.*

An error of 1° F. in the difference of the indications of the attached thermometers will render the calculated altitude incorrect by a quantity varying with the temperature of the air from 2·2 to 2·7 feet. To obtain the correct mean temperature of the mercurial columns without considerable loss of time is one of the greatest difficulties the observer has to encounter. In the barometers of Newman, the *cistern* is furnished with a thermometer having its bulb (of a tapering form) immersed in the mercury.

An erroneous estimate of the mean temperature of the air amounting to 1° will vary in value from one foot in 500 at 32° F. to one in 480 at 90° F. Errors will occur when the arrangement of the strata of the atmosphere of different temperatures is such that the half sum of the thermometers does not represent their correct mean temperature; or when one or both thermometers have been placed with so little discretion as to give the temperature of some small portion of air affected by local heat, in lieu of the general temperature of the surrounding air. Errors may also be introduced when the fixed points of the thermometer are placed too high or too low; when the calibre of the tube is unequal, and not allowed for in the graduation of the scale, or when the height of the mercurial column has been noted as affected by parallax.

An inaccuracy of 1° in observing the dew point cannot possibly give rise to an error of more than one foot in 2000. Most of the other sources of error in estimating the mean hygrometric state of the air will be similar to those occurring in ascertaining the mean temperature. Whatever the possible state of the atmosphere as to humidity, provided we add to the mean of the detached thermometers, half the correction for saturated air under the pressure of 30 inches, the error in altitude will not exceed $\frac{1}{100}$th, but more generally it must fall short of half that quantity.

Were we in possession of a sufficient number of barometrical observations made at the foot and summit of isolated mountains, carefully levelled, together with the *true* mean temperatures and dew points of the intercepted strata of the atmosphere, we could

* This is only an approximation; for the half sum of the pressures we should substitute the mean-density pressures.

easily determine by calculation the coefficients for dry air at different degrees of the thermometer. Subjoined are the coefficients for air in a mean state of saturation as deduced from the barometrical observations of Shuckburgh, Ray, and Ramond.

		Ramond.	Shuckburgh and Ray.
Air 32°	Coefficient	60345	 60000
52	———	63027	 62922
60	———	64100	 64091
72	———	65709	 65844

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

Leeds, May 10, 1825. J. Nixon.

Explanation of the Tables.

Table I.—The degrees affixed to the dew-points being the equations for saturated air under a pressure of 25 inches *multiplied* by 25, the equations for the dew-points at the two stations are found by dividing the degrees given in the table by the observed pressures in inches, and adding the sum of the two equations to that of the detached thermometers, which call the corrected sum of the thermometers.

Example.

	Pressure.	Air.	Dew-point.	
Upper station.	27·5 in.	.. 82·2	.. 70.	Eq. = 152 ÷ 27·5 = 5·5
Lower ditto ..	29·8	.. 84·0	.. 70	—— 152 ÷ 29·8 = 5·1
Sum of thermometers		166·2		Sum 10·6
Equation for moisture		10·6		
Corrected sum of therms..		176·8		

Table VI.—When the observations have been made without hygrometer, the equations given in this table must be added to the sum of the detached thermometers.

Example.

(Chimborazo) Upper station	29·1°
(Pacific Ocean) Lower station	77·5
Sum of thermometers..............	106·6
Equation.........................	2·9
Corrected sum of thermometers.....	109½

The tabulated equations (calculated for half saturated air) may be modified according to the estimated degree of saturation.

TABLE V contains the logarithms (exclusive of the index 4) of the coefficients for dry air at different degrees and quarters of a degree of the *sum* of the detached thermometers, including an exact correction for the diminished specific gravity of the mercury in the vertical line.

Rule.—1. Affix to the observed heights of the barometers at the two stations the corresponding tabular logarithms (rejecting indices), which consider as natural numbers, and make the first four figures to the left hand whole numbers, and the remainder decimals. 2. Find the difference of these two numbers, generally termed the *logarithmic difference.* 3. Take out the logarithm of that difference (still avoiding indices), and prefix to it with an intervening decimal point, the number expressive of the quantity of whole numbers contained in the logarithmic difference. 4. Add to this logarithm and index of whole numbers the logarithm of the coefficient for the corrected sum of the thermometers precisely as given in the table. 4. Find the natural number of the product, exclusive of the index, which will be the altitude in feet, consisting of the quantity of whole numbers denoted by the index in the product.

Example.

Pressures.

Pacific Ocean 29·8781 in. Log. 4753·53
Chimborazo 14·8296 1711·30

Log. of 3042·23 (prefix. 4) = 4·483192
Log. coefficient of the cor. sum of therms. .. 109·5 0·799899
217

Log. 5·283308

Natural number of 283308 (the whole numbers being five)	19200·0
Correction for the diminished specific gravity of the air, sufficiently explained in the Table (IV) +	17·7
Correction for latitude 2°, Table III. + ...	54·4
Altitude of Chimborazo	19272·1 feet.

The calculation is effected with the pressures reduced to 32° F. The observations were,

Pacific Ocean 30·0005 in. (77·5)* Log. 4771·29
Chimborazo.. 14·8536 (50·0) 1718·32

Diff. of attached therms. 27·5 3052·97 Log. = 4·484723
Log. of coefficient 0·800116

5·284839

* The indications of the *interior* thermometers represented by the ( ).

Corresponding natural number	19268·1
Correction for 27·5 × 2·45 feet (Table II)	67·4
	19200·7 feet.

scarcely differing three-fourths of a foot from the preceding rigid method of calculation. Had the thermometers been graduated as proposed, we should have had

Pacific Ocean	189·0
Chimborazo	122·0
Correction subtractive	77·0 feet.

Two more examples are subjoined :—

No. I.

		Air.	Dew-pt.	Pressures.	
*Fort Thornton....	(84°)	84·0	70	29·795 in.	Log. 4741·43
Sugar-loaf hill	(82.2)	82·2	70	27·527	4397·59
Diff. of att. therms.	1·8	166·2		3·536356=Log. of	343·84
Equa. for moisture		10·6		0·828295 Coeff. of	$176\frac{3}{4}$
Cor. sum of therms.		$176\frac{3}{4}$		4·364651 Log. of	2315·5

Correction for attached thermometers	−4·7
	2310·8
Correction for latitude 8°...........	+6·3
	2317·1
Fort Thornton above the sea.	+191·5
Altitude of Sugar-loaf hill	2508·6 ft.

No. II.

			Pressures.	
Malham Tarn.	(60)	60	29·223 in.	Log. 4657·25
Great-close hill†	(55)	55	28·903	4609·43
Diff. of att. therms...	5	115	2·679610 Log. of	47·82
Equation for moisture		$3\frac{1}{4}$	0·803878 Coeff. of	$118\frac{1}{4}$
		$118\frac{1}{4}$	3·483488 Log. of	304·4

Correction for attached thermometers	12·5	12·8
Correction for latitude 54	0·3	
		291·6
Trigonometrical altitude of Great-close hill ...		1525·3
Altitude of Malham Tarn above the Irish Sea		1233·7 ft.

* Observations in Sierra Leone, by Capt. Sabine.

† A conical hill of Gordale limestone one-third mile to the north-east of the Tarn.

TABLE I.—*Correction for Moisture.*

Dew point.		Dew point.		Dew point.		Dew point.		Dew point.	
0°....	11°	32°....	39°	47°....	67°	62°....	116°	77°....	193
2	12	33	40	48	70	63	120	78	200
4	13	34	42	49	73	64	124	79	207
7	15	35	43	50	75	65	128	80	213
10	17	36	45	51	78	66	132	81	220
13	19	37	47	52	81	67	137	82	228
16	21	38	48	53	84	68	142	83	235
18	23	39	50	54	87	69	147	84	243
21	26	40	52	55	90	70	152	85	251
24	29	41	54	56	93	71	157	86	259
27	32	42	56	57	97	72	163	87	268
28	33	43	58	58	100	73	168	88	276
29	35	44	60	59	104	74	174	89	286
30	36	45	62	60	108	75 ...	180	90	295
31	37	46	65	61	112	76	186	91	305

Rule.—Divide the degrees affixed to the dew point by the observed pressure in inches.

TABLE II.—*Value in Feet of 1° of Difference of the attached Thermometers at different Temperatures of the Air.*

Sum of detached Thermometers.	
4°F.	2·18 feet.
14	2·21
24	2·24
34	2·26
44	2·29
54	2·31
64	2·34
74	2·37
84	2·39
94	2·42
104	2·44
114	2·47
124	2·50
134	2·52
144	2·55
154	2·57
164	2·60
174	2·63
184	2·65
194	2·68

Rule.—Multiply the feet in the second column by the difference of the attached thermometers, and deduct the product, when the temperature of the upper barometer is inferior to the other, from the difference of level, or altitude.

Table III.—*Correction for Latitude.*

Ad in latitude.	Altitude in feet. 1000	2000	3000	4000	5000	6000	7000	8000	9000	10000	11000	12000	Subtract in lat.
	Feet.												
0°	2·8	5·7	8·5	11·3	14·2	17·0	19 9	22·7	25·5	28·4	31·2	34·0	90°
2	2·8	5·7	8·5	11·3	14·2	17·0	19·8	22·6	25·5	28·3	31·1	34·0	88
4	2·8	5·6	8·4	11·2	14·0	16·9	19·7	22·5	25·3	28·1	30·9	33·7	86
6	2·8	5·6	8·3	11·1	13·9	16·7	19·4	22·2	25·0	27·8	30·5	33·3	84
8	2·7	5·5	8·2	10·9	13·6	16·4	19·1	21·8	24·5	27·3	30·0	32·7	82
10	2·7	5·3	8·0	10·7	13·3	16·0	18·7	21·3	24·0	26·7	29·3	32·0	80
12	2·6	5·2	7·8	10·4	13·0	15·5	18·1	20·7	23·3	25·9	28·5	31·1	78
14	2·5	5·0	7·5	10·0	12·5	15·0	17·5	20·0	22·5	25·1	27·6	30·1	76
16	2·4	4·8	7·2	9·6	12·0	14·4	16·8	19·2	21·7	24·1	26·5	28·9	74
18	2·3	4·6	6·9	9·2	11·5	13·8	16·1	18·4	20·7	23·0	25·2	27·5	72
20	2·2	4·3	6·5	8·7	10·9	13·0	15·2	17·4	19·6	21·7	23·9	26·1	70
22	2·0	4·1	6·1	8·2	10·2	12·2	14·3	16·3	18·4	20·4	22·4	24·5	68
24	1·9	3·8	5·7	7·6	9·5	11·4	13·3	15·2	17·1	19·0	20·9	22·8	66
26	1·7	3·5	5·3	7·0	8·8	10·5	12·3	14·0	15·8	17·5	19·3	21·0	64
28	1·6	3·2	4·8	6·3	7·9	9·5	11·1	12·7	14·3	15·9	17·5	19·0	62
30	1·4	2·8	4·3	5·7	7·1	8·5	9·9	11·4	12·8	14·2	15·6	17·0	60
31	1·3	2·7	4·0	5·3	6·7	8·0	9·3	10·6	12·0	13·3	14·6	16·0	59
32	1·2	2·5	3·7	5·0	6·2	7·5	8·7	10·0	11·2	12·4	13·7	14·9	58
33	1·2	2·3	3·5	4·6	5·8	6·9	8·1	9·2	10·4	11·5	12·7	13·8	57
34	1·1	2·1	3·2	4·2	5·3	6·4	7·4	8·5	9·5	10·6	11·7	12·7	56
35	1·0	1·9	2·9	3·9	4·9	5·8	6·8	7·8	8·7	9·7	10·7	11·6	55
36	0·9	1·8	2·6	3·5	4·4	5·3	6·2	7·0	7·9	8·8	9·7	10·6	54
37	0·8	1·6	2·3	3·1	3·9	4·7	5·5	6·2	7·0	7·8	8·6	9·4	53
38	0·7	1·4	2·1	2·7	3·4	4·1	4·8	5·5	6·2	6·9	7·5	8 2	52
39	0·6	1·2	1·8	2·4	3·0	3·5	4·1	4·7	5·3	5·9	6·5	7·1	51
40	0·5	1·0	1·5	2·0	2·5	2·9	3·4	3·9	4·4	4·9	5·4	5·9	50
41	0·4	0·8	1·2	1·6	2·0	2·4	2·8	3·2	3·6	4·0	4·3	4·7	49
42	0·3	0·6	0·9	1·2	1·5	1·8	2·1	2·4	2·7	3·0	3·3	3·6	48
43	0·2	0·4	0·6	0·8	1·0	1·2	1·4	1·6	1·8	2·0	2·2	2·4	47
44	0·1	0·2	0·3	0·4	0·5	0·6	0·7	0·8	0·9	1·0	1·1	1·2	46

Table IV.—*Correction for the diminished Specific Gravity of the Air.*

Altitude above the Sea.		Altitude above the Sea.	
5000 F.	1·2 feet.	13000 F.	8·1 feet.
6000	1·7	14000	9·4
7000	2·3	15000	10·8
8000	3·1	16000	12·3
9000	3·9	17000	13·8
10000	4·8	18000	15·5
11000	5·8	19000	17·3
12000	6·9	20000	19·2

Rule.—Enter the Table with the altitudes above the sea of the upper and lower stations, and add the difference of the corresponding corrections to the altitude, or difference of level.

TABLE V.—*Dry Air. Logarithms of the Multipliers of the Logarithmic Differences (including the Correction for the diminished Specific Gravity of the Mercury.)*

Sum of therms.			Sum of therms.			Sum of therms.		
0°	0·749892		54°	0·775381		108°	0·799455	½°—217
1	0·750378		55	0·775839		109	0·799889	¾ —325
2	0·750864		56	0·776297		110	0·800322	
3	0·751348	¼°—121	57	0·776754		111	0·800755	
4	0·751832	½ —242	58	0·777211		112	0·801187	
5	0·752317	¾ —363	59	0·777667	¼°—114	113	0·801619	
6	0·752800		60	0·778123	½ —228	114	0·802050	
7	0·753282		61	0·778578	¾ —341	115	0·802481	¼°—107
8	0·753764		62	0·779033		116	0·802911	½ —215
9	0·754246		63	0·779488		117	0·803341	¾ —322
10	0·754727		64	0·779941		118	0·803771	
11	0·755207	¼°—120	65	0·780394		119	0·804200	
12	0·755687	½ —240	66	0·780847		120	0·804629	
13	0·756167	¾ —360	67	0·781299	¼°—113	121	0·805057	
14	0·756646		68	0·781751	½ —226	122	0·805485	
15	0·757124		69	0·782203	¾ —338	123	0·805913	¼°—107
16	0·757602		70	0·782654		124	0·806339	½ —213
17	0·758079		71	0·783104		125	0·806767	¾ —320
18	0·758556		72	0·783554		126	0·807193	
19	0·759032	¼°—119	73	0·784004		127	0·807618	
20	0·759508	½ —238	74	0·784453		128	0·808043	
21	0·759983	¾ —357	75	0·784902	¼°—112	129	0·808469	
22	0·760458		76	0·785350	½ —224	130	0·808893	
23	0·760932		77	0·785798	¾ —336	131	0·809317	¼°—106
24	0·761406		78	0·786246		132	0·809742	½ —212
25	0·761879		79	0·786693		133	0·810165	¾ —317
26	0·762351		80	0·787139		134	0·810587	
27	0·762823	¼°—118	81	0·787585		135	0·811010	
28	0·763295	½ —236	82	0·788031		136	0·811432	
29	0·763766	¾ —354	83	0·788476	¼°—111	137	0·811854	
30	0·764237		84	0·788920	½ —222	138	0·812275	
31	0·764707		85	0·789364	¾ —333	139	0·812696	
32	0·765177		86	0·789808		140	0·813117	
33	0·765646		87	0·790251		141	0·813536	¼°—105
34	0·766114		88	0·790694		142	0·813956	½ —210
35	0·766582		89	0·791136		143	0·814375	¾ —315
36	0·767050		90	0·791578		144	0·814794	
37	0·767517	¼°—117	91	0·792019	¼°—110	145	0·815213	
38	0·767983	½ —234	92	0·792460	½ —220	146	0·815631	
39	0·768449	¾ —350	93	0·792901	¾ —331	147	0·816048	¼°—104
40	0·768915		94	0·793341		148	0·816465	½ —208
41	0·769380		95	0·793781		149	0·816883	¾ —313
42	0·769845		96	0·794220		150	0·817299	
43	0·770309	¼°—116	97	0·794658		151	0·817715	
44	0·770772	½ —232	98	0·795096	¼°—109	152	0·818131	
45	0·771235	¾ —347	99	0·795535	½ —218	153	0·818546	
46	0·771698		100	0·795972	¾ —328	154	0·818961	
47	0·772160		101	0·796409		155	0·819375	¼°—103
48	0·772622		102	0·796846		156	0·819789	½ —207
49	0·773083		103	0·797282		157	0·820203	¾ —310
50	0·773543		104	0·797717		158	0·820616	
51	0·774003	¼°—115	105	0·798153		159	0·821029	
52	0·774463	½ —230	106	0·798587		160	0·821442	
53	0·774922	¾ —344	107	0·799021	¼°—108	161	0·821854	

Sum of therms.			Sum of therms.			Sum of therms.		
162°	0·822266		175°	0·827583		188°	0·832835	½°—201
163	0·822677	¼°—103	176	0·827989		189	0·833236	¾ —301
164	0·823088	½ —205	177	0·828395		190	0·833637	
165	0·823498	¾ —308	178	0·828800		191	0·834038	
166	0·823908		179	0·829205	¼°—101	192	0·834438	
167	0·824318		180	0·829610	½ —202	193	0·834838	
168	0·824728		181	0·830015	¾ —303	194	0·835238	
169	0·825137		182	0·830419		195	0·835637	¼°—100
170	0·825545		183	0·830822		196	0·836036	½ —199
171	0·825953		184	0·831226		197	0·836434	¾ —299
172	0·826361	¼°—102	185	0·831629		198	0·836832	
173	0·826769	½ —204	186	0·832031		199	0·837230	
174	0·827176	¾ —306	187	0·832433	¼°—100	200	0·837628	

Table VI.—*Mean Correction for Moisture.*

Sum of therms.	Add.	Sum of therms.	Add.	Sum of therms.	Add.	Sum of therms.	Add.
0° to 9°	0·4°	89° to 90°	2·1°	126°	4·0°	160°	7·1°
10 19	0·5	91 94	2·2	128	4·1	162	7·3
20 27	0·6	95 96	2·3	130	4·2	164	7·6
28 35	0·7	97 98	2·4	132	4·4	166	7·8
36 40	0·8	99 100	2·5	134	4·6	168	8·1
41 46	0·9	102	2·6	136	4·7	170	8·3
47 52	1·0	104	2·7	138	4·9	172	8·6
53 56	1·1	106	2·8	140	5·0	174	8·9
57 60	1·2	108	2·9	142	5·2	176	9·2
61 66	1·3	110 ...	3·0	144	5·4	178	9·5
67 70	1·4	112	3·1	146	5·6	180	9·8
71 74	1·5	114	3·2	148	5·8	182	10·1
75 76	1·6	116	3·3	150	6·0	184	10·4
77 80	1·7	118	3·5	152	6·2	186	10·8
81 84	1·8	120	3·6	154	6·4		
85 86	1·9	122	3·7	156	6·7		
87 88	2·0	124	3·9	158	6·9		

Article III.

On Hydracids, and their Combinations with Saline Bases. By Dr. J. Berzelius.*

By the term hydracid, we understand a combination of a simple or compound body with hydrogen, which, although destitute of oxygen, possesses all the essential characters of the oxygenous acids. Hydracids must be regarded, therefore, as constituted of hydrogen and a peculiar radical, which, as is the case with the oxygen acids, may be either simple or compound. In consequence of this dissimilarity, they naturally divide them-

* From Lärbok i Kemien, Andra Delen, Andra Upplagan; Appendix lxvi.

selves into two classes, viz. acids with a *simple*, and acids with a *compound radical.*

To the *first* of these classes belong: 1. *Sulphuretted hydrogen gas;* 2. *Seleniuretted hydrogen gas;* 3. *Telluretted hydrogen gas;* 4. *Hydrochloric acid;* and 5. *Hydriodic acid; chlorine* and *iodine* being regarded as simple substances. My principal arguments against the new theory of the constitution of muriatic acid were founded upon the little analogy which subsisted between it and its combinations, and the hydracids and their salts, in so far as the latter were known at the time of its first promulgation. The subsequent discovery, however, of many hydracids, and in particular of those with a compound radical, has developed the analogy between muriatic acid and the hydracids so completely, that these objections can no longer be considered as valid.

To the *latter* class; namely, hydracids with a compound radical, belong: 1. *Prussic acid* (cyanuretted hydrogen); 2. *Sulphoprussic acid* (sulphuretted cyanogen combined with hydrogen); 3. *Ferroprussic acid* (a combination of cyanuretted iron with cyanuretted hydrogen); and to these may be added, 4. Another compound not so accurately known, whose constituents, although differently proportioned, are also cyanogen and sulphur.

Sulphoprussic acid may be obtained by mixing a solution of sulphocyanuret of potassium in a minimum of water with concentrated phosphoric acid, and distilling in a retort with a gentle heat. The hydracid is volatilized, and condenses in the receiver. The potassium in this experiment, in order to combine with the phosphoric acid, oxidizes itself at the expence of water, and the hydrogen thus set at liberty unites at the same instant with the sulphuretted cyanogen.

Sulphoprussic acid is so constituted, that its elements, if reduced to the gaseous state, would all occupy the same volume; or it is composed by weight of hydrogen 1·68, azote 23·85, carbon 20·30, and sulphur 54·17. Its saturating capacity, as is the case with all hydracids, is such, that it combines with a quantity of a base whose oxygen is exactly sufficient to convert its hydrogen into water.

The radical of this acid, *sulphuretted cyanogen,* has not hitherto been isolated, and is known only in the compounds which it forms with hydrogen or with metals. When we attempt to obtain it by distilling a sulphuretted metallic cyanuret, it always undergoes decomposition: a metallic sulphuret containing a minimum of sulphur remains, and sulphuret of carbon, cyanogen, and azote are disengaged.

But cyanogen combines also with a double proportion of sulphur, and forms a bisulphuret, which in union with hydrogen affords a hydracid, differing in composition from the foregoing, and capable, like it, of combining with metals. This new acid was discovered by Wöhler. He found that when sulphuretted cyanuret of mercury is gently ignited in a glass vessel filled with muriatic acid gas, or with sulphuretted hydrogen gas, there is deposited upon the colder sides of the vessel a quantity of anhydrous sulphuretted prussic acid in the state of colourless transparent drops, which, after a few seconds, become solid, and form transparent, stellular, aggregated groupes of crystals. These crystals rapidly undergo decomposition, cyanogen is disengaged, and a pomegranate yellow, opaque, uncrystalline powder remains. This powder is insoluble in water, and is in every respect identical with the precipitate which is obtained when liquid sulphoprussic acid is boiled in contact with the atmosphere. It appears to be composed of prussic acid, combined with twice the proportion of sulphur which exists in sulphoprussic acid. That it is a hydracid, and not an anhydrous combination of sulphur and cyanogen, is proved by the circumstance that when we heat it with potassium, a combination, accompanied by ignition, ensues between the two substances, hydrogen gas is evolved, and the compound which remains consists of a mixture of the sulphuret and sulphocyanuret of potassium.

Combinations of Hydracids with Saline Bases.

A full exposition of the nature of the compounds of hydracids with saline bases constitutes a most essential part of the theory of these acids, because it furnishes the only means by which all the apparent inconsistencies can be reconciled. In attempting this explanation, two different views present themselves: either the hydracid unites without decomposition with the oxidated base, or its hydrogen decomposes the base, while the radicals of both the acid and base enter into combination. Of these views I consider the latter to deserve the preference; for if a solution of a salt obtained by saturating a hydracid with an oxide be evaporated, it very frequently happens that there crystallizes a reduced compound of the radicals of the acid and base, wholly destitute both of hydrogen and oxygen; and when such crystals contain water, that is, in the circumstances in which they may be regarded as compounds of an oxidated base with a hydracid, they frequently lose it in vacuo or in a dry atmosphere, and effloresce exactly like substances which lose merely water of crystallization. But these compounds of the radicals of hydracids with the radicals of bases resemble so intimately the salts which are formed by oxygen acids and oxides, that they coincide with them in all their physical properties, as taste, appearance, solubility in water, and in other liquids, and it would be difficult, without offering extreme violence to natural arrangement, to class them among any other substances except the salts. Gay-Lussac ascertained, for example, that if oxide of mercury be placed in contact with prussic acid gas, the latter is absorbed and water disengaged, while the product is cyanuret of mercury. This water proceeds from the hydrogen of the prussic acid and the oxygen of the oxide of mercury, neither of which enter into the new combination; but this compound, formerly called prussiate of oxide of mercury, possesses so close a resemblance to the salts of that oxide, in appearance, taste, and all its other properties, that the most positive evidence to the contrary would be requisite in order to convince one that it is not a salt of the oxide of mercury. I have myself ascertained, that if the ferroprussiate of potash, that is, a combination of prussic acid with potash and oxidule of iron, be crystallized, there results a compound, containing precisely the quantity of oxygen and hydrogen which would be necessary to convert it into a double salt of prussic acid with oxidated bases; but that these crystals lose the whole of their water like an efflorescent salt, either when confined in vacuo in the ordinary temperature of the atmosphere, or when exposed to dry air in a temperature between 77° and 86°; and it is certainly a far simpler view of the phænomena to regard this water as existing in the state of mere water of crystallization, than to assume that efflorescence, which can result only from the expansive force of water already formed, should have occasioned the mutual decomposition of the base and acid. Besides, we have never been able to discover any other difference between the compounds which are regarded as salts of hydracids, and those which indisputably contain no other ingredient except the radicals of the acids and bases, than that which subsists between salts with and without water of crystallization.

We adopt, therefore, in preference, the theory *that salts containing a hydracid do not exist*, but that when a hydracid is brought into contact with an oxidated base, the hydrogen of the acid combines with the oxygen of the base and forms water, while, at the same instant, the radicals of both unite mutually in their reduced condition, and the product is a substance which resembles so closely the salts of the oxidated radical of the base, that it cannot be distinguished from them in any of its physical characters. Hence, when sulphoprussic acid, which, although wholly destitute of oxygen, possesses a strong and pure acidity, is mixed with carbonate of potash, and when carbonic acid is thereby expelled with the same degree of effervescence which would be occasioned by the addition of an oxygen acid, the potash is decomposed by the hydrogen of the acid, and a combination ensues between sulphuretted cyanogen and potassium. If the mixture was sufficiently concentrated, it crystallizes, and the crystals contain neither hydrogen nor oxygen, but only potassium, sulphur, carbon, and azote; nevertheless they resemble a saline compound, particularly nitre, so perfectly, that they might be readily mistaken for crystals of that salt. In the new theory respecting the constitution of muriatic acid, the same decompositions take place when potash or soda is saturated with muriatic acid, and all the phenomena may be consistently explained on the supposition that chlorine is a simple body so soon as we cease to maintain the existence of hydrochlorated salts, and admit that what we have hitherto styled muriates are combinations of chlorine with the radicals of bases. For the nature of hydracids in general presupposes that these combinations must in every respect resemble salts.

The acid properties of a hydracid consist therefore in its *decomposing* bases, and not in saturating them; hence it follows that the property of acidity neither belongs to the substance itself, nor results from the nature of its constitution, but merely indicates a condition opposed to the property of alkalinity. In the case of hydracids, therefore, it depends at the same instant on the strong affinity which subsists between hydrogen and oxygen, and between the radicals of the acid and the base: and this is the reason why the radical of a hydracid possesses few or none of the characters of an acid substance, because it is unable, unless aided by hydrogen, to deoxidate or decompose the alkaline bases.

This reciprocal alteration in the elementary constitution of the acid and base, takes place even in the combinations of ammonia with hydracids. Here, the ammonia is converted into ammonium, by seizing upon the fourth atom of hydrogen which forms one of the constituents of the acid, and this ammonium subsequently enters into combination with the radical of the hydracid. For example, when muriatic acid gas is combined with ammonia, yielding the compound which, according to the old theory, was regarded as muriate of ammonia containing water of crystallization, the acid undergoes decomposition, the ammonia combines with its hydrogen, and is thereby converted into ammonium, and the latter, remaining in union with the chlorine of the muriatic acid, forms with it chloride of ammonium. When ammoniacal gas is mixed with chlorine, a portion of it is decomposed, the azote is disengaged, and the hydrogen, combining with another portion of the ammonia, forms with it ammonium, which now unites with the chlorine. Sal ammoniac is, therefore, a chloride of ammonium just as common salt is a chloride of sodium.

The resemblance between the compounds properly denominated salts, and those formed by the radicals of hydracids and of bases is so complete, that (as I have already observed), it is impossible, without offering violence to their external characters, to regard them as belonging to dissimilar classes of bodies. Nevertheless, in a theoretical of point view, there is a wide difference between the compounds of oxidated acids and bases, and those of combustible bodies without oxygen; and it may, perhaps, be argued from this, that our present theory supposes arrangements which have no existence in reality. Dulong has attempted to reconcile this inconsistency by regarding all acids which contain water as hydracids. He joins the oxygen of the water to the acid, and forms with the radical of the acid and its oxygen a compound radical, which, in union with the hydrogen of the water, constitutes the hydracid. Thus he regards hydrous sulphuric acid as a compound of hydrogen with a radical composed of an atom of sulphur and four atoms of oxygen; that is, containing one-third more of oxygen than is considered to exist in sulphuric acid. When this acid combines with a metal, potassium for example, hydrogen only is disengaged, and the potassium combines with the compound radical of the hydracid. The sulphate of potash thus formed ought to be regarded as a compound, not of sulphuric acid and potash, but of potassium and the radical of the hydracid (that is, sulphur with the whole quantity of oxygen, constituting a single integrant particle). When this hydracid is placed in contact with potash, the alkali is reduced by the hydrogen of the acid, water is formed, and the potassium unites with the compound radical of the acid. Again, if this acid be mixed with ammonia, no trace of water is produced, but the hydrogen unites with the ammonia, and forms ammonium, which now combines with the radical of the acid. Now there does not exist a single neutral ammoniacal salt, which does not contain this quantity of hydrogen; that is, which, according to the commonly received theory, does not contain a portion of chemically combined water, whose hydrogen corresponds with this quantity. This explanation of Dulong's is unquestionably entitled to considerable praise; because it re-establishes the harmony in the doctrine of salts which had been disturbed by the new theory respecting the nature of muriatic acid, and indeed to a still more general extent, by the phænomena accompanying the combinations of hydracids.

The combination of hydracids with saline bases gives rise both to acid and subsalts. The acid salts are produced when the saline looking compounds of the radicals of the acid and base combine with a new quantity of the hydracid, the result of which union is a body possessed to a greater or less extent of acid properties. Such are the ferruretted prussic acid, the compounds styled hydrosulphuretted alkalies, and according to the new theory respecting the nature of muriatic acid, the acid muriate of oxide of gold. Hitherto, however, we have become acquainted with only a very limited number of these acid salts. The compounds containing an excess of base are much less unfrequent, and they are produced when the neutral compounds of the radicals of the acid and base unite with a portion of the oxide of the latter; the result being a subsalt, coinciding in all its characters with the subsalts which the same oxidated basis forms with the oxygen acids. The acid salts may be regarded as double combinations of the radical of the acid with two combustible bodies, for example, ferroprussic acid is a double cyanuret of iron and hydrogen: the subsalts again may be regarded as double combinations of the radical of the base with oxygen and with the radical of the acid; for example, the substance styled subprussiate of mercury is a combination of cyanuret of mercury with oxide of mercury. The existence of these subsalts appears to me to furnish the strongest argument which, in the present state of our knowledge, can be advanced in favour of Dulong's theory respecting the constitution of salts.

From what I have now stated it is obvious that none of the compounds which have hitherto been styled prussiates, contain either prussic acid or an oxidated basis, but that they consist of cyanogen and the radical of the base: we call them, therefore, cyanurets, or sometimes metallic cyanurets (*cyanurer eller cyanmetaller*). This view enables us to explain why a solution of the cyanuret of potassium possesses such little permanency, and why its taste participates simultaneously both of potash and of prussic acid. All substances which have any tendency to combine with potash, as, for example, the carbonic acid of the atmosphere, the constituents of saliva, &c. determine the formation of that alkali by oxidation at the expence of water, and prussic acid is at the same instant disengaged.

Neither does there exist a class of salts corresponding with the name of sulphoprussiates, because here also, as we have already seen, the acid is decomposed by the bases, and compounds are formed, which for the present may be styled *sulphocyanurets* and *cyanosulphurets*, the latter appellation being reserved exclusively for those compounds which contain cyanogen united to the larger proportion of sulphur.

Article IV.

Notice of a Meteoric Stone which fell at Nanjemoy, in Maryland, North America, on Feb. 10, 1825. By Dr. Samuel D. Carver. In a Letter to Professor Silliman.*

I take the liberty of forwarding you a notice of a meteoric stone which fell in this town on the morning of Thursday, Feb. 10, 1825. The sky was rather hazy, and the wind southwest. At about noon the people of the town and of the adjacent country were alarmed by an explosion of some body in the air, which was succeeded by a loud whizzing noise, like that of air

* From the American Journal of Science for June, 1825.

rushing through a small aperture, passing rapidly in the course from north-west to south-east, nearly parallel with the river Potomac. Shortly after, a spot of ground on the plantation of Capt. W. D. Harrison, surveyor of this port, was found to have been recently broken, and on examination a rough stone of an oblong shape, weighing sixteen pounds and seven ounces, was found about eighteen inches under the surface. The stone, when taken from the ground, about half an hour after it is supposed to have fallen, was sensibly warm, and had a strong sulphureous smell. It has a hard vitreous surface, and when broken appears composed of an earthly or siliceous matrix, of a light slate colour, containing numerous globules of various sizes, very hard, and of a brown colour, together with small portions of brownish yellow pyrites, which become dark coloured on being reduced to powder. I have procured for you a *fragment** of the stone, weighing *four pounds* and *ten ounces*, which was all I could obtain. Various notions were entertained by the people in the neighbourhood on finding the stone. Some supposed it propelled from a quarry eight or ten miles distant on the opposite side of the river; while others thought it thrown by a mortar from a packet lying at anchor in the river, and even proposed manning boats to take vengeance on the captain and crew of the vessel.

I have conversed with many persons living over an extent of perhaps fifty miles square; some heard the explosion, while others heard only the subsequent whizzing noise in the air. All agree in stating that the noise appeared directly over their heads. One gentleman, living about 25 miles from the place where the stone fell, says, that it caused his whole plantation to shake, which many supposed to be the effect of an earthquake. I cannot learn that fire-ball or any light was seen in the heavens —all are confident that there was but one report, and no peculiar smell in the air was noticed. I herewith transmit the statement of Capt. Harrison, the gentleman on whose plantation the stone fell.

Statement of W. D. Harrison, Esq.

On the 10th of Feb. 1825, between the hours of twelve and one o'clock, as nearly as recollected, I heard an explosion, as I supposed, of a cannon, but somewhat sharper. I immediately advanced with a quick step about twenty paces, when my attention was arrested by a buzzing noise, resembling that of a humming bee, which increased to a much louder sound, something like a spinning-wheel, or a chimney on fire, and seemed directly over my head; and in a short time I heard something fall. The

* This specimen is not yet received.—*Amer. Ed.*

time which elapsed from my first hearing the explosion, to the falling, might have been fifteen seconds. I then went with some of my servants to find where it had fallen, but did not at first succeed (though, as I afterwards found, I had got as near as 30 yards to the spot); however, after a short interval, the place was found by my cook, who had (in the presence of a respectable white woman) dug down to it before I got there, and a stone was discovered from 22 to 24 inches under the surface, and which, after being washed, weighed sixteen pounds, and which was no doubt the one which I had heard fall, as the mud was thrown in different directions from 13 to 16 steps. The day was perfectly clear, a little snow was then on the earth in some places which had fallen the night previous. The stone when taken up had a strong sulphureous smell; and there were black streaks in the clay which appeared marked by the descent of the stone. I have conversed with gentlemen in different directions, some of them from 18 to 20 miles distant, who heard the noise (not the explosion). They inform me that it appeared directly over their heads. There was no fire-ball seen by me or others that I have heard. There was but one report, and but one stone fell to my knowledge, and there was no peculiar smell in the air. It fell on my plantation, within 250 yards of my house, and within 100 of the habitation of the negroes.

I have given this statement to Dr. Carver, at his request, and which is as full as I could give at this distant day, from having thought but little of it since. Given this 28th day of April, 1825. W. D. Harrison,

Surveyor of the port of Nanjemoy, Maryland.

Article V.

Astronomical Observations, 1825.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Siderial Time.

1825.	Stars.	Transits.		
July 26.	— θ Ophiu.	17h	11′	21·10″
26.	— b Ophiu.	17	15	46·29
26.	— e^2 Ophiu.	17	20	49·86
26.	— d Ophiu.	17	33	01·77
26.	— Moon's First or West Limb	17	47	29·56
26.	— μ^2 Sagitt.	18	03	22·74
26.	— 21 Sagitt.	18	15	01·08

1825.	Stars.	Transits.		
July 27.	—21 Sagitt.	18h	15′	00·92″
27.	—135 Sagitt.	18	27	30·92
27.	—131 Sagitt.	18	28	32·10
27.	—28 Sagitt.	18	35	29·91
27.	—30 Sagitt.	18	40	24·05
27.	—31 Sagitt.	18	41	42·60
27.	—Moon's First or West Limb	18	46	01·18
27.	—o Sagitt.	18	54	16·41
27.	—π Sagitt.	18	59	25·97
27.	—4 Sagitt.	19	02	04·99
27.	—61 Sagitt.	19	10	13·39
27.	—138 Sagitt.	19	20	35·63
28.	—ρ^1 Sagitt.	19	11	35·63
28.	—138 Sagitt.	19	20	35·23
28.	—176 Sagitt.	19	26	18·23
28.	—e^1 Sagitt.	19	30	46·39
28.	—e^2 Sagitt.	19	32	34·89
28.	—Moon's First or West Limb	19	41	47·89
28.	—g Sagitt	19	48	05·63
28.	—β Capric.	20	11	15·00
29.	—β Capric.	20	11	14·82
29.	—π Capric.	20	17	22·70
29.	—g Capric.	20	18	56·84
29.	—13 Capric.	20	27	36·86
29.	—τ Capric.	20	29	33·16
29.	—Moon's First or West Limb	20	34	29·33
29.	—325 Capric.	20	41	07·23
29.	—351 Aquarii	20	43	35·50
29.	—8 Aquarii	20	50	21·73
29.	—ν Aquarii	21	00	07·55
30.	—14 Aquarii	21	06	58·24
30.	—17 Aquarii	21	12	39·33
30.	—19 Aquarii	21	15	52·91
30.	—Moon's Second or East Limb	21	26	25·14
30.	—ϵ Aquarii	21	28	30·17
30.	—c^1 Capric.	21	35	44·52
30.	—c^2 Capric.	21	37	00·42
30.	—30 Aquarii	21	54	08·28
30.	—2 Aquarii	22	01	30·46
31.	—c^1 Capric.	21	35	44·58
31.	—c^2 Capric.	21	37	00·45
31.	—345 Aquarii	21	49	07·32
31.	—30 Aquarii.	21	54	08·12
31.	—2 Aquarii	22	01	30·42
31.	—θ Aquarii	22	07	40·00
31.	—Moon's Second or East Limb	22	13	52·63
31.	—166 Aquarii	22	28	45·67
31.	—183 Aquarii	22	31	49·13
31.	—219 Aquarii	22	38	52·96

Occultation by the Moon.

Immersion of b Sagittarius. . . . . 17h 27′ 59·8″ Siderial Time.

Article VI.

On the Comparative Advantages of Oil and Coal Gas. By Robert Christison, MD. FRSE. and Edward Turner, MD. FRSE.*

The question of the relative advantages of oil and coal gas resolves itself into two: the first regards their relative economy; the second their comparative utility.

1. Before we can determine their relative economy, it is requisite to settle their average quality. Taking their specific gravity as the ground of comparison, we apprehend that, in small towns, where the cannel coal can be had at a low price, coal gas companies may be able to manufacture a gas of the density of 700. In larger cities, such as Glasgow and Edinburgh, where coal of every kind is dearer, and the cannel coal cannot easily be procured in sufficient quantity, the average specific gravity of the gas will not exceed 600. And in such a town as London, where the cannel coal can scarcely be procured at all, the average specific gravity will not exceed 450.

The average specific gravity of oil gas should eventually be the same every where. It is difficult to ascertain what the average is at present, as made by large establishments; but there is no substantial cause why it should fall short of 920. We have assigned strong reasons, however, for believing that it must be soon improved considerably. This improvement indeed may be no great gain; for the question will then occur, whether it can be effected without diminishing the quantity of gas in the same proportion with its increase in quality. It is generally supposed, that an improvement in the quality of oil gas is necessarily attended by a loss in quantity; but, so far as can be discovered, this idea rests on experiments performed by operatives only, whose authority we are satisfied, from repeated observation, can by no means be relied on. If charcoal is left in the retorts at the end of each charge, it is clear that the gas may be improved by the addition of all this charcoal, without any diminution in quantity; for, if it be added to the light carburetted hydrogen, which gives little light, so as to convert it into the olefiant gas, which is powerfully illuminating, the change, it is well known, will take place without any alteration in volume. On the other hand, if good oil gas be exposed to a high temperature, it is partly decomposed, and deposits some of its charcoal. Part of the olefiant gas becomes light carburetted hydrogen, and without any increase in volume; for the volume is not increased unless it is resolved into charcoal and hydrogen.

* From the Edinburgh Philosophical Journal.

Hence a bad gas may be made from oil, which shall not exceed in quantity the good gas of Taylor and Martineau. And, in point of fact, we have several times found, when the retorts were choked with charcoal, and the specific gravity of the gas was only 660, that the quantity fell short of 100 cubic feet per gallon, which is said to be about the average produce when the gas is good. When oil gas has a specific gravity of 910, charcoal is still found in the retorts. It may therefore be improved by the addition of all this charcoal, and still retain its volume. Besides it may be possible to improve it by the addition of charcoal from other sources. Hence, while we at present assign to oil gas the average specific gravity of 920, we cannot help anticipating a considerable improvement, and positive gain.

From what has been said of the average quality of coal gas in different quarters of the kingdom, it is clear that the question of its economy, compared with oil gas, can be only answered relatively. In Edinburgh and Glasgow, where coal is moderately cheap, and coal gas of good quality, oil gas must be somewhat dearer; in London, where the coal is dear, and the gas bad, oil gas should be positively cheaper; and in other places the two will be nearly the same in price. This statement is, of course, drawn from our own experiments on their illuminating power, coupled with the well-known computations of Accum, Peckston, Ricardo, and others, regarding their relative cost.

The second element in the question of their relative advantages, is their comparative utility. It is certain that whatever difference may exist between them in this respect must be in favour of oil gas.

In the first place, the quality of the light is superior. It is whiter, and has a peculiar sparkling appearance, superior to that of coal gas. It is therefore a more beautiful light, fitter for the artificial illumination of colours, and not liable to give the human countenance that unpleasant sallow appearance which every one has observed to be caused by coal gas.

An objection has been urged to the employment of gas in general, that it has a disagreeable odour. This objection does not apply at all, unless the gas is unconsumed; for neither oil nor even coal gas, so far at least as our observation goes, emits any odour, if properly burnt. But if they escape, and mix with the air, their presence is then readily detected by the smell. The odour of oil gas is purely empyreumatic, but quite distinct; we have possessed occasional specimens, which had a faint smell, but we never found it altogether inodorous. The best oil gas appears to have the least smell. The odour of coal gas is of a mixed kind, being in part empyreumatic like oil gas, and partly of an exceedingly offensive nature, like that of sulphuretted hydrogen. In Edinburgh coal gas we have generally observed the empyreuma alone; but frequently the other is perceptible also, and sometimes it prevails to an insufferable degree.

The most serious objection to coal gas arises from the presence of impurities. These are, a black matter like tar, and compounds of sulphur,—all derived from the coal itself, and therefore necessarily present originally in every description of coal gas. Without purification, therefore, coal gas could scarcely be used at all; and it becomes a question of importance to determine, whether or not the noxious ingredients may be wholly removed from it. The greater part of the tar is deposited at the works in the proper vessels, but a minute portion does commonly pass over with the gas. It tends to clog the apertures of the burner, and of course soils substances upon which it is deposited. In common shops, where a free current of air is preserved, the effect is hardly noticed; but we suspect that a part of the inconvenience found by jewellers to attend the use of coal gas arises from this cause.

The most formidable of the compounds of sulphur present in coal gas is sulphuretted hydrogen. The presence of this gas is hurtful in two ways. If it escape unburnt, it offends by its insupportable odour, and attacks silver, and paint, with great readiness. When consumed, it forms sulphurous and sulphuric acids, which may injure the health, if habitually inspired, and act chemically on various substances, as on iron and steel. Hence the necessity of removing it entirely from coal gas. On this subject two important questions naturally occur, to both of which we can give a decisive answer. 1st, Can sulphuretted hydrogen be wholly separated from coal gas? and, 2dly, when it is removed, Can coal gas be regarded as perfectly free of sulphur?

We are satisfied that sulphuretted hydrogen may be wholly removed; for we have repeatedly examined the Edinburgh coal gas by the most delicate tests, without detecting a trace of it. Of course we do not vouch that it is always equally pure, because the least neglect, on the part of the workmen, must inevitably cause some sulphuretted hydrogen to escape into the pipes. It is equally certain, however, that coal gas, when completely free of sulphuretted hydrogen, still contains sulphur. On burning a small jet of coal gas, free from sulphuretted hydrogen, so as to collect the fluid formed during the combustion, the presence of sulphuric acid was uniformly detected, demonstrating the existence of some compound of sulphur. What that compound is has not yet been ascertained; but from its peculiar unpleasant odour, and the circumstances under which it is generated, the sulphur is most probably in combination with carbon, either in the form of the volatile liquid, sulphuret of carbon, as Mr. Brande conjectures, or, what is perhaps more likely, as a gaseous compound, containing a less proportion of sulphur than exists in that liquid.

In whatever state of combination the sulphur may be, it does not affect the salts of lead like sulphuretted hydrogen; nor does it act so readily, if at all, on polished silver and gold. Hence the gas which contains only this impurity, will be less injurious, when any of it escapes unburnt, than such as contains sulphuretted hydrogen; but since it uniformly yields acid vapours during its combustion, one part of the objection remains in full force.

These various objections, whatever weight they may have, apply to coal gas only.

Article VII.

A Synopsis of the Genera of Reptiles and Amphibia, with a Description of some new Species. By John Edward Gray, Esq. FGS. &c.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *British Museum, July* 12, 1825.

The reptiles have been comparatively neglected by recent zoologists, perhaps on account of the popular prejudices against this interesting and curious class of animals which Linnæus designates "Animalia pessima tetra nuda." It is only necessary to overcome these prejudices, and to examine them even superficially, and we cannot but be struck with the beauty of their colours, the wonderful nature of their structure, and the peculiarities of their habits and manners. Indeed I do not know any class of animals better calculated to excite the wonder and astonishment of a student of nature.

With the hopes of inducing some inquiry into, and examination of, this department of natural history, I have attempted to bring together into the form of a synopsis, the labour of the preceding writers on this class, and have also thrown into it my own notes formed on an examination of the specimens at present under arrangement in the British Museum, which are exceedingly interesting to me in several points of view, first, as containing several undescribed species, and specimens of interesting or obscure genera; and secondly, the older specimens having been examined, and most carefully named by my late uncle, who paid great attention to this department of zoology, and several of whose manuscript species still remain unpublished.

I need not dwell on the distinctness of the two classesof reptiles and amphibia, or of the scaly and naked-skinned groups, as they are allowed to be perfectly distinct by all modern naturalists, although they do not agree with regard to the rank of the latter group. I am inclined to follow the opinion of Macleay, Blainville, and others, in considering them both as classes, and consequently of equal rank.

Class III.—Reptilia.

Body covered with scales or hard plates imbedded in the skin; heart with two auricles and one ventricle respiring by lungs. The blood is cold; the windpipe ringed; the ribs are perfect, and there are several vertebræ; the penis is distinct, sometimes double. The egg is covered with a shell, mostly hatched in the body of the mother.

Synopsis of the Orders.

Body covered with imbedded hard plates; legs distinct.
Ears closed with a valve. .... Emydosauri.
Ears naked, valveless .... Sauri.

Body covered with scales, or two large shields.
Legs 2-4 weak; ears naked .... Sacrophidii.
Legs 0; ears 0 .... Ophidii.
Legs 4; body covered with two shields. .. Chelonii.

Mr. Macleay, in his excellent *Horæ Entomologicæ*, has observed that the order of this class appears to assume a circular disposition; the most visible break in this arrangement is in the passage between the snakes and the tortoises; for the connexion between the latter order and the crocodiles must be visible to every one, if they only consult Shaw's figure of the *Testudo serpentina*, and compare it with that of the crocodile, for it is in fact a crocodile with a shortened body, covered with united instead of distinct shields, and a bird's beak. The passage from the crocodiles to the lizards by means of the *Minitors*, has long been known to naturalists, who have often considered the latter as species of the former genus; and even Linnæus placed them in the same section of his genus Lacerta. The Sincs have always been placed in the same genus or group with the lizards; but their affinity with the slow-worms did not escape the penetrating eye of Linnæus, who observes that the Lacerta Chalcides, is "Media inter Lacertas et Angues;" and the union of the genera *Sincus*, *Anguis*, and *Amphisbena*, into an order, although it has not been done by any zoologist that I am aware of, appears to be strictly natural, for the feet in this order exist in such various degrees of developement, that the being with or without them appears to be only a family or generic character, and not ordinal. Linnæus placed the genera *Tortrix* and *Eryx* of the true serpents as species of his genus *Anguis,* thus showing that he considered them as nearly allied. So far the passage from one order to the other has been very easy and gradual; and indeed sometimes I have been doubtful, as in the last case, to which order I should refer the genera. There is every reason to believe from general structure, that there exists an affinity between the tortoises and the snakes, but the genus that exactly unites them is at present unknown to European naturalists, which is not astonishing when we consider the immense number of undescribed animals which are daily occurring.

Mr. Macleay thought these two orders might be united by means of *Emys Longicollis* (the long-necked tortoise) of Shaw; but the family to which this animal belongs appears to be the one which unites this class to the crocodile: if I may be allowed to speculate from the peculiarities of structure which I have observed, I am inclined to think that the union will most probably take place, by some newly discovered genera, allied to the marine or fluviatile soft-skinned turtles, and the marine serpent.

§ 1. *Body covered with imbedded hard plates; legs distinct, fit for walking.* Loricata, *Gray;* not *Merrem.*

Order 1. Emydosauri, *Blainv.*

Ears closed by two longitudinal valves; anus longitudinal; body covered with large imbedded plates; tongue short adnate; legs four; toes four before, five behind; sternum long; clavicles none; lungs not extended to the abdomen; *living in or near water.*

Fam. I. Crocodilidæ.

Feet three clawed; hinder ones; palmate or semi palmate tail compressed.

1. Aligator, *Cuv.*
Head blunt; hind feet semi-palmate. *America.*
A. lucius, *Gray.* Crocodilus lucius, *Cuv.*

2. Crocodilus, *Cuv.* Champse, *Merrem.*
Head blunt; hind feet palmate. *Old and New Continent.*
C. biscutatus, *Cuv.*

3. Gavial, *Oppel.* Gavials, *Cuv.*
Head very long; hind feet semi-palmate. *Old Continent.*
G. gangeticus, *Gray.* Crocodilus gangeticus, *Cuv.*

Fam. II. Icthiosauridæ.

Feet paddle-shaped; toes five; cervical vertebra 18. *Marine.*

1. Icthiosaurus, *Kœnig.* Proteosaurus, *Home.*
Teeth in a grove.
Latreille applied the name Ichiosaurus to the larva of a sala-

mander; but the genus has been properly rejected by all latter zoologists.

I. communis, *Kœnig.*

2. Saurocephalus. *Harlan,* 1824.

Teeth in separate sockets.

S. .... *Harlan.*

Fam. III. Plesiosauridæ.

Feet paddle-shaped; toes five; cervical vertebra 35 or 41. Marine.

1. Plesiosaurus, *Conybeare.*

P. dolichodeirus, *Conybeare.*

Cuvier has described a genus of large lizard fossil under the name of Geosaurus, *Oss. Fos.* v. ii. 328, which he says is intermediate between the *monitors* and *crocodiles.*

The genus Megalosaurus of *Buckland, Geol. Trans.* is, perhaps, allied to this order.

Order II. Sauri, *Blainv.*

Drum of the ears naked, or covered with the skin; anus transverse; body covered with large and small imbedded scales; legs four, toes 5, before and behind; sternum short; clavicles distinct; lungs extended into the abdomen; *living mostly on land.*

§ 1. *Tongue not extensile.* Ascolabata, *Merrem.*

Fam. I. Stellionidæ. Stelliones, *Cuvier.*

Toes free; inequal; body subcompressed; throat subpendulous, extensile.

The throat of all, but more especially of the species of the latter section of this family, are more or less capable of being dilated by the processes of the os hyoides, as noticed by Baron Cuvier in his Essay on the Osteology of Lizards (Ossment Fossiles, v. ii. p. 281); and it has lately been described and figured in an excellent paper by Mr. Bell, in the Zoological Journal, as existing in the genus *Anolius.*

† *Without any teeth in the throat; teeth equal, conical; toes simple,* Agamina, *Gray.* Stellionidæ, *Bell,* without character.

Gen. 1. Uromastrix, *Merrem.*

Body and head scaly; tail with large whirled pointed scales; femoral pores distinct.

U. Richii, *Gray.* U. acanthinurus, *Bell,* not U. Anthurus, *Merrem.* Common Africa.

2. Zonurus, *Merren.* Cordylus, *Gronovias.*

Body scaly; head and abdomen shielded; tail whorled; spinose; femoral pores distinct.

Z. Cordylis, *Merren.* L. Cordylus, *Lin.*

3. AGAMA.

Body and head scaly; tail with small scales; femoral pores none; toes 5-5.

This genus contains the following subgenera characterized by the form of the scales, &c.

1. Stellio, *Daud;* St. vulgaris; Lacerta stellio, *Lin.* 2. Agama, *Daud;* A. muricata, *Daud.* 3. Tapayia, *Gray;* T. orbicularis; Lacerta, *Lin.* 4. Trapelus, *Cuv.*; T. mutabilis, *nob.* Calotes, *Merren.* 5. Calotes, *Cuv.*; C. ophiomachus, *nob.*; Lacerta Calotes, *Lin.* 6. Lyriocephalus, *Merrem* (Lophyrus, *Oppel.*). L. margaritaceus, *Merren.*

4. PNEUSTES, *Merrem.* Agama, *Daud.*

Toes four before, five behind; tail prehensile.

P. prehensilis, *Merrem.* Carapopeba, *Margrave.*

5. BASILICUS, *Laurent.*

Head and body scaly; tail with a dorsal fin supported by bony rays; femoral pores distinct.

B. mitratus, *Daud.*

6. DRACO, *Lin.*

Head and body scaly; sides of the body with wing-like expansions supported by the spurious ribs; femoral pores none; tail round, scaly.

D. viridis, *Daud.*

7. PTERODACTYLUS, *Cuv.*

The index finger of the fore foot longer than the body "supporting a flying membrane," *Cuv.*

P. longirostris, *Cuv.*

†† *With teeth in the throat.*

8. CLAMYDOSAURUS, *Gray.*

Head and body scaly; tail round scaly; neck with a large pliated frill on each side; femoral pores none.

C. Kingii, *Gray.* New Holland, Capt. P. King; see the inedited Journal of his Voyage. (I am not certain that this genus has palatine teeth.)

9. IGUANA, *Daud.*

Iguanina, *Gray.*

Teeth unequal or compressed, denticulated; head shielded; body scaly; back furnished with a dorsal crest; femoral pores distinct; toes 5-5 simple; tail crested.

I. tuberculata, *Laur.* Lacerta Iguana, *Lin.*

10. CYCLURA, *Harlan.*

Head —— ? body scaly; back with a dorsal crest; femoral pores distinct; toes 5-5 simple; tail with large whorled pointed scales.

C. carinata, *Harlan, Acad. N. S. Phil.* 1824.

11. Amblyrhyncus, *Bell.*
Head short, truncated, above tuberculated; body scaly; neck back and tail with a spiny crest; toes 5-5 simple, nearly equal; femoral pores distinct; teeth trilobate.
A. cristatus, *Bell, Zool. Journ.* ii. t. 12. Mexico.

12. Polychrus, *Cuv.*
Head pyramidical shielded; body scaly, inflatile; not crested; femoral pores distinct; toes 5-5 simple.
P. marmoratus, *Merrem.*

13. Anolius, *Cuv.* Anolis, *Merrem.*
Head pyramidical; scaly (or subscutate); body scaly; toes 5-5 very unequal, ends dilated.
This genus may be divided into several subgenera, according to its scales and dorsal crests.
1. Anolius. A. padagricus, *Daud.* 2 .... Lacerta bullaris, *Lin.* 3 .... A. limeatus. *Daud.* 4. A. Cuvieri, *Merrem;* allied to *Basilicus?*
The fossil genus *Mosasaurus* of Conybeare, according to *Cuvier, Oss. Fos.* v. ii. 337, is intermediate between the *Agamina* and the *Iguanina.*

Fam. II. Geckotidæ.
Toes nearly equal, mostly dilated, beneath transversely scaly; body depressed; throat not extensile; teeth conical or three-lobed; none in the palate.

1. Phyllurus, *Gray.* Phyllures, *Cuv.*
Tail depressed, lanceolate; toes simple, filiform, clawed; body and head scaly.
P. Whitii, *nob.* Lacerta platura, *White's Jour.* Agama. Platyura, *Merrem.* Perhaps belong to the former family.

2. Uroplates, *Daud.*
Tail depressed, edged with a membrane; toes semi-palmate, dilated at the ends, scales longitudinally divided, claws sunk in the grove.
*U. fimbriatus. Stellio fimbriatus, *Schneid.*** Caudiuerbera, *Laur.* C. cristatus. Lacerta caudiverbera, *Lin.* ***Gecko tetradatylus, *Merrem.*

3. Ptyodactylus, *Gray.* Ptyodactyles, *Cuv.*
Tail round; toes free, dilated at the end, scales longitudinally divided, claws sunk in on the grove; femoral pores none.
P. lobatus. L. gecko, *Hasselt.*

4. Thecadactylus. Thecadactyles, *Cuv.*
Tail round, scaly; toes dilated their whole length, furnished beneath with scales divided by a longitudinal fullow, containing the claws; thumb clawless; thigh poreless. *America.*
T. lævis. Greco levis, *Daud.* Lac. rapicauda, *Gmelin.*

5. HEMIDACTYLUS. Hemidactyles, *Cuv.*

Tail round, beneath ringed; toes dilated at the base into an oval disk, formed of two series of scales; claws and femoral pores distinct. *Old Continent.*

*H. tuberculosus, *Gray.* Gecko, *Daud.* H. maculatus, *Gray.* Gecko maculatus, *Merrem.* **? H. triedrus; H. aculeatus; and H. platyurus, Gecko, *Merrem;* belong to this genus.

6. GECKO.

Tail round, scaly; toes dilated their whole length, furnished with transverse series of scales, clawed; thumb clawless; femoral pores distinct.

G. verus, *Merrem.* Lacerto gecko, *Lin.* G. vittatus and G Spectator, *Merrem,* belong to this genus.

7. TARENTOLA, *Gray.*

Tail round, scaly; toes dilated their whole length, furnished with transverse series of scales; thumb, index, and little fingers clawless; femoral pores none.

T. stellio. Gecko stellio, *Merrem.* Lacerta Mauritanica, *Lin.*

8. PLATYDACTYLUS, *Gray.* Platydactyles, *Cuv.*

Tail round, scaly; toes dilated their whole length, furnished with a series of scales, clawless; femoral pores none; thumb very small. *Isle of France.*

P. Cuvieri, *Gray.* Gecko inungius, *Cuv.* P. ocellatus, *Gray.* Gecko, *Cuv.* and P. squalidus, *Gray.* Gecko, *Daud.*

9. PHELSUMA, *nob.*

Tail round scaly; toes dilated their whole length, furnished with a series of scales, clawless; thumb small; femoral pores distinct. *Isle of France.*

P. crepidianus, Gecko, *Merrem.* P. ornatum, *Gray.* Brown, back ornamented with six rows of red oval spots. Capt. King.

§ 11. *Tongue extensile.* Sauraæ, *Merrem.*

Fam. III. TUPINAMBIDÆ.

Tongue deeply two cut, very extensile; teeth only in the jaws; tail mostly laterally compressed; subaquatic (allied to the Emydosauri).

1. URANUS, *Merrem.* Tupinambis, *Lam.* Monitors, *Cuv.*

Teeth conical; throat collarless; head and body scaly; belly annulated; toes 5-5; femoral pores none. *The Ancient Continent.*

**Tail rounded.* U. Dracæna, *Merrem.* L. Dracæna, *Lin.*
***Tail compressed, beneath rounded.* 1. U. varius, *Merrem.* Lacerta varia, *White,* N. H.

2. Ada, *Gray*. Dragonnes, *Cuv.*

Head shielded; body scaly, with larger shields on the back; throat with two pleats; toes 5-5; femoral pores distinct; teeth conical; tail compressed at the end. *America.*

A. crocodilinus, *Gray*. Teius crocodilinus, *Merrem.* La Dragonne, *Lacepede.*

3. Teius, *Merrem.* Les Sauvegardes, *Cuv.*

Head shielded; body scaly, scale of the abdomen long; throat with two pleats; toes 5-5, or 5-4; femoral pores distinct; teeth denticulated; tail compressed. *America.*

T. bicarinatus, *Merrem.* Lacerta, *Lin.*

4. Ameiva, *Say.*

Head shielded; body scaly; scale of the abdomen broad; throat with two pleats; toes 5-5; femoral pores distinct; teeth denticulated; tail round. *America.*

A. vulgaris. Lacerta Ameiva, *Gmelin.*

Fam. IV. Lacertinidæ.

Tongue deeply two cut; very extensible; teeth in the jaws and palate; tail round; neck surrounded with a collar of larger scales; toes 5-5.

1. Lacerta, *Lin. Cuv.*

Head and abdomen shielded; back scaly; a collar of larger scales round the throat; femoral pores distinct; teeth conical.

L. agilis, *Lin.*

2. Tachydromus, *Oppel.* Takydrome, *Daud.*

Head, back, and abdomen shielded; femoral pores none, with two vesicles at the anus.

T. sexlineatus, *Daud*, t. 39.

The species of this family require further division and examination: the latter genus is allied in form to the next order, or *Saurophidii.*

Fam. V. Camelionidæ.

Tongue round, club-shaped, very extensile; teeth three-lobed; tail prehensile; body and head minutely scaly; toes 5-5, united; two and three together, clasping; tympanum covered with the skin.

1. Chamelion, *Lin.*

The only genus as yet known in the family.

*C. vulgaris, *Latr.* Lacerta chamæleon, *Lin.* *Africa.*

**C. calcaratus, *Merrem.* ***C. bifidus, *Brogniart.*

This family is allied to several of the *Stellionidæ*, as *Pneustes*, &c. but its affinity with *Lacertunidæ* is not so apparent.

§ 11. *Body covered with scales or a bony case; legs often* *wanting, or too small for walking; sometimes adapted for swimming.*

Order III. SAUROPHIDII, *Gray.*

Drum of the ear deep seated, partly covered with a posterior transverse valve or by the skin; eyes furnished with longitudinal eyelids; skin covered with uniform imbricate scales, or rings of square plates; feet two, or four small, weak, sometimes wanting; occipital condyle three cut; lungs two unequal, or rarely only one; ossa quadratam one on each side; upper maxilla immoveable.

§ 1. *Body covered with imbricate scales; anus transverse, not terminal; tongue extensile?*

Fam. I. SINCIDÆ, *Gray.*

Body fusiform; scales uniform, shining; tongue fleshy, slightly extensile; teeth denticulated; drum of the ear deep, partly covered with a transverse posterior valve; legs four weak; toes nearly equal.

1. SINCUS, *Daud.*

Body fusiform, uniformly scaly; head shielded; feet four; femoral pores none; toes 5-5; teeth in the jaws, and two rows in the palate.

S. officinalis, *Schneider.* Lacerta Sincus, *Lin.*

2. TILIQUA, *Gray.*

Body fusiform, uniformly scaly; head shielded; feet four; femoral pores none; toes 5-5; teeth only in the jaws.

T. tuberculatus, *Gray.* Lacerta Sincoides, *White.*

3. GYMNOPHTALMUS, *Merrem.*

"Body fusiform; head shielded; feet four; femoral pores ....? toes 4-5; teeth conical (only in the jaws?); tongue two-forked; eyelids none," *Merren.*

G. quadrilineatus, *Merrem.* Lacerta, *Lin.*

4. TRACHYDOSAURUS, *Gray.*

Body fusiform; head shielded; back covered with hard bony scales, like the frontal shelds in form; abdomen with thin scales; feet four; toes 5-5; femoral pores none; tail short depressed.

T. rugosus, *Gray.* New Holland, Capt. P. P. King, RN.

5. CICIGNA, *Gray.*

Body subfusiform, with a distinct lateral line; head shielded; feet four; femoral pores distinct; toes 5-5 unequal.

C. sepiformis, *Gray.* Sincus sepiformis, *Schneider.*

Fam. II. ANGUDIDÆ.

Body cylindrical; scales uniform, shining; tongue fleshy necked; drum of the ear, partly covered with a transverse posterior valve; feet four or two, weak; anus transverse, not terminal.

1. Seps. *Daud.* Chamæsaura, *Schneider.*
Head shielded; legs four; toes 3-3; body without any distinct lateral line; scale uniform.

S. chalcidica, *Merrem.* Grey, with nine grey lines above; tail longer than the body; scales of the head unequal. Lacerta chalcides, *Lin.* C. chalcis, *Schneid.* Chalcides Seps, *Latreille.* Seps. tridactylus, *Daud.*

S. equalis, *Gray.* Grey ......; tail thick, half as long as the body (perhaps injured); scales of the head equal; head and body 30-10; tail 17th of an inch; scale of the head numerous, very nearly equal.

2. Tetradactylus, *Merrem.*
Head shielded; legs four; toes 4-4; body furnished with a distinct lateral line; scale of the back quadrade of the abdomen hexagonal; tongue short entire.

T. Chalcidicus, *Merrem.* Lacerta tetradactylus, *Lacep. Ann. Mus.*

3. Monodactylus, *Merrem.* Chamæsaura, *Schneid.*
Head shielded; body with acute keeled scales; feet four; toe one to each foot; tongue short entire.

M. anguinus, *Merrem.* Lacerta anguina, *Lin.* Chalcides pinnata, *Laur.* C. anguinea, *Laur.*

4. Bipes, *Laup.*
Head shielded; body with imbrical scales; fore feet hid in the skin; hind feet with two toes; tongue short apex necked.

B. anguinus, *Merrem.* Lacerta bipes, *Lin.*

Merrem describes from Gronovius an animal under the names of *Pygodactylus Gronovii,* but he doubts it being distinct from the former; it is only said to differ in having only one toe to the hind feet. Cuvier, R. A. describes the former as only having one; on what authority I do not know.

5. Pygopus, *Merrem.*
Head shielded; body with a distinct lateral line ("back scaly; abdomen with small shelds," *Merrem*); femoral pores distinct; eyes large; drum of the ear large; teeth in the jaws only; tongue short, entire; fore feet hid in the skin; hind feet clawless; rounded, lobed.

P. lepidopus, *Merrem.* Bipes lepidopus, *Lacep.* N. Holland.

6. Pseudopus, *Merrem.* Sheltopusik, *Latreille.*
Head shielded; body furnished with a distinct lateral line; fore feet wanting; hind feet short, two or three lobed; tongue two-forked; teeth blunt only in the jaws.

P. serpentinus, *Merren.* Lacerta apus, *Pallas.* Chamæsaura apus, *Schneid.* Bipes sheltopusik, *Bonnat.* Sheltopusik didactylus, *Lat.* Seps. sheltopusik, *Daud.* Russia.

7. OPHIOSAURUS, *Daud.* Hyalinus, *Merrem.*
Head shielded; body with a distinct lateral line; feet none, (hid under the skin); drum of the ear apparent; teeth in the jaws and palate.
O. ventralis, *Daud.* Anguis, *Lin.* Chamæsaura, *Schneid.*

8. ANGUIS, *Lin. Cuv.*
Head shielded; body without any lateral line; feet none (hid under the skin); drum of the ears covered with the skin; teeth only in the jaws.
A. fragilis, *Lin.*

9. ACONTIAS, *Cuv.* Eryx, *Daud.*
Head shielded; the anterior shield projecting over the mouth, lateral line not distinct; feet none, nor no bones (hid under the skin); drum of the ears covered with the skin; teeth in the jaws and palate, allied to the next family.
*Eyes distinct. A. Meleagris, *Merrem.* Anguis, *Lin.* Eryx Meleagris, *Daud.* **Eyes hid with the skin. A. cæcus, *Cuv.*

§ 2. *Body covered with intricate scales; anus terminal.*
Fam. III. TYPHLOPIDÆ, *Gray.*
Body cylindrical, covered with imbricate scales; feet or legs none; head shielded, muzzle advanced; tongue long, forked extensile; anus terminal; drum of the ear hid under the skin.

1. TYPHLOPS, *Schneider.*
Eyes visible under the skin.
Dr. Waggler has published a genus under the name of *Stenostoma* which does not appear to differ from this or *Acontias.*

§ 3. *Body covered with rings of square scales.*
Fam. IV. AMPHISBÆNIDÆ, *Gray.*
Body cylindrical, covered with rings of square scales; feet or legs none; head and sometimes the chest shielded; tongue short, cut; teeth conical only in the jaws; anus terminal; drum of the ear hid under the skin.

1. AMPHISBÆNA, *Lin.*
Body covered with rings of uniform sized square scales; head shielded; anus with a series of pores in front.
A. alba, *Lin.*

2. Leptostermon, *Wagler.*

Head and chest shielded; body covered with rings of square scales; anus without any pores.

L. microcephalus, *Wagler* 10, t. 26, f. 2.

Fam. V. Chalcididæ.

Body cylindrical, covered with rings of uniform square scales; legs two or four; head shielded; tongue ....; teeth ....; anus transverse submedial; drum of the ear hidden.

1. Chirotes, *Cuv.* Bipes, *Latr.* Bimanus, *Oppel.*

Legs two, posterior; toes five, clawed.

C. canaliculatus, *Merrem.* La Cannell, *Lacep.* Lacerta lumbricondes, *Shaw.*

2. Chalcides, *Daud.* Chalcis, *Merrem.*

Legs four; toes three, clawed.

C. flavescens, *Bonnat.* Le chalcide, *Lacep.* Chamæsaura Cophias, *Schneid.* Chalcis Cophias, *Merrem.*

3. Cophias, *Gray.* Colobus, *Merrem,* not *Illiger.*

Legs four; toe one, clawed.

C. Daudini, *Gray.* Colobus Daudini, *Merrem.* Chalcides Monidactylus, *Daud.*

Order IV. Ophidii, *Brogniart.* Serpentes, *Lin.*

The drum of the ear wanting; eyes destitute of the third lid; skin covered with imbricate scale or plates; feet none; chest and blade bones wanting; ribs encircling the body; body of the vertebra uniting by a convex and a concave surface; the os tympanum or pedicel of the lower jaw moveable, and suspended to another similar bone or mastoide, attached to the skull only by ligaments. The branches of the jaw only united together by ligaments, so as to let them separate more or less from each other, and allow the animal to swallow large bodies; the palatine arches movable, armed with sharp recurved teeth.

§ 1. *Upper jaws with fangs only.* Venati.

The jaws are very dilatible; the tongue very extensile; head large behind; the upper maxillary bones small, supported on a long pedical, and very mobile, furnished with a fang, pieced with a little canal, which give passage to the liquor secreted by a considerable gland under the eye. The fang, when the animal is not irritated, is hid in a plait of the palatine integuments; viviparous.

Fam. I. Crotalidæ.

Body and tail covered beneath with simple transverse plates; head usually scaly. America.

** With a Rattle.*

1. Crotalus, *Lin.*

Head covered with scales; the muzzle perforated with a small fovea behind each nostril; tail furnished with a rattling appendix formed by the dry terminal scales. *America.*

C. horidus, *Lin.*

2. Crotalophorus, *Lin. Gray.*

Head covered with plates; muzzle with a small fovea behind each nostril; tail furnished with a rattling appendix. *America.*

C. miliaris, *Lin.*

*** Without any Rattles.*

3. Echis, *Merren.* Scytales, *Latr.* not *Gronov.*

Head covered with scales; the muzzle not perforated; tail simple. Allied to Viperadæ, *Merrem.*

S. zic zac, *Daud.* Boa horrata, *Schneider.*

4. Acanthopis, *Daud.* Ophyas, *Merrem.*

Head with large scales in front; no pores behind the nostrils; tail with double plate only beneath the end, which terminate in a very acute point.

A. cerastinus, *Daud.* Boa palpebrosa, *Shaw.* Ophryas Acantophis, *Merrem.*

5. Langaha, *Brug.* Langaya, *Shaw.*

Head covered with large plates; muzzle long, pointed; tail surrounded by ring-like plates, except at the end which is scaly.

L. nasuta, *Shaw.* L. madagascariensis, *Merrem.* Is this genus allied to *Dryinus?*

Fam. II. Viperidæ.

The body scaly; the abdomen covered with annulated plates; the tail with divided scale beneath; anus without spurs.

Head distinct, scaly, behind broad. Viperina.

1. Trigonocephalus, *Oppel.* Lachensis, *Daud.* Cophias *Merrem.*

Head triangular with a distinct fovea behind the nostrils; tail round; apex simple, conical, sometimes armed.

T. atrox, *Merrem.*

2. Crasedocephalus, *Kuhl.* Bothrops, *Wagler.*

Head truncated, with a distinct fovea behind each nostril; tail round, the plates towards the anus entire, apex simple, conical, plates halved.

C. crotalinus, *Kuhl.* Crotalus Mutus, *Lin.*

3. Cobra, *Laur.* Vipera, *Laur.* Echidna, *Merrem,* not *Geoff.*

Head covered with scales without any fovea behind the nostrils; tail round.

V. Cerastes, *Laur.* Coluber Cerastes, *Lin.*

5. Pelias, *Merrem.* Coluber, *Laur.*
Head scaly, with three larger plates, without any fovea behind the nostrils; tail round.
P. Berus, *Merrem.* Coluber Berus, *Lin.*

***Head broad behind, with plates.* Naiina.

5. Naia, *Laur.*
Head, with nine plates behind, broad; neck very expansile, covering the head like a hood; tail round.
N. tripudians, *Merrem.* Coluber Naja, *Lin.*

****Head indistinct, with plates; mouth small.* Elaphina.

6. Sepedon, *Merrem.*
Head with nine plates, without any fovea behind the nostrils; tail round.
S. Hæmachates, *Merrem.* Hæmachate, *Lacep.*

7. Elaps, *Schneid.*
Head rarely distinct from the body with plates, without any fovea behind the nostrils; tail round.
E. Lenniscatus, *Schneid.* Coluber henniscatus.
The fangs of this genus are said not to be perforated; it is, therefore, closely allied to *Coluberidæ,* and the tribe should be removed to the latter family; I have at present considered it as the passage between the two sections of *Ophidii.*

8. Micrurus, *Wagler.*
Head indistinct with nine plates, without any fovea behind the nostrils; tail very short, acute; subcaudal plates one and two rowed.
M. spixii, *Wagler.*

9. Platurus, *Latr.*
Head with plates; tail compressed, broad two edged, allied to *Hydridæ.*
P. fasciatus, *Latr.* Coluber laticaudatus, *Lin.* Laticauda scutata, *Laur.* Hydrus colubrinus, *Schneid.*

§ 11. *Upper jaw with teeth, and with or without fangs; oviparous.*

Fam. II. Hydridæ.
Nostrils on the top of the head, operculated; teeth and usually fangs; body covered above with scales, and beneath with scales or narrow plates.

**Tail compressed. Living in water.*

1. Aipysurus, *Lacep.*
Head shielded; belly with a row of small shields; tail beneath scaly; neck dilatible.
*A. lævis, *Lacep.* Enhydris lævis, *Merrem.*

2. Enhydris, *Merrem.*
Head shielded; belly with a row of small shields; tail beneath scaly; body keeled; neck simple.
E. spiralis, *Merrem.* Hydrus spiralis, *Shaw.*

3. Disteria, *Lacep.*
Head shielded; belly with a row of shield apparently formed of two rows of scales soldered together; tail beneath scaly; neck simple.
D. doliata, *Lacep.* N.H. Capt. P. P. King.

4. Hydrophis, *Daud.* Leioselasma, *Lacep.*
Head shielded; belly and tail, beneath shielded.
*H. nigro anctus, *Daud.* *Russel, Ind. Serp.* t. 6. **Leioselasma, striata, *Lacep.* *Ann. Mus.* iv. *** ? H. spiralis, *nob.* H. spiralis, *Shaw.*

5. Pelamis, *Daud.* Hydrophis, *Latr.* and *Daud.*
Head shielded; body and tail entirely scaly.
P. bicolor, *Daud.* Anguis platura, *Lin.*

6. Chersydreas, *Cuv.* Acrocordus, *Shaw.*
Head and body entirely covered with small scales; tail compressed.
C. granulatus, *Merrem.* Hydrus, *Schneider.* Pelamis, *Daud.*

***Fangs none; tail round.*

7. Acrocordus, *Hornstedt.*
Head and body entirely covered with small scale; tail round; fangs none.
A. Javanicus, *Hornstedt.*

Fam. V. Colubridæ.
Jaws furnished with teeth, and sometimes fangs; head covered with plates; abdomen covered with broad ring-like plates; tail with two, and sometimes only one series of plates beneath; anus destitute of spurs.

**Mouth with fangs.*

1. Trimeresurus, *Lacepede.*
Head narrow, shielded; body with broad smooth scales on the sides, and narrow keeled scales on the back; tail with whole and divided scales.
T. leptocephalus, *Lacepede, Ann. Mus.* N. H.

2. Bungarus, *Daudin.*
Head blunt with nine plates; body scaly with the dorsal scales larger than the rest. Subcaudal scale one rowed, entire.
B. cæruleus, *Daud.* Boa lineata, *Shaw.*

3. Ophis, *Wagler.*

Head with small imperforate teeth placed before but not behind the fangs; abdominal plates broad, the subcaudal plates two rowed.

O. Merremi, *Wagler.*

***Mouth without fangs; without any fovea before the eyes.*

4. Coluber, *Lin.* Natrix, *Laur.* Coronella, *Laur.*

Head with eight or nine plates; nostril simple, solid, convex; mouth large, bent down at the angles; tail beneath with all the lower divided; scales of the back equal.

*C. albus, *Lin.* C. brachyurus, *Shaw.* **Coronella, *Laur.* C. cervina, *Laur.* Coluber stolatus, *Lin.* ***Homalopsis, *Kuhl.* H. monilis, Coluber, *Lin.*

5. Dipsas, *Laur.* not *Leach.* Bungarus, *Oppel.* not *Daud.*

Head large, oblong, with eight or nine plates; rostral scale, simple, solid, convex; mouth large, angle bent down; shield beneath the tail all divided; scales of the centre of the back hexangular, larger than those of the sides.

*D. indica, *Laur.* Coluber bucephalus, *Shaw.* *Bungarus, Catesbeii, Coluber, Catesbeii, *Merrem.*

6. Ahætulla, *Gray.*

Head distinct, oblong, with nine plates, before rounded very blunt, depressed; rostral plate single, convex, with a concave arch on the labial margin; mouth large, angles recurved; subcaudal shields two rows; scales of the sides linear, adpressed, those on the centre of the back, forming the dorsal series larger, triangular; body long, slender.

A. decorus, *nob.* Coluber decorus, *Shaw.* A. cærulescens. C. cærulescens, *Lin.* A. Sagitalis, Coluber Sagitalis, *E. W. Gray's MSS.* C. sagittatus, *Shaw.* A. punctulatus, *Gray,* N. Holland. *Capt. P. P. King.*

7. Macrosoma, *Leach,* without character.

Head long with nine plates; rostral plate single, convex, with a concave excavation on the labial margin; mouth large, angle bent down; shield beneath the tail all two rowed; scales of the back uniform; body long, slender.

M. elegans, *Leach. Bowdich Ashantee,* Coluber elegans, *Shaw.*

8. Passerita, *Gray.* Dryinus, *Merrem,* not *Fabr.* Natrix, *Laur.*

Head with nine plates; snout moveable, acute, with two scales in front, one before the other; plates under the tail rowed; fangs distinct; body very thin; scales like the genus *Ahætulla;* tail very long.

D. mysterizans, *Merrem.* Coluber mysterizans, *Lin.*

9. Hurria, *Daud.*

Head with nine plates; scale of the body uniform; plate under the tail entire and divided.

**Head narrow, indistinct.* H. lineata, *Daud.* Hurriah, *Russel.* ***Head very broad.* Ibiba, *Gray.* I. irregularis. Hurria pseudoboiga, *Daud.*

10. Scytale, *Gronovius.*

Head with nine plates; scale of the body uniform; plates under the tail all entire.

*Head distinct, blunt. S. coronata, *Merrem.* **Head indistinct. S. anguiformis, *Merrem.* Anguis scutatus, *Laur.*

11. Erpeton, *Lacep.* Rhinopirus, *Merrem.*

Head with large plates, with two soft scaly appendices at the end of the nose; abdomen largely shielded; tail above and below scaly.

E. tentaculatus, *Lacep.* Rhinopirus. Erpeton, *Merrem.*

Fam. V. Boidæ.

Jaws furnished with teeth, and sometimes fangs; head scaly, or with a few plates in front; abdomen and tail covered beneath with narrow short plates; anus furnished with spurs.

**Head distinct.* Boina.

1. Boa, *Lin.* Cenchris, *Lin.* Constrictor, *Laur.*

Head distinct, scaly; mouth and tail above scaly, below broadly shielded; tail long, round, tapering.

B. Constrictor, *Lin.*

2. Cenchris, *Lin.* Boa, *Laur.* Xyphosoma, *Wagler.*

Head distinct; shielded over the nose; trunk and tail above scaly, below broadly shielded; tail round, tapering; body compressed, subfusiform.

C. murina, Boa Cenchria, *Lin.* Boa Cenchris, *Gmelin.*

3. Python, *Daud.*

Head distinct, scaly, or subshielded over the nose; trunk above scaly, beneath broadly shielded; tail round, beneath with divided, and sometimes a few entire plates.

P. tigris, *Daud.* Coluber Nepa, *Laur.* C. boæformis, *Shaw.*

Obs. Some of the species of this genus are somewhat allied to Hydridæ.

***Head indistinct; body cylindrical; mouth small.* Totricina.

4. Torquatrix, *Haw.* Tortrix, *Oppel.* not *Lin.*

Head not distinct; from the trunk mouth small; body above scaly, below covered with small hexangular shields; tail blunt, round, scales simple and divided; mouth small; tongue short, cut.

T. Scytale, *Gray.* Anguis Scytale, *Lin.* A. Corallina, *Laur.*

5. Eryx, *Daud.* Erix, *Cuv.*
Head distinct from the trunk; body covered above with hexagonal scales, below with small narrow subquadrate shields; tail short, blunt, with one row of scales beneath.
E. turicus, *Daud.*

6. Clothonia, *Daud.*
Head distinct from the trunk; body covered above with hexagonal scales, below with small narrow subquadrangular shields; tail short, blunt, with simple and double shields.
C. anguiformis, *Daud.*

Order V. Chelonii, *Latreille.* Cheloniens, *Brogniart.* Testudinata, *Oppell.*

Body short, inclosed between two horizontal shields, with the head, neck, tail, and four legs, passing out between; mouth toothless, often covered with a horny bill; tongue short.

The upper shield, or *Carapace*, is formed by the ribs (eight pair) enlarged and united together, and to the annular part of the dorsal vertebra, by toothed sutures, so as to be immovable; the lower shield, or *plastron*, is formed of the pieces which represent the chest bone (usually nine), and a circle of bones analogous to the sternal cartilages of quadrupeds. The vertebra of the neck and tail alone are movable. The two bony envelopes are immediately covered with the skin or scales, and surround the muscles of the extremities.

§ 1. *Feet and head retractile into the carapace; carapace solid, covered with horny scales.* Cryptopodi.

Fam. I. Testudinidæ.

Body covered with horny shields; carapace convex solid; sternum, attached by the greater part of its sides to the carapace; legs horny; feet club shaped; toes indistinct, bluntly clawed; dorsal plates, 13; sternal, 12. Terrestrial.

Testudo, *Dumaril.* Chersini, *Merrem.*
T. græca, *Lin.*

Fam. II. Emydidæ, *Bell MSS.*

Body covered with horny shields; carapace depressed; sternum attached to the carapace by a small surface; lips horny or soft; feet digitate; fingers distinct; claws sharp; fluviatile or lacustral.

**Beak horny; sternum entire.* Emydina.

1. Emys, *Brogn.*

Toes 5-4, or 4-4; depressed elongated, palmated; sternum immovable.

*Sternum very narrow. Rapara. R. serpentina, *Gray.*

Testudo, *Lin.* **Sternum 11 or 12 scaled, broad. E. centrata, *Merren.* T. concentrica, *Shaw.* ***Toes 4-4; sternum 13 scaled. E. longicolis, *Gray.* Testudo, *Shaw.*

The plastron of the last subgenus is covered with 13 scales; that is six pair marginal, and an unequal sided hexangular, one in the middle of the anterior lobe. I have only observed an approximating distribution of the plates in a species of *strenotherus;* all the other *Emyda* that I have seen have had only the six pair of marginal plates, the first pair sometimes soldered so as to form only 11 plates.

Beak horny; sternum transversely sutured. Terraphenina.

2. Terraphene, *Merrem.* Cistula, *Say.* Tortuis a boit, *Cuvier.*

Body convex; sternum of 11 or 12 plates, moveable; the two central plates united to the carapace by ligament; the posterior lobe broad fixed, the anterior one, of five or six plates, separated by a transverse ligamentous hinge.

T. clausa, *Merren.* Testudo, *Gmelin.*

This genus forms the pass between the Emydæ and the Testudinidæ, for it has the convex form and solid shell of the latter, and the feet and general characters of the former. It is also intermediate in point of habits, for it is often found in hot dry places.

Mr. Bell observes, that Testudo Europea is a species of this genus; if so the name of it should be changed, as that was certainly the *Emys* of the ancients.

3. Sternotherus, *Bell, MSS.* Tortues a boit ***Cuv.*

Body depressed; sternum of 11 or 12 plates; the central part of two plates united to the carapace by two long processes fixed; the anterior lobe moveable, separated by a transverse ligamentous hinge; the posterior lobe narrow, fixed.

S. odorata, *Gray.* Testudo, *Latr.* S. pensylvanica, Testudo, *Gmelin.*

Obs. Cuvier describes the anterior and posterior lobes of the sternum of these species to be moveable; but the hinder was fixed on the specimens which I have examined, which were all dry.

4. Kinosternom, *Spix.*

Body depressed; sternum central part fixed; anterior and posterior lobes moveable; throat bearded.

K. longicaudatum.

**Beak soft.* Chelidina.

5. Chelys, *Dumeril.* Matamata, *Merrem.*

Claws 5-4; body depressed; lips soft; nose produced.

C. fimbriata. Testudo matamata, *Brug.*

This genus is allied by its soft lips to the next family.

§ 11. *Feet and head not or only partly retractile into the carapace; carapace mostly soft.* Gymnopodi.

Fam. III. Trionicidæ.

Body covered with a coriaceous skin; lips fleshy; feet digitate palmate; five toed, three clawed. *Fluviatile.*

1. Trionix, *Geoff.*

Nose produced.

T. ferox, *Geoff.* Testudo ferox, *Pennant.*

Fam. IV. Sphargidæ.

Body covered with a coriaceous skin; lips horny; feet fin-shaped. *Marine.*

1. Sphargis, *Merrem*

S. mercurialis, *Merrem.* Testudo coriacea, *Lin.* Luth, *Daubenton.*

Fam. V. Cheloniadæ.

Body covered with horny shields; lips horny; feet fin-shaped. *Marine.*

1. Chelonia, *Brogn.* Caretta, *Merrem.*

C. Mydas. Testudo Mydas, *Lin.* Caretta cephalo, *Merrem.*

A Table of the Affinity of the Orders of Reptiles.

Normal Groups.	*Annectant Groups.*
Order I.—*Sauri.*	
1. Stellionidæ.	3. Lacertinidæ.
2. Geckotidæ.	4. Chamælionidæ.
	5. Tupinambidæ.
Order II.—*Emydosauri.*	
1. Crocodilidæ.	3. Plesiiosauridæ.
2. ————?	4. Icthiosauridæ
	5. ————?
Order III.—*Chelonii.*	
1. Testudinidæ.	3. Trionicidæ.
2. Emydidæ.	4. Sphargidæ.
	5. Carettidæ.
Order IV.—*Ophidii.*	
1. Crotalidæ.	3. Hydridæ.
2. Viperidæ.	4. Colubridæ.
	5. Boidæ.

Normal Groups.	*Annectant Groups.*
	Order V.—*Saurophidii.*
1. Sincidæ.	3. Typhlopsidæ.
2. Anguidæ.	4. Amphisbænidæ.
	5. Chalcidæ.

The last family agrees with some of the Sauri, in having four legs and plates.

The first of these columns represents the natural groups which have the characters of the order in the most perfect state, and consequently are not directly allied to the other order, except through the medium of the annectant families, which are the first (No. 3) and last (No. 5) of the right hand column which are themselves united together by the central (No. 4) family of each group.

The two fossil families may be the type of *Emydosauri*, but the group is so imperfectly known at present, that it is impossible to determine it.

Class IV.—Amphibia.

Body with a soft naked skin; heart with one auricule and one ventricule; respiring by lungs and gill, and often by lungs only when perfect; claws none; head articulation to the vertebra by two condyles. Blood cold; windpipe membranaceous; ribs none, or very short and imperfect; egg skin membranaceous. Animal often changes its form and habit during growth; egg fecundated after they are deposited, hatched in the water where they are laid. They do not only differ from the perfect animal by having gills, but they often change their external and internal conformation, and generally gain legs.

This class contains so few genera that it is scarcely necessary to divide it into orders. I shall, therefore, for the present merely divide it into families, which may be considered as either,

§ 1. *Undergoing transformation; gills deciduous; eyelids three distinct; spiracule none.* Mutabilia, *Gray.* The larva elongated, respiring by deciduous gills.

Order 1. Anoura, *Dumeril.* Salientia, *Laur.* Batrachein, *Blainv.*

Fam. I. Ranadæ.

Body short, thick; feet four, long; tail none; drum of the ear apparent; sternum and clavicles distinct. Larva elongate tailed, apodous; gills turfted on four cartilaginous support, covered by the skin, pierced with one or two lateral spiracules.

†*Skin shining.*

**Hylina*

Hyla, *Laur.* Calamita, *Schneid.*

Body slender; skin mostly smooth; toes all dilated at the end, the fourth one of the hind feet, of a m derate length.

*H. tinctoria; *Laur.* Rana tinctoria, *Shaw.* **C. intermixtus, *Merrem.* ***Calamita, hind feet semipalmate. H. arboreus, *Schneid.* Rana arboreus, *Lin.* ****Boana, *Gray.* Granulated feet palmate, β maxima. Rana Boans, *Lin.*

***Ranina.*

Rana, *Lin. Laur.* Ranaria, *Raff.*

Body subventricose; skin smooth; back angular; paratoid glands none externally; toes attenuated, hind ones palmate, the fourth of the hind foot very long; teeth in the jaws and palate.

R. temporaria, *Lin.*

Megophrys, *Kuhl.*

Body ventricose; skin smooth; back convex; toes attenuated, the hinder ones semipalmate; head angular, with a conical horn over each eye. The Old Continent.

M. Kuhlii, *nob.* Java?

Ceratophrys, *Desm.*

Body ventricose; skin rough; back convex; toes attenuated, the hinder ones semipalmate, nearly equal; head angular, with a conical horn over each eye. America.

C. Sebæ, *nob.* Rana cornuta, *Lin.*

††*Skin dull, warty.*

****Bombinatorina.*

Breviceps, *Merrem.*

Body ventricose; back convex; skin warty; no external paratoids; toes attenuated; the head blunt, confluent; mouth small, not extending beyond the front angle of the eye; teeth in the jaws.

B. gibbosus, *Merrem.* Rana gibbosa, *Lin.*

Bombinator, *Merrem.*

Body ovate; back convex; skin warty; no external paratoids; toes attenuated, the fourth of the hind foot longest; head rounded, confluent; mouth large, extended to the back of the eyes; teeth none.

B. ventricosa. Rana ventricosa, *Lin.*

*****Piprina.*

Pipra, *Laur.*

Body ovate, depressed; back flat; skin warty; no external paratoids; toes attenuated; head triangular, confounded with the body; mouth large; the young are hatched on the back of their mother.

P. Tedo, *Merrem.* Rana pipa, *Lin.*

******Bufonina.*

Bufo, *Laur.*

Body ovate; back convex; skin warty; paratoids porous, distinct; toes attenuated; head rounded, confounded with the body; mouth toothless.

*B. vulgaris, *Laur.* Rana Bufo, *Lin.* O. nasutus, *Spix.*
***Head beaked.* Oxyrhychus, *Wagler.*

My late friend Dr. Kuhl has noticed another genus of this family under the name of *Occidogyna,* but he only observes that the body is regularly oval, and that the hind legs are peculiar and intermediate between the frogs and toads.

Order 2. Urodela, *Dum.* Caudata, *Oppell.* Pseudosaurii, *Blainv.*

Fam. II. Salamandridæ.

Body subcylindrical, long; feet four, short; tail distinct; sternum and clavicles none. Larva with four feet; branchia turfted, three on each side exposed, supported by cartilaginous rings, covered by a membranaceous operculum.

1. Salamandra, *Laur.*

Tail round; paratoids porous; toes 4-5.

S. maculosa, *Laur.* Lacerta salamandra, *Lin.*

2. Triton, *Laur.* not *De Montf.* Triturus, *Rafinesque.* Molge, *Merrem.*

Tail compressed; paratoids none; toes 4-5.

The axolotle appears to be the larva of an animal of this genus, although Sir E. Home has discovered that it contains eggs, for, according to Baron Cuvier, they are to be found in the tadpoles. The Sirex opercularis of *Beauvois* (Phil. Trans. Philad.) and the Proteus New Cæsariensis of *Green* (Jour. Acad. Nat. Sci. Phil.) appear also to be larvæ. The *Trois doigts* of Lacepede is said to be a true lizard. Latreille formed the genus *Ichytheosaurus* of the larvæ of this genus.

§ 11. *Not undergoing any transformation; gill none or permanent; eyelids two; spiracules distinct.* Amphipneusta.

Order 3.—Sirenes, *Lin.*

Fam. III. Sirenidæ.

Branchia persistent. Skull formed of several distinct bones; body compressed; legs two or four.

**Gill flaps distinct.* Proteina.

Hypochthon, *Merrem.* Proteus *Laur.* not *Muller.*

Legs four; toes 3-2; branchia three on each side, fringed; body subdepressed; tail compressed, finned; muzzle depressed, long; jaws with teeth.

H. Laurentii, *Merrem.* Proteus anguinus, *Laur. rept.* 37, t. 4, f. 3. Cuvier, Humboldt, Obs. Zool. i. 119. *Rusconi, Anat.*

Laurentii included in his genus the Axolotle, and the larva of a species of Triton.

MENOBRANCHUS, *Harlan.* Necturus, *Raffinesque.*

Legs four; toes 4-4; branchia three on each side; body sub-depressed; tail compressed; muzzle truncated, depressed; two rows of teeth in the upper and one in the lower jaw.

M. Sayii, *nob.* Above brown, with irregular black spots, and a black band arising from the nostrils passing through the eye, and dilated on the sides, becoming obsolete at the tail.

Triton lateralis, *Say, James, Travels,* i. 303; and Anatomy, *Jour. of Nat. Sci. Phil.* iii; Siren, *Barton;* Proteus, *Mitchell, Silliman's Journal,* iv.; Siren lacertina, *Schneid. H. Amph.* i. 48. *Le Comte,* l. c. 57; Menobranchus lateralis, *Harlan Acad. N. S. Phil.* iv.; Necturus maculatus, *Raffinesque, Ann. Nat.*? Icon, *Acad. Nat. Sci. Phil.* iv. t. xxi.

Inhabit. Ohio, North America.

Mr. Say, in his description of this animal, pointed out the necessity of, and the character by, which this animal should be distinguished from Triton and Proteus.

M. tetradactylus. Two rows of teeth in each jaw, a duplicature of skin forming a collar just before the gills.

Le tetradactyle, *Lacepede, Ann. Mus.* x. M. tetradactylus, *Harlan,* l. c.

This animal is considered by Mr. Say to be a larva.

** *Operculum none.* Serenina.

SIREN, *Lin.*

Legs two, anterior; toes five; branchiæ three on each side, tripinnatifid; operculum none; spiracules three; body long, subcylindrical; tail compressed; head rounded; teeth in the jaws and palate?

S. lacertina, *Lin.* Muræna Siren, *Gmelin, S. N.* i. 1136; Mud. Iguana, *Ellis, Phil. Trans.* t. vi. 189. Siren, *Pennant, Arctic Zool.* ii. 335.? Siren, *Camper. in Berl. Naturf.* viii. 482. *Cuv. Humb. Obs. Zool.* i. 98. Anat.

PSEUDOBRANCHUS.

"Legs two, anterior; toes three; body subcylindrical; tail compressed; spiracules three, furnished with a fleshy trilobate covering (branchia), the lobes entire and naked; teeth none."

P. striata, Siren striata, *Le Conte, Ann. Lyceum Nat. Hist. New York,* i. 54, t. 4.

Mr. Le Comte has the idea that neither the Siren nor this animal breathe by the lateral appendages usually called gills, which he thence considers as the covers of the spiracules.

Fam. IV. AMPHIUMIDÆ.

Branchia none; skull formed of a solid bony substance; gill flaps open during life; body subcylindrical; tail compressed; legs four.

1. ABRANCHUS, *Harlan.* Protonopsis, *Barton?*

Legs four, strong; toes 4-5; the outer edge of the feet fringed; the outer toes of the hind feet palmated.

A. alleganensis, *Harlan Journal Nat. Sci. Phil.* iv. Salamandra gigantea, *Barton's Account of Siren Lacertina*, p. 28. S. alleganensis, *Latr. Rept.* ii. 253. Molge gigantea, *Merrem*, 187, not Larva. Hell bender, *Ohio.*

Inhab. Lakes of North America.

2. AMPHIUMA, *Garden.* Chrysodonta, *Mitchel?*

Legs four, boneless; toes 2-2, outer longest; body subcylindrical; tail end compressed; teeth one row in each jaw, and two in the palate.

Amphiuma Means, *Garden.* Letter to Ellis, *in the Correspondence of Linnæus*, i. 399, to Linnæus, l. c. i. 333. Sireni similis, *Linnæus's Letter to Garden.* Siren Lacertina, *Garden, Amer. Acad.—R. Harlan, Jour. Acad. N. S. Phil.* vii. 54. (Anatomy.) *Phil. Mag.* 1824. Chrysodonta larvæ formis, *N. Y. Medical Reporter*, i. 529. Inhab. North America.

Order 4. Apoda, *Merrem.* Pseudophidii, *Blain.*

Fam. V. CÆCILIADÆ.

Branchia none; head depressed, formed of a solid bony substance; teeth in the jaws and palate; legs none; body cylindrical; tail short, blunt; anus round, nearly terminal.

1. CÆCILIA, *Lin.*

C. tentaculata, *Lin.* Ibiare, *Lacepede.*

So very little is known of this curious class of animals, that it is impossible to say any thing with respect to the connexion which exists between the families or orders; but that such an affinity does exist must be obvious to every one who considers the difficulty of distinguishing them. I have attempted to bring together all the species that have been described of the Sirenidæ and Amphisumidæ, as Merrem (the last work published on the species of Amphibia), describes only two of these animals. It is to be hoped that Mr. Say and Dr. Harlan will continue their researches, that have so much illustrated a group, which has particularly attracted the attention of Ellis, Garden, Linnæus, Cuvier, Schreiber, Rusconi, and Sir Everard Home.

Article VIII.

Influence of the Moon on Animal and Vegetable Economy. By Mr. N. Mill.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Addington-square, Camberwell, Aug.* 3, 1825.

The subject of the moon's influence has engaged but very little of the attention of the philosophical world, and with the exception of the theory of the tides, has been scarcely noticed. Its influence in promoting and accelerating animal decomposition is known only to a certain class of persons, not the most renowned indeed for studying the doctrine of cause and effect, or extending philosophical knowledge; (namely), persons in the Navy and Company's service; but who, nevertheless, are sufficiently alive to interest. It is a fact well established and authenticated by numbers of these gentlemen, who have experienced heavy losses thereby, that if an animal fresh killed, be exposed to the full effulgence of the moon at certain seasons, and in certain places, a very few hours only will be sufficient to render the animal so exposed a mass of corruption; whilst another animal, not exposed to such influence, and only a few feet distant, will not be in the slightest manner affected. It would be impossible in the present imperfect state of our knowledge of this luminary and its influence, to draw any just conclusions from so few facts as have been collected upon this subject; but it will be most desirable to accumulate them as much as possible in order to deduce some accurate reasoning from them; I therefore subjoin some facts which have come to my knowledge of the highest practical importance to this maritime nation; and the disclosure of which, I trust, will open a field for investigation that has hitherto been uncultivated and neglected. The influence of the moon on vegetation has not altogether been unobserved, because fruit when exposed to moonshine has been known to ripen much more readily than that which has not; and plants shut out from the sun's rays and from light, and consequently bleached, have been observed to assume their natural appearance if exposed to the effulgence of the moon. These are also facts fully established, but from which no rational theory has been drawn. A very intelligent gentleman, named Edmonstone, who was for nearly 30 years engaged in cutting timber in Demerara, and who had made a number of observations on trees during that period, has done me the favour to give me explicit answers to a series of queries which I presented for his inspection; and which, I doubt not, will be appreciated as they merit. I shall present them in detail with the answer to each.

Question.—1st. Influence of the moon on vegetation?

Answer.—I have paid but little attention to the moon's influence on any thing exposed to it but trees; the moon's influence, however, on these is very great. So observable is this, that if a tree should be cut down at full moon, it would immediately split, as if torn asunder by the influence of a great external force applied to it. This separation of its parts takes place, I presume, owing to the immense quantity of juice which is contained in the body of the tree. In consequence of this, trees cut at full moon are of comparatively little use; in a very short time after being cut down, they are attacked by a moth somewhat similar to what is often found in American flour. Trees cut down at this season are likewise attacked much earlier by the rot, than if allowed to remain to another period of the moon's age.

Question.—2nd. The nature of the trees?

Answer.—It is impossible to give in this small space a statement of the different trees to be met with in the West India Islands and our Colonies in South America. They are as different as those are which we have in our own country, indeed by far more numerous.

Question.—3rd. If evergreens?

Answer.—All the trees in those countries may be stiled evergreens, as there is a constant succession of leaves upon them all.

Question.—4th. Their names?

Answer.—With the scientific names of the various trees to be found in our colonies in the West Indies and South America, I am unable to supply you. The names given to the most of them are Indian names applied to them by the natives.

Question.—5th. If usually cut at particular or in all seasons?

Answer.—The trees intended to be applied to durable purposes, are cut only during the first and last quarters of the moon, for the reasons mentioned in the answer to question 1.

Question.—6th. If the sap rises during the absence of the moon, or during its effulgence?

Answer.—The sap rises to the top of the tree at full moon, and falls in proportion to the moon's decrease.

Question.—7th. Whether common to all trees or only to certain species?

Answer.—The influence of the moon over the rising and falling of the juice of trees is common to all the species of trees with which I am acquainted. I had occasion to observe these effects in the experience of 30 years amongst the various kinds of wood with which the colonies of South America abound.

From this statement, it appears obvious, that trees cut at the *fall of the moon* will *split* as if torn asunder by great external force; that they are more liable to the *attacks of worms;* that they are attacked *much earlier by the rot; and that the sap rises to the top of the tree at full moon, and falls in proportion to the moon's decrease; and this effect is common to all species of trees* with which this gentleman was acquainted.

It will be perceived that these observations are confined to the continent of South America and islands adjoining; but if the moon has a correspondent influence in other countries, (which there is no reason to doubt) and this gentleman's observations be correct, the practical importance of them in felling timber deserves the utmost thanks from those persons who are in any way interested in practices of this kind, as well as from society at large.

Article IX.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1824. *Part II.*

(*Concluded from* p. 65.)

XVI. *A Comparison of Barometrical Measurement with the Trigonometrical Determination of a Height at Spitzbergen.* By Capt. E. Sabine, FRS.

An account of the results of this comparison will be found in the *Annals* for May, 1824, p. 385.

XVII. *Experimental Inquiries relative to the Distribution and Changes of the Magnetic Intensity in Ships of War.* By George Harvey, Esq.: communicated by John Barrow, Esq. FRS.

We should not be able to give our readers an adequate idea of the results of these inquiries, occupying above forty pages of the Transactions, in the confined space we could devote to the subject, and must, therefore, refer them to the paper itself. This we must also do, and for the same reason, with respect to another valuable paper by Mr. Harvey, mentioned below, but of which a short notice has already appeared in the *Annals.*

XVIII. *Experiments on the Elasticity and Strength of Hard and Soft Steel.* In a Letter to Thomas Young, MD. For. Sec. RS. By Mr. Thomas Tredgold, Civil Engineer.

"If a piece of very hard steel be softened," Mr. Tredgold states, addressing Dr. Young, "it is natural to suppose that the operation will produce a corresponding change in the elastic power, and that the same load would produce a greater flexure in the soft state than in the hard one, when all other circumstances were the same. Mr. Coulomb inferred from some comparative experiments on small specimens, that the state of temper does not alter the elastic force of steel; and your Experiments on Vibration led to the same conclusion (Nat. Philos. ii. 403). But the subject appeared to require further investigation, and particularly because it afforded an opportunity of ascertaining some other facts respecting steel, which had not been before examined.

"In making the experiments which I am about to describe, each bar was supported at its ends by two blocks of cast iron. These blocks rested upon a strong wooden frame. The scale to contain the weights was suspended from the middle of the length of the bar, by a cylindrical steel pin of about $\frac{3}{8}$ths of an inch in diameter. And as in experiments of this kind it is desirable to have the means of raising the weight from the bar, without altering its position, in order to know when the load is sufficient to produce a permanent change of structure, I have a powerful screw with a fine thread fixed over the centre of the apparatus, by which the scale can be raised or lowered, when the cords on which the screw acts are looped on to the cross pin by which the scale is suspended.

"To measure the flexure, a quadrantal piece of mahogany is fixed to the wooden frame; two guides are fixed on one edge of the mahogany, in which a vertical bar slides, and gives motion to an index. The bar and index are so balanced, that one end of the bar bears with a constant pressure on the specimen, and the graduated arc over which the index moves is divided into inches, tenths, and hundredths; and thousands are measured by a vernier scale on the end of the index. There is a screw at the lower end of the vertical bar, by which the index is set to zero, when necessary.

"The first trials were made with a bar of blistered steel of a very good quality. It was drawn out by the hammer to the width and thickness I had fixed upon, and then filed true and regular. It was then hardened, and tempered to the same degree of hardness as common files.

"The total length of the bar was 14 inches; the distance between the supports 13 inches; the breadth of the bar 0·95 inches, and the depth 0·375 inches; the thermometer varied from 55° to 57° at the times of trial.

	lbs.		inches.
"With a load of	54	the depression in the middle was	0·02
	82		0·03
	110		0·04

The last load remained on the bar some hours, but produced no permanent alteration of form.

"The temper of the bar was then lowered to a rather deep straw yellow, and it was tried again; when the same loads produced exactly the same flexures as before.

"The temper was then lowered till the colour was an uniform blue, or spring temper; and the trials were repeated with the same loads; but the flexures were still the same.

"It was now heated to redness and very slowly cooled. In this state the same loads still produced the same flexures; and the load of 110 lbs. caused no permanent change of form.

"The bar was hardened again, and made very hard; in this state the same loads produced the same flexures; and

	lbs.		inches.
with a load of	300	the depression in the middle was	0·115
	350		0·130
	580		broke.

When the bar was relieved from the load of 350 lbs. it retained a permanent flexure of 0·005 inches, which increased to 0·01 with the addition of 10 lbs. to the load.

"I found that a bar of much greater length might be tempered without difficulty, and therefore had another bar made of the same kind of steel; the length of which being 25 inches, about double the flexure could be given with the same strain upon the material, and therefore any small degree of difference in the elastic force might be more easily detected, for the preceding experiments are sufficient to show that if there be any difference, it must be extremely small.

"The breadth of this bar was 0·92 inches; the depth 0·36 inches; and the distance between the supports 24 inches. It was soft, so as to yield easily to the file.

	lbs.		inches.
"With a load of	18·6	the depression in the middle was	0·05
	37·0		0·10
	47·0		0·127

The bar was then hardened, so that a file made no impression on any part of it, and the same loads did not produce flexures that were sensibly different from those in the soft state.

"I then lowered the temper till it assumed an uniform straw colour; when

	lbs.		inches.
with a load of	47	the depression in the middle was	0·127
	85		0·230
	130		0·350
	150		0·400

The load of 150 lbs. produced a permanent set of 0·012, but 130 lbs. produced no sensible effect. The loading was continued, and

	lbs.		inches.
with	185	the depression in the middle was	0·50
	385		1·04

When 385 lbs. had been upon the bar about a minute, it emitted a faint creaking sound, and consequently I ceased to add fresh weights; in about fourteen minutes the bar broke, exactly in the middle of the length.

"On comparing the fractures of the specimens, there was no apparent difference except in colour. The grain was fine and equal; the small sparkles of metallic lustre abundant, and equally diffused; but in the harder specimen they had a whiter ground.

"From these experiments it appears that the elastic force of steel is sensibly the same in all states of temper.

"The height of the modulus of elasticity, calculated by the formula you have given in your Nat. Phil. (vol. ii. p. 48) is,

According to the first experiment	8,827,300 feet.
And according to the second experiment..	8,810,000

"Now the height of the modulus, as you had determined it for steel by Experiments on Vibration, is 8,530,000 feet (Nat. Phil. ii. p. 86). The modulus for cast steel calculated from Duleau's experiments (Essai Théorique et Expérimental sur le Fer forgé, p. 38) is 2,400,000 feet, and for German steel 6,600,000 feet.

"The force which produces permanent alteration is to that which causes fracture in hard steel, as 350 : 580; or as 1 : 1·66 in the same steel of a straw yellow temper as 150 : 385, or as 1 : 2·56.

"When the tension of the superficial particles at the strain which causes permanent alteration is calculated by the formula given in my Essay on the Strength of Iron, p. 146, 2nd Edition, it is 45,000 lbs. upon a square inch in tempered steel; and the absolute cohesion 115,000 lbs. Mr. Rennie found the direct cohesion of blistered steel to be 133,000 lbs. (Philosophical Transactions for 1818.)

"But in the very hard bar, the strain which produced permanent alteration was 51,000 lbs. for a square inch, and the absolute cohesion only 85,000 lbs.

"From these comparisons I think it will appear, that in the hardening of steel, the particles are put in a state of tension among themselves, which lessens their power to resist extraneous force. The amount of this tension should be equal to the difference between the absolute cohesion in different states. Taking Mr. Rennie's experiment as the measure of cohesion in the soft state, it will be 133,000 − 115,000 = 18,000 lbs. for the tension with a straw yellow temper; and 133,000 − 85,000 = 48,000 lbs. for the tension in hard steel. And if this view of the subject be correct, the phenomena of hardening may be explained in this manner, which nearly agrees with what you have observed in your Lectures, I, p. 644: after a piece of steel has been raised to a proper temperature, a cooling fluid is applied capable of abstracting heat more rapidly from the surface than it can be supplied from the internal parts of the steel. Whence the contraction of the superficial parts round the central ones which are expanded by heat; and the contraction of the central parts in cooling, while they are extended into a larger space than they require at a lower temperature, produces that uniform state of tension, which diminishes so much the cohesive force in hard steel. The increase of bulk by hardening agrees with this explanation; and it leads one to expect, that any other metal might be hardened if we could find a means of abstracting heat with greater velocity than its conducting power."

XIX. *A short Account of some Observations made with Chronometers, in two Expeditions sent out by the Admiralty, at the Recommendation of the Board of Longitude, for ascertaining the Longitude of Madeira and of Falmouth.* In a Letter to Thomas Young, MD. For. Sec. RS. and Secretary to the Board of Longitude. By Dr. John Lewis Tiarks.

Dr. Tiarks terminates this article with the following summary of the ultimate results he has obtained:—

"From the foregoing observations we may now conclude, that the longitudes laid down in the account of the Survey will deviate from the truth, in the same proportion in which the parallels of latitude on a spheroid, having the degree of the meridian in latitude 50° 41′ equal to that of the earth, and the ellipticity $\frac{1}{149}$ differ from those of the terrestrial spheroid, the compression of which is nearly $\frac{1}{310}$. The following comparisons will further illustrate the subject. If the radius of the Equator be =3486908 fathoms, and the semi-axis of the earth = 3475560 fathoms, which is nearly the result of the measurements in France, and Bouguer's in Peru, and corresponds to the compression $\frac{1}{310}$, the length of the degree perpendicular to the meridian in latitude 50° 41′ will be 60975·7 fathoms. For the spheroid adopted in the Survey, that degree is found 61,182 fathoms. The ratio of these numbers is 296 : 297, and the correction of the longitudes would be $\frac{1}{296}$; the same correction is, by the chronometrical observation, $\frac{1}{309}$. The length of the geodetical line BD, supposing the difference of longitude as determined in the account of the Survey, viz. 1° 26′ 47·93″, would be 338231 feet; whereas it was found to be 339397·6 feet; but if the longitude be increased in the ratio determined by the chronometers, the line will be 339334 feet, which is only 63·6 feet short of the measurement. The spheroid resulting from the compression which would make the difference of longitude of B and D = 1° 27′ 4·75″ (as it ought to be according to the results of the chronometers), and from the degree of the meridian in latitude 50° 41′, viz. 60851 fathoms, would have these dimensions: radius of the Equator = 3487907 fathoms; semi-axis = 3476687 fathoms;

compression $\frac{1}{314}$. The results of the chronometrical observations are therefore as much as could be expected in accordance with the correct determinations of the figure of the earth."

XX. *Of the Effects of the Density of Air on the Rates of Chronometers.* By George Harvey, FRSE. &c.: communicated by Davies Gilbert, Esq. VPRS.

See above; and *Annals* for May, 1824, p. 392.

XXI. *A Letter from* L. W. Dillwyn, Esq. FRS. addressed to Sir Humphry Davy, Bart. PRS.

We gave in our seventh volume, p. 177, Mr. Dillwyn's former letter on the interesting subject of the geological distribution of fossil shells, and the facts in the history of the creation which it indicates: we now extract his present communication.

"I beg leave to offer to the Royal Society some further observations on the relative periods at which different families of testaceous animals appear to have been created, and on the gradual approximation which may be observed in our British strata, from the fossil remains of the oldest formations to the living inhabitants of our land and waters.

"The series of strata beginning with transition lime and ending with lias, contains shells belonging to various genera of conchifera, cephalopoda, annelides, and herbivorous trachelipoda; and also some other shells, as for instance, the multilocular and spiriferous bivalves, which cannot be referred to either of those natural orders, or groups of genera, into which all the other testacea, both recent and fossil, have been divided. In the simple bivalves belonging to these strata, the marks which best serve to distinguish their families are generally obliterated, and but little more can with any certainty be observed, than that the two orders into which Lamark has divided them, have existed together throughout every formation from transition rocks to the present day. An examination of the few perfect specimens which I have met with, however, leads me to suspect that all the dimyaria of these strata have the ligament external, and consequently, that internal ligaments were confined to the monomyairia, till after the lias had been deposited.

"In the secondary beds above the lias, all the shells may be referred to some of our now existing orders of animals, and the extinction of the unknown orders is immediately followed by the first appearance of another order of mollusca, to which Lamarck has limited the name of gasteropoda, and, as was first suggested to me by Mr. Miller, all those fossils of the older strata, which have been supposed to be inside and outside casts of patellæ, were obviously formed in the concave sides of the vertebra, or by the intervertebral cartilages of a fish. As a few of the carnivorous trachelipoda are said to have been found in the oolites, their first appearance may probably be referred to the same epoch; but I have not myself been able to detect either of the families of this section of trachelipoda in any secondary bed, excepting the denuded tracts of green sand in Devonshire; and there, perforations exactly similar to those which abound among tertiary and recent shells are also of frequent occurrence, although I have never met with any such perforation in any other secondary formation, nor even in any of those regular beds of green sand, which actually underlie the chalk in other counties. I am not enough of a geologist to decide, as to whether any admixture of secondary and tertiary fossils may possibly have taken place when these denudations were made, but I can in no other way account for the fact, that all the species which have been perforated, as well as the carnivorous trachelipodes themselves, are nearly similar to those of the London clay; and I have never been able to find any such perforation in either of those species which are found in the more regular beds of green sand, and which are sometimes mixed with them. These perforations may be readily distinguished from those more oblique and lateral burrowings which are often found in secondary fossils, and are always conveniently formed for suction by being broadest in the outer surface, and go straight through that part of the shell which is immediately over the animal; whereas in the latter the holes are cylindrical, and much more resemble the indiscriminate burrowings which are common in recent oyster shells.

"In my former Letter, which the Royal Society has done me the honour to publish in the Philosophical Transactions of last year, I have pointed out some of the changes which took place immediately after the chalk formation was completed; and of the British strata it may be further observed, that it is only in the tertiary beds that any traces of the cirrhipeda, or of any of the families of naked mollusca have been found. The beak, which has been figured by Blumenbach, and which among the fossils of the lias is mentioned by Conybeare and Phillips as the beak of a sepia, belonged, as I think, unquestionably, to the cephalopode animal of an ammonite; and it sufficiently resembles the lower mandible of the parrot-like beak which Rumphius has described of nautilus pompilius. As might be expected, if these mandibles, or rather casts of mandibles, belong to the ammonites, they differ generically in shape from those of every living genus of cephalopoda which has been figured or described, and I have found them in all those beds; and, so far as I can ascertain, they have been discovered in those beds only of the lias, lower oolite, and chalk, which contain the larger ammonites. From the greater tenuity of these beaks in the smaller species, they may probably have yielded to pressure, and decay before the mud which filled them had become sufficiently hard to retain their shapes; and as the lower mandibles of the cephalopoda are always much larger and thicker than the upper ones, the non-appearance of any of the latter may be accounted for in the same way. The sepiæ are moreover furnished with one of those thick dorsal plates which are commonly called cuttle-fish bones, and most, if not all the other sepiadæ, contain an internal horny substance of the same nature, which is generally at least as thick and durable as the mandibles; and if the fossil beaks of the secondary strata belonged to this family, then, in all probability, some of the dorsal plates would be found with them; but nothing of the kind has been discovered in any older British stratum than the London clay. So far from being able to detect any traces of the naked mollusca, I have not been able to find, in the secondary strata, either of those shells by which the animal is only partially covered, nor any of those of the convolutæ, which necessarily change their shells at different periods of their growth, and of which the animal must therefore occasionally remain exposed, till a fresh coat of calcareous matter has been secreted. In my former Letter I have stated, that all the marine spirivalves of the secondary strata belong to operculated genera, and these observations serve still more strikingly to prove that, till the chalk deposits were completed, the mollusca, in our latitude, required a more perfect protection either from their enemies, or from the surrounding element, than afterwards became necessary.

"The same gradual approximation towards recent shells, which may be traced in the older strata, is also carried on through the tertiary formations, and the affinity, which is complete with respect to orders in secondary beds above the lias, becomes further extended, and every tertiary shell may be referred to some existing genus; but though the approximation has proceeded thus far in the London clay, yet all its immensely numerous species are now extinct; and it is only in those uppermost beds of crag, which lie between the London clay and our present creation, that any fossil can be completely identified with a living species: the shells which may be thus identified are however mixed with many extinct species; and though the fossils of the crab appear generally to have belonged to a warmer climate than ours, yet their character is much less tropical than those of the London clay, and in every respect they all approach nearer to the present inhabitants of the British coasts.

"I have already observed, that the shells of unknown families are confined to the beds below the lower oolite; and in all the upper formations a relationship is completed between fossil and recent shells in the following regularly approximating series. In the secondary strata above the lias as to *natural orders*, in the London clay as to *genera*, and partially as to *species* in the crag.

"These observations refer exclusively to the animals of the 9th, 10th, 11th, 12th, and 13th classes of invertebrata in Lamarck's arrangement; and whether the same sort of regularly approximating affinity can be observed in the other classes, I must leave it for those who are more conversant with them to decide."

XXII. *An Account of the Organs of Generation of the Mexican Proteus, called by the Natives Axolotl.* By Sir E. Home, Bart. VPRS.

The author of this paper considers that Cuvier has proved that the Proteus of Germany, as well as that of Carolina, are actually animals in a perfect state, and not larvæ. The discovery that the vertebræ of the Mexican Proteus were cupped in the same manner as those of the two other species, had already convinced him that it also belonged to the same tribe, and was consequently an animal in a perfect state. To place this question, however, beyond all doubt, Sir Everard obtained from Mr. Bullock several specimens, having the organs of generation in a developed state, brought from a Lake three miles from the city of Mexico. The temperature of this lake is never below 60°, and its elevation above the sea is 8000 feet: in the month of June, the Protei are so abundant in it as to form a principal part of the food of the peasantry. Three plates of these animals, with dissections of their generative organs, are given from drawings by Mr. Bauer. The female organs in their developed state are beautifully shown, and there is every probability, from the appearance of the ova contained within them, that they pass out singly.

XXIII. *An Account of Experiments on the Velocity of Sound, made in Holland.* By Dr. G. Moll, Professor of Natural Philosophy in the University of Utrecht, and Dr. A. Van Beek: communicated by Capt. H. Kater, FRS.

In our next number we shall give an abstract of these important and accurate experiments; together with remarks on the questions involved in the subject of the velocity of sound, and on some late investigations of them

XXIV. *A Catalogue of nearly all the principal fixed Stars between the Zenith of Cape Town, Cape of Good Hope, and the South Pole, reduced to the* 1*st of January*, 1824. By the Rev. Fearon Fallows, MA. FRS.

The nature of this first contribution to science from the new Observatory at Cape Town, renders it, of course, unsusceptible of abbreviation. The same may be said of the concluding paper in the volume; viz.

XXV. *Remarks on the Parallax of α Lyræ.* By J. Brinkley, DD. FRS. Andrew's Professor of Astronomy in the University of Dublin.

Article X.

Proceedings of Philosophical Societies.

GEOLOGICAL SOCIETY.

June 3.—A paper was read, entitled "Remarks on Quadrupeds imbedded in recent Alluvial Strata. By C. Lyell, Sec. GS."

In a former communication to the Society, the author had stated that he had found it difficult to explain the circumstances under which the remains of quadrupeds were very generally found imbedded in the shell marle in Scotland, often at considerable depths, and far from the borders of those lakes in which the marle is accumulated.

These animals must have been drowned when the lakes were of a certain depth. Their bones are found in the marle unaccompanied by sand or gravel, or any proofs of disturbing forces. From the shape of the surrounding land in some instances, it appears that floods could not have swept them in, and from the occasional absence of rivers flowing into others, they could not have been washed in by them.

The author, therefore, suggests that they were lost in attempting to cross the ice in winter, the water never freezing sufficiently hard above the springs to bear their weight, and springs abounding always in those lakes in Forfarshire and Perthshire in which marle is deposited.

The skeletons of some of the animals found in the shell marle in Forfarshire are in a vertical position, but some are not. The same circumstance has been remarked with regard to the elks occurring in the marle in the Isle of Man. Of these facts Mr. Lyell offers the following explanation.

Cattle which are lost in bogs and marshes sink in and die in an erect posture, and are often found with their heads only appearing above the surface of the ground. When, therefore, a lake in which marle is deposited is shallow, the quadrupeds which fall through the ice sink into the marle in the same manner, and perish in an upright posture, but when the lake is deep, and the animals are dead before they reach the bottom, they become enveloped in the marle in any position rather than the vertical.

June 17.—An extract of a letter was read from John Kingdom, Esq.: communicated by Joseph Townsend, Esq. FGS.

Mr. Kingdom mentions in this letter the situation in which certain bones of a very large size, appearing to have belonged to a whale and a crocodile, were lately found completely imbedded in the oolite quarries, about a mile from Chipping Norton, near Chapel House.

A paper was also read, entitled "Observations, &c. on a Walk from Exeter to Bridport." Mr. Woods, in this communication, describes the nature of the soil in the neighbourhood of Exeter, and the strata exhibited in the cliffs and on the sea shore between that city and the east side of Bridport Harbour.

Article XI.

SCIENTIFIC NOTICES.

Chemistry.

1. *Formation of Ammonia.*

Mr. Faraday has lately published in the Journal of Science, an account of some experiments on the formation of ammonia by the action of substances apparently containing no azote; he states his belief, however, that the results depend upon the difficulty of perfectly excluding azote, and the extreme delicacy of the test of its presence afforded by the formation of ammonia. The principal experiments performed by Mr. Faraday, we shall give nearly in his own words:—

"Put a small piece of clean zinc foil into a glass tube closed at one end, and about one-fourth of an inch in diameter; drop a piece of potash into the tube over the zinc; introduce a slip of turmeric paper slightly moistened at the extremity with pure water, retaining it in the tube in such a position that the wetted portion may be about two inches from the potash; then holding the tube in an inclined position, apply the flame of a spirit lamp, so as to melt the potash that it may run down upon the zinc, and heat the two whilst in contact, taking care not to cause such ebullition as to drive up the potash; in a second or two, the turmeric paper will be reddened at the moistened extremity, provided that part of the tube has not been heated. On removing the turmeric paper and laying the reddened portion upon the hot part of the tube, the original yellow tint will be restored: from which it may be concluded that ammonia has been formed; a result confirmed by other modes of examination."

The atmospheric being suspected to be the source of the azote, the experiment was repeated on hydrogen gas, but the same results were obtained. It was afterwards imagined that the azote might be furnished by accidentally touching the potash with animal or other substances containing azote; and as a proof of how necessary it is to avoid fingering the substances experimented upon, Mr. Faraday states the following experiment:—

"Some sea sand was heated red hot for half an hour in a crucible, and then poured out on to a copper-plate, and left to cool; when cold, a portion of it (about 12 grains) was put into a clean glass tube; another equal portion was put into the palm of the hand, and looked at for a few moments, being moved about by a finger, and then introduced by platina foil into another tube, care being taken to transfer no animal substance but what had adhered to the grains of sand: the first tube when heated yielded no signs of ammonia to turmeric paper, the second a very decided portion.

"As a precaution, with regard to adhering dirt, the tubes used in precise experiments were not cleaned with a cloth, or tow, but were made from new tube, the tube being previously heated red hot, and air then drawn through it; and no zinc or potash was used in these experiments, except such as had been previously tried by having portions heated in a tube to ascertain whether when alone they gave ammonia.

"It was then thought probable that the alkali might contain a minute quantity of some nitrous compound, or of a cyanide, introduced during its preparation. A carbonate of potash was therefore prepared from pure tartar, rendered caustic by lime calcined immediately preceding its use, the caustic solution separated by decantation from the carbonate of lime, not allowed to touch a filter or any thing else animal or vegetable, and boiled down in clean flasks; but the potash thus obtained, though it yielded no appearance of ammonia when heated alone, always gave it when heated with zinc.

"The water used in these experiments was distilled, and in cases where it was thought necessary was distilled a second, and even a third time. The experiments of Sir Humphry Davy show how tenaciously small portions of azote are held by water, and that, in certain circumstances, the azote may produce ammonia. I am not satisfied that I have been able to avoid this source of error.

"At last, to avoid every possible source of impurity in the potash, a portion of that alkali was prepared from potassium; and every precaution taken that could be devised for the exclusion of azote; yet, when a lamp was applied to the potash and zinc, the alkali no sooner melted down and mingled with the metal, than ammonia was developed; which rendered the turmeric paper brown, the original yellow re-appearing by the application of heat to the part.

"Still anxious to obtain a potash which should be unexceptionably free from any source of azote, I heated (says Mr. F.) a portion of potash with zinc, endeavouring to exhaust any thing it might contain which could give rise to the formation of ammonia: it was then dissolved in pure water, allowed to settle, the clear portion poured off and evaporated in a flask by boiling; but the potash thus prepared gave ammonia, when heated with zinc, in hydrogen gas."

"Potash is not the only substance which produces this effect with the metals and vegetable substances. Soda produces it; so also, does lime, and baryta, the latter not being so effective as the former, or producing the phenomena so generally. The common metallic oxides, as those of manganese, copper, tin, lead, &c. do not act in this manner.

"Water or its elements appear to be necessary to the experiment. Potash or soda in the state of hydrates generally contain the water necessary. Potash dried as much as could be by heat, produced little or no ammonia with zinc; but re-dissolved in pure water and evaporated, more water being left in it than before, it was found to produce it as usual. Pure caustic lime, with very dry linen, produced scarcely a trace of ammonia, whilst the same portion of linen with hydrate of lime yielded it readily.

"The metals when with the potash appear to act by, or according to, their power of absorbing oxygen. Potassium, iron, zinc, tin, lead, and arsenic evolve much ammonia, whilst spongy platina, silver, gold, &c., produce no effect of the kind. A small portion of fine clean iron wire dropped into potash melted at the bottom of a tube, caused the evolution of some ammonia, but it soon ceased, and the wire blackened upon its surface; the introduction of a second portion of clean wire caused a second evolution of ammonia. Clean copper wire, in fused potash, caused a very slight evolution of ammonia, and became tarnished."

Mineralogy.

2. *Sulphato-tri-carbonate of Lead.*

M. Stromeyer has lately examined the sulphato-tri-carbonate of lead, whose composition was first pointed out by Mr. Brooke. His results, which confirm the conclusions of the latter gentleman, give, for the constituent elements of the mineral,

Carbonate of lead	72·7
Sulphate of lead	27·3
	100·0

and it consequently consists of one atom of sulphate of lead and three atoms of carbonate of lead, as previously determined by Mr. Brooke.

3. *Hydrate of Magnesia.*

A specimen of native hydrate of magnesia from Swinaness, in Unst, examined by M. Stromeyer, gave

Magnesia	66·67
Oxide of manganese	1·57
Protoxide of iron	1·18
Lime	0·19
Water	30·39
	100·00

4. *Magnesite.*

100 parts of magnesite, from Salem, in India, gave the same accurate analyst:

Carbonic acid	51·83
Magnesia	47·88
Lime	0·28
Oxide of iron	Trace
	99·99

5. *Seleniuret of Lead.*

MM. Stromeyer and Hausmann have examined an ore from the Laurence Mine, at Clausthal, which proves to be a seleniuret of lead. The ore is combined with brown spar, and from its imparting a smalt blue colour to glass, was supposed to contain cobalt, and had been described under the name of *cobalt bleyerz*, by M. Hausmann.

Externally, seleniuret of lead has a greater resemblance to some varieties of galena than to any other substance, but its colour is peculiar, and partakes more of a blue tint than even the *wasserblei* of the German mineralogists (*sulphuret of molybdena*). It has a tendency to crystallization, but its crystalline form has not yet been ascertained. The fracture is fine grained; lustre metallic, not very brilliant. It is inferior in hardness to galena, is not harsh to the touch, gives a dark streak, and retains its metallic lustre after being rubbed; its specific gravity = 7·697. It becomes negatively electrical by friction.

Seleniuret of lead fuses readily before the blowpipe, gives out a powerful smell resembling that of rotten turnips, and becomes covered with a brownish red crust, which is succeeded by a coating of yellow oxide of lead. When the flame is directed on the ore, a bright blue tint is developed; fused with borax it gives a pale smalt coloured glass.

Heated in a glass tube by a spirit lamp, selenium sublimes, exhaling its peculiar offensive odour, and the surface of the glass becomes covered with a light sublimate of a brownish red colour. By heating the tube to redness, the ore fuses, but suffers no other apparent change; but on continuing the heat, the brownish red sublimate gradually disappears, and is succeeded by a white, acicular crystalline substance, which attracts moisture by exposure, and deliquesces. It reddens litmus paper strongly; becomes yellow by the action of sulphuric acid, and red by sulphuretted hydrogen: hence it is similar to selenic acid.

Cold nitric acid acts on seleniuret of lead, and after some time the mass assumes a cinnamon red colour, in consequence of the separation of selenium. If the acid be heated, the whole of the ore is dissolved, the selenium first separating in the form of red flakes, which soon lose their colour, turn brown, and gradually disappear. If the quantity of ore be in excess, the selenium collects in brown flakes on the surface of the solution, which sometimes assumes the appearance of an oily film.

The nitric acid solution has a pale reddish colour, derived from a slight portion of cobalt; but no other metal, besides lead and cobalt, is contained in the ore; neither is any trace of sulphur detected by the action of nitrate of barytes.

The analysis of this ore gave M. Stromeyer per cent.

Lead	70·98
Cobalt	0·83
Selenium	28·11
	99·92

6. *Selenium in the Sulphur of the Lipari Isles.*

Amongst the volcanic productions of the Lipari Isles, a sal ammoniac is found combined with sublimed sulphur in alternate white and brownish orange layers, the colour of the latter of which has generally been attributed to iron. On examination, however, neither tincture of galls, prussiate of potash, nor ammonia, gave any indication of that metal, but sulphuretted hydrogen gave a precipitate of orpiment, owing to the presence of some arsenious acid.

When the sal ammoniac is dissolved in water, a brownish yellow residuum is left, which fuses readily in a glass tube, and affords an orange coloured sublimate. On hot coals it inflames, and exhales at first a mixed odour of sulphur and arsenic, which is succeeded by the peculiar offensive smell of selenium. By digestion in nitric acid till the orange colour disappeared, a solution was obtained from which sulphate of potash threw down a considerable quantity of a cinnabar coloured precipitate, possessing all the characters of selenium; and the solution afforded by evaporation acicular crystals of selenic acid.

This discovery by M. Stromeyer of selenium amongst the volcanic products of the Lipari Islands, renders it probable that the peculiar orange tint of the sulphur found in those islands proceeds chiefly from selenium, and not, as hitherto supposed, from arsenic combined with the sulphur.

7. *Latrobite.*

M. Gmelin, of Tubingen, has found the composition of the mineral named Latrobite by Mr. Brooke, and described by him in the *Annals of Philosophy*, vol. v, New Series, to be as follows:

Silica	44·653
Alumina	38·814
Lime	8·291
Oxide of manganese	3·160
Potash	6·575
	101·493

Zoology.

8. *On the Teeth of the Koala.* By J. E. Gray, Esq.

Cuvier, in his Animal Kingdom, only describes the cutting teeth of the Koala. Blainville, in his Podomus of a new Distribution of Animals, abridged in the ninth volume of this Journal, describes the cutting teeth as $\frac{6}{2}$ upper middle longest, false canine $\frac{2 \,.\, 2}{0 \,.\, 0}$, grinders, $\frac{4 \,.\, 4}{4 \,.\, 4}$ with four tubercles. Mr. F. Cuvier, in his work on the Teeth of Mammalia, observes, that he has not seen a skull of the Koala, but that it must doubtless be allied to the Phalangers. I some time ago met with a skull of this animal in the collection of the College of Surgeons, and I am indebted to the kindness of Mr. Clift for allowing me to take a description of it.

The skull short, compressed, and depressed, so as to be subquadrangular. The temporal fossæ large, the cutting teeth $\frac{6}{2}$ upper, two front large, distant at the base, conversing at the apex, the rest small; lower large, approximating together at the tip; canine teeth $\frac{1 \,.\, 1}{0 \,.\, 0}$, small conical placed on the suture of the intermaxillary bone, grinders $\frac{5 \,.\, 5}{5 \,.\, 5}$ all with two fangs, the front one on each side smallest, rather compressed; the rest depressed, each with four acute tubercles.

Blainville describes his animal as chocolate brown, and Cuvier and Goldfus as ash grey, the latter agrees with the five specimens that I have seen. Whether this difference was occasioned by Cuvier and Blainville describing two different animals, or by the latter, in his hasty notes, having confounded it with the Wombat in his description, I am not able to determine.

9. *On the Umbilicus of Marsupial Animals.*

It has been generally thought that Marsupial animals are destitute of any umbilicus, and are only attached to the mother by means of the mouth.

Geoffroy St. Hilaire has lately discovered in some specimens of the fœti of *Didelphis Virginiana* preserved in spirits, which had been taken from the mother by Dr. Barton very soon after their introduction into the pouch, evident vestiges of placentral organisation, and of an umbilicus. They were only five lines long, and already formed; in the two male which he examined the umbilicus was large for the size of the animal, as it was also in the female, and very distinct from the entrance of the pouch.

Mr. Geoffroy observes, that the series of transformations common to all mammalia are *Ovulum*, *Embryo*, and fœtus. These three stages of genital products require three distinct situations which in the other mammalia are found in the sexual canal, but in the *Marsupialia* they are very differently distributed through in an equally continuous series. The ovulum and the embryo are formed and developed in the sexual canal and the fœtus out of it. The womb is the third station in the mammalia, where the common fœtus is incubated and nourished; and the *Marsupium* or nursing pouch is for the same purpose in the marsupial animals. The difference, therefore, consists only in the name of the last part.—(Ann. Sci. Nat. and Zool. Jour. i. 403.)

Miscellaneous.

10. *A Method of fixing Crayon Colours.* By James Smithson, Esq.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *London, Aug.* 23, 1825.

Wishing to transport a crayon portrait to a distance for the sake of the likeness, but without the frame and glass, which were bulky and heavy, I applied to a man from whom I expected information for a method of fixing the colours. He had heard of milk being spread with a brush over them, but I really did not conceive this process of sufficient promise to be disposed to make trial of it.

I had myself read of fixing crayon colours by sprinkling solution of isinglass from a brush upon them, but to this too, I apprehended the objections of tediousness, of dirty operation, and perhaps of incomplete result.

On thinking on the subject, the first idea which presented itself to me was that of gum-water applied to the *back* of the picture; but as it was drawn on sized blue paper pasted on canvass, there seemed little prospect of this fluid penetrating. But an oil would do so, and a drying one would accomplish my object. I applied drying oil diluted with spirit of turpentine; after a day or two when this was grown dry, I spread a coat of the mixture over the front of the picture, and my crayon drawing became an oil painting.

Article XII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Dr. Shearman has in the press a work, entitled "Practical Observations on the Nature, Causes, and Treatment of Water in the Brain."

Sketches Political, Geographical, and Statistical, of the United Provinces of Rio de la Plata, &c. 8vo.

JUST PUBLISHED.

Observations on Tetanus, illustrated by Cases in which a new and successful Mode of Treatment has been adopted. By H. Ward. *5s.*

Flora Conspicua. By Richard Morris, FLS. Royal 8vo. No. II. *3s. 6d.*

Elements of Conchology according to the Linnæan System. By the Rev. E. I. Burrow, FLS. &c. New Edition. 16*s.*

Military Medical Reports, containing Pathological and Practical Observations illustrating the Diseases of Warm Climates. By James M'Cube, MD. 8vo. 7*s.*

Practical Commentaries on the present Knowledge and Treatment of Syphilis, with coloured Illustrations of some ordinary Forms of that Disease. By Richard Wellbank. 8vo. 7*s.* 6*d.*

Directions for drinking the Cheltenham Waters. 12mo. 2*s.* 6*d.*

Article XIII.

NEW PATENTS.

C. Friend, Bell-lane, Spitalfields, sugar-refiner, for improvements in the process of refining sugar.—July 26.

J. Reedhead, Heworth, Durham, for improvements in machinery for propelling vessels of all descriptions, both in marine and inland navigation.—July 26.

J. E. Brooke, Headingly, Leeds, woollen manufacturer, and J. Hardgrave, Kirkstall, of the same place, woollen manufacturer, for improvements in or additions to machinery used in scrubbing and carding wool, or other fibrous substances.—July 26.

D. O. Richardson, kerseymere and cloth printer, and W. Hirst, manufacturer, both of Leeds, for improvements in the process of printing or dyeing woollen and other fabrics.—July 26.

J. Kay, Preston, Lancashire, cotton spinner, for machinery for preparing and spinning flax, hemp, and other fibrous substances, by power.—July 26.

R. Witty, Sculcoates, Yorkshire, civil engineer, for an improved chimney for Argand and other burners.—July 30.

J. Lean, Fishpond House, near Bristol, for a machine for effecting an alternating motion between bodies revolving about a common centre or axis of motion; also certain additional machinery or apparatus for applying the same to mechanical purposes.—July 30.

The Rev. W. Barclay, Auldearc, Nairnshire, for an improved instrument to determine angles of altitude or elevation, without the necessity of a view of horizon being obtained.—July 30.

R. Badnall, Leek, Staffordshire, silk manufacturer, for improvements in the manufacture of silk.—July 30.

S. Bagshaw, Newcastle-under-line, Staffordshire, for a new method of manufacturing pipes for the conveyance of water and other fluids.—Aug. 8.

G. Charleton, Maidenhead-court, Wapping, and W. Walker, New Grove, Mile-end Road, Stepney, master mariners, for improvements in the building or constructing of ships or other vessels.—Aug. 10.

S. Lord, J. Robinson, and J. Forster, Leeds, copartners, merchants, and manufacturers, for improvements in machinery for and in the process of raising the pile on woollen cloths and other fabrics, and also in pressing the same.—Aug. 11.

W. Hirst, H. Hirst, and W. Heycock, woollen cloth manufacturers, and S. Wilkinson, mechanic, Leeds, for an apparatus for preventing coaches, carriages, mails, and other vehicles, from overturning.—Aug. 11.

J. Stephen Langton, Langton Juxta Partney, Lincolnshire, for an improved method of seasoning timber and other wood.—Aug. 11.

J. Perkins, Fleet-street, engineer, for improvements in the construction of bedsteads, sofas, and other similar articles.—Aug. 11.

H. R. Fanshaw, Addle-street, London, silk embosser, for an improved apparatus for spinning, doubling and twisting, or throwing silk.—Aug. 12.

J. Butler, Commercial Road, Lambeth, for a method of making coffins for the effectual prevention of bodies being removed therefrom, or taken therefrom, after interment.—Aug. 12.

M. Lariviere, Frith-street, Soho, mechanicean, for a machine for perforating metal plates of gold, silver, tin, platina, brass, or copper, being applicable to all the purposes of sieves, hitherto employing either canvas, linen, or wire.—Aug. 15.

J. A. Taylor, Great St. Helen's, London, for a new polishing apparatus for household purposes.—Aug. 15.

C. Downing, Bideford, Devonshire, for improvements in fowling-pieces and other fire-arms.—Aug. 15.

A. Schoolbred, Jermyn-street, St. James's, tailor, for improvements on, or a substitute for, back stays and braces for ladies and gentlemen, chiefly to prevent relaxation of the muscles.—Aug. 18.

P. Taylor, City Road, engineer, for improvements in making iron.—Aug. 18.

P. Williams, Leeds, and J. Ogle, Holbeck, Yorkshire, cloth manufacturers, for improvements in fulling mills, or machinery for fulling and washing woollen cloths, or such other fabrics as may require the process of felting or fulling.—Aug. 20.

Article XIV.

METEOROLOGICAL TABLE.

1825.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
7th Mon.							
July 1	N W	30·27	29·90	74	45	—	05
2	W	30·30	30·27	76	45	—	
3	N	30·27	30·26	83	57	—	
4	N W	30·39	30·26	80	55	—	
5	N W	30·39	30·25	75	48	—	
6	N	30·25	30·16	68	45	·91	04
7	N W	30·18	30·16	71	45	—	
8	N	30·16	30·13	76	51	—	
9	N	30·13	30·12	79	53	—	
10	W	30·12	30·10	87	50	—	
11	W	30·18	30·10	89	58	—	
12	N W	30·18	30·16	89	58	—	
13	N W	30·25	30·16	86	59	·95	
14	S W	30·25	30·11	92	58	—	
15	S	30·24	30·11	95	62	—	
16	N	30·32	30·24	91	58	—	
17	E	30·24	30·22	92	58	·90	
18	S E	30·24	30·22	97	62	—	
19	S E	30·29	30·24	95	58	—	
20	S E	30·33	30·29	87	56	·94	
21	E	30·29	30·27	79	47	—	
22	E	30·27	30·14	80	52	—	
23	E	30·25	30·14	74	44	—	
24	N E	30·40	30·25	72	42	—	
25	N E	30·40	30·38	78	40	·85	—
26	N E	30·38	30·37	74	45	—	
27	N E	30·37	30·26	82	48	—	
28	E	30·28	30·26	84	49	—	
29	E	30·26	30·23	81	43	—	
30	E	30·23	30·13	80	47	·95	
31	E	30·13	30·10	91	52	·30	
		30·40	29·90	97	40	5·80	·09

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Seventh Month.—1. Showers. 2—5. Fine. 6. Cloudy, with showers. 7. Cloudy. 8—31. Fine, clear, and dry.

RESULTS.

Winds: N, 5; NE, 4; E, 8; SE, 3; S, 1; SW, 1; W, 3; NW, 6.

Barometer: Mean height

For the month.... .... 30·229 inches.

Thermometer: Mean height

For the month.................................. 66·887°

Evaporation .... 5·80 in.

Rain.. 0·09

*** The very unusual height of the thermometer on several days of this month having led me to examine the position of the instrument, I was induced to think that it indicated a higher temperature than that of the air, in consequence of radiation from the dry and heated earth in the neighbourhood. To ascertain the extreme amount of this error, I suspended a thermometer in a spot thickly shaded with trees, and overhanging a river, so as to exclude the influence of radiation, and found it indicate 4° to 5° lower on the days of the greatest heat—probably the real temperature of the *air* was between these points.

Laboratory, Stratford, Eighth Month, 23, 1825. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1825.

Article I.

Observations on the Synonima of the Genera Anomia, Crania Orbicula, and Discina. By J. E. Gray, Esq. FGS. &c.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *British Museum, Sept.* 12, 1825.

Linnæus, in his Systema Naturæ, has formed a genus with the following characters, "*Animal* corpus ligula, emarginata ciliata, ciliis valvulæ superiori affixis: *brachiis* 2, linearibus, corpore longioribus, conniventibus, porrectis, valvulæ alternis, utrinque ciliatis, ciliis affixis valvulis utrisque. *Testa* inæquivalvis; valvula altera planiuscula, altera basi magis gibba, harum altera sæpe basi perforata. *Cardo* edentulus, cicatricula lineari prominente introrsum dente laterali; valvulæ vero planioris in ipso margine. *Radii* duo ossei pro basi animalis." This genus he has called *Anomia*, a name most probably derived from the specific titles of the *Concha anomia* of Fabius Columna, and the *Pectunculus anomius* of Dr. Lester, which appear intended to have been expressive of the inequality or dissimilarity of the valves of those shells.

The above character of the animal agrees exactly with that given by Cuvier to a group of Mollusca, which he calls Brachiopoda, from the belief of their ciliated arms being the modified feet of the Conchophorus Mollusca; and the description of the shell, on account of the larger valve being said to be gibbous and perforated in the hinder part, and the inside of one of the valves being furnished with two bony rays, must have been taken from one of those shells which Bruguiere has since formed into a genus, under the name of *Terebratula;* a word most likely derived from Linnæus, who uses it as a specific name for one of the species which belong to this section of his genus.

Linnæus, from not knowing the animal, has placed several species in his genus which recent observation has proved not to agree with the characters that he has given to it. Thus *A. craniolaris* has a similar animal, but has no teeth in its hinge, and has consequently been formed into a genus, under the name of *Crania*, by Bruguiere. *A. spina*, of which the animal is unknown, has been equally separated into a genus called *Plagiostama*, by Lamarck. The animal again of *A. Ephiphium*, *A. Cepa*, *A. Electrica*, *A. squamula*, and *A. patelliformis*, is very dissimilar from that described by Linnæus as appertaining to *Anomia*. It is in fact most nearly allied to that of the oyster, and is, therefore, what Linnæus calls an *Ascidia*. To these latter species, Bruguiere has retained the name of *Anomia*, which is much to be regretted, as had he studied the beautiful character of Linnæus, he would have found that that author did not intend these species as the type of his genus; and there is little doubt that had he known their animal, he would have placed them with the *Ostreæ*, or have formed a new genus for their reception; there being no part of his system where he so rapidly increased the number of his genera as in the *Testacea*. It is more to be regretted that later conchologists should not have corrected this error, but have let it continue so long, that although it errs against one of the best laws of nomenclature, we are almost inclined to allow it to remain without correction, for Bruguiere's names have been in general use both by conchologists and geologists, and consequently any alteration in them would introduce much confusion into these delightful sciences. Still, however, it is but proper to point out that the immortal Swede was the first to describe with accuracy the curious animal of the *Brachiopodous* Mollusca.

But I do not consider that the same line of conduct should be pursued in the next case which I am going to state, where even greater confusion has been caused by the haste to form conclusions, and by little attention being paid to the characters given by the original describers of genera.

Linnæus, in his "*Fauna Suecica*," has placed as the first species of *Anomia*, a shell, under the name of *A. craniolaris;* and here I may object to the opinion held by many persons, that the first species of the genus is to be considered the type of it. This may be the case in the works of Fabricius, but I believe not so in those of most other authors. Linnæus appears to have referred this shell to the present genus, from it having sometimes, as he describes, three holes in its lower valve; but these holes are only the places where the adductor muscles of the animal have been attached, which, being of a more brittle texture, decay sooner than the rest of the shell, which is always found in a fossil state. Bruguiere has formed this shell into a genus under the name of *Crania*, from the similarity of the three scars, or holes, to the apertures in a human skull that correspond with the eyes and nose of every subject. He has taken the Linnæan specific character as a generic one, or at least he describes the lower valve as pierced with three holes. Lamarck has adopted this genus, and referred to it a recent species, which I believe to be the same, or very nearly related to the following shell.

Muller, in his excellent work, *Zoologia Danica,* has described a shell with the name of *Patella anomala,* which he afterwards figured with its animal, and discovered that it was furnished with an under valve; and yet for some unaccountable reason, he still considered it as a *Patella!* Poli, in his superb work on the shells of the Two Sicilies, figured a similar shell under the name of *Anomia turbinata,* referring it to this genus most likely on account of the animal agreeing with the Linnæan character. But as he always gave the animals a new generic name, he called the present animal *Criopus,* as he did that of *Anomia Epiphium, Echion.* Cuvier, from observing that the animals of these species were similar to the *Terebratulæ,* and that the shell was soldered to the rock immediately by the outer surface of the lower valve, and not attached, as in the latter genus, by a *tendon,* formed them into a genus under the name of *Orbicula,* taking no notice of the genus *Crania* of Bruguiere. The genus *Orbicula* has also been adopted by Lamarck; but I believe that most persons, on consulting the figures of Muller, Poli, Chemnitz, and of a shell described by Montague, in the Linnæan Transactions, under the name of *Patella distorta,* which has since been discovered to have an attached under valve, and stated to be an *Orbicula* by Blainville, will agree with me, that these shells are all the same (or two very nearly allied) species, and consequently that the genus *Orbicula* must be expunged from the system, for it is nothing more than a recent species of *Crania.*

Mr. G. B. Sowerby, in a paper read at the Linnæan Society on March, 1818, entitled "*Remarks on the Genera Orbicula and Crania of Lamarck, with Descriptions of two Species of each Genus, and some Observations to prove the P. distorta of Montague to be a Species of Crania,*" has described a shell which was found on some stones which had been brought from Africa, as ballast, and were used to repair the roads. This he considered to be *Orbicula Norvegica* of Lamarck, who expressly stated that the latter shell is attached by the soldering of the outer surface of the lower valve to the rock, whereas Mr. Sowerby's shell is affixed by means of a tendron passing through a slip in the disk of the lower valve. This tendon he has called a foot; but it has no analogy with the foot of the animals of the bivalves; and I believe is only a slightly modified adductor muscle. Some of these shells having been sent to Lamarck, the latter formed them into a genus under the name of *Discina,* giving a very expressive generic character, but being misled by receiving it from

England, he considered it as found on the shores of our coast. Mr. Sowerby's paper not having been printed till the publication of the thirteenth volume of the Linnæan Transactions, which was several years after the reading of the paper, and the volume of Lamarck's History containing this shell appearing in the mean time, he added as a note to his paper, that Lamarck had erroneously described his shell twice over, once under the name of *Discina*, and again under that of *Orbicula*, and he has published the same opinion in his "genera of shells." But with this idea I am sorry that I cannot coincide, and more especially so as Ferussac, in his Synoptic Table, and several others, have taken Mr. Sowerby's *ipse dixit* for fact. To prove that the *Discina* is not a species of *Orbicula*, it is only necessary to inspect the beautiful plate by Mr. James Sowerby, which accompanies his brother's paper, and compare it with the figures of Muller; and I believe that most persons will allow that Muller's figures agree best in the form of the muscular impression with the *Crania*, on the before-mentioned plate; and the slit in the under valve could never have escaped the accurate eye of Muller, who has figured his shell with so much detail.

It appears to be the fate of this genus that it should be confused. Blainville, in his article on Mollusca, just published, has divided the genus *Orbicula* into two sections; 1. The adherent valve not pierced, O. lævis, *Sow. Lin. Trans.* xiii. 2. The adherent valve pierced, and provided with a compressed medial apophysis, *Discina*, O. Norvegica, *Sow. Lin. Trans.* xiii. when it is only necessary to consult the shells or plates, for the lower valves of both are exactly similar, and oddly enough the true *O. Norvegica* of Muller is taken no notice of.

In accordance to these views, the genera before-mentioned appear to require the following synonima;—

1. Anomia, *Bruguiere*.
   Anomiæ Pars, *Linnæus*.
   Echioderma (Echion), *Poli*.

2. Terebratula, *Bruguiere*.
   Anomia, *Linnæus*.
   Criopiderma (Criopus), *Poli*.

To this genus may be added as subgenera *Magus*, *Spirifer*, and perhaps *Productus* of Sowerby, and *Gryphea* of Megerle.

3. Crania, *Bruguiere*.
   Anomiæ pars, *Linnæus*.
   Patella species, *Muller*, *Montague*.
   Criopiderma (Criopus), *Poli*.
   Orbicula, *Cuvier and Lamarck*, not Sowerby nor Blainville.
   Terebratula, *Schweigger*.

4. Discina, *Lamarck*.
   Orbicula, *Sowerby*, *Blainville*, not Lamarck.

ARTICLE II.

Corrections in Right Ascension of 37 Stars of the Greenwich Catalogue. By James South, FRS.

Mean AR 1825.	γ Pegasi h. m. s. 0 4 14·25	Polaris h. m. s. 0 58 17·50	α Arietis h. m. s. 1 57 19·77	α Ceti h. m. s. 2 53 8·58	Aldebaran h. m. s. 4 25 53·44	Capella h. m. s. 5 3 46·61	Rigel h. m. s. 5 6 8·00	β Tauri h. m. s. 5 15 14·29	α Orionis h. m. s. 5 45 42·18
Oct. 1	+ 4·63″	+ 65·40″	+ 4·95″	+ 4·42″	+ 4·42″	+ 5·55″	+ 3·61″	+ 4·62″	+ 3·81″
2	63	65·57	96	44	45	59	63	65	84
3	64	65·68	97	45	48	63	66	68	87
4	64	65·80	98	47	51	67	68	72	90
5	64	65·91	5·00	49	54	71	71	75	92
6	65	66·03	01	51	57	75	74	78	95
7	65	66·14	02	52	60	79	76	81	98
8	65	66·19	03	53	62	83	78	84	4·01
9	65	66·23	05	55	65	87	81	87	03
10	65	66·28	06	56	68	91	83	90	06
11	65	66·33	07	58	70	94	86	93	09
12	65	66·37	08	59	73	98	88	96	12
13	64	66·35	10	61	75	6·02	91	5·00	15
14	64	66·33	11	63	78	05	94	03	17
15	64	66·30	12	64	81	09	96	06	20
16	64	66·28	14	66	83	13	99	09	23
17	64	66·25	15	68	86	17	4·02	12	26
18	64	66·17	16	69	88	20	04	15	29
19	63	66·09	16	71	90	24	07	18	31
20	63	66·00	17	72	93	27	09	21	34
21	63	65·91	18	74	95	31	11	24	37
22	63	65·83	19	75	97	34	14	27	39
23	62	65·68	20	77	5·00	38	16	30	42
24	62	65·52	20	78	02	41	18	33	45
25	62	65·36	21	80	04	45	21	36	48
26	61	65·20	22	81	06	49	23	39	50
27	61	65·05	22	82	08	52	26	42	53
28	61	64·84	23	83	10	55	28	45	56
29	60	64·63	23	84	12	58	30	48	58
30	60	64·42	24	85	14	61	33	50	61
31	59	64·21	25	86	16	64	35	53	63
Nov. 1	59	64·00	26	87	18	67	37	56	65
2	58	63·73	26	88	20	71	39	58	68
3	58	63·46	27	89	22	74	42	61	70
4	57	63·19	28	90	24	77	44	63	73
5	57	62·92	28	91	26	80	46	66	75
6	56	62·64	29	92	28	83	48	68	78
7	55	62·32	29	93	30	86	50	70	80
8	55	62·00	30	94	32	88	52	72	83
9	54	61·68	30	94	33	91	54	75	85
10	53	61·36	30	95	35	94	56	77	88
11	52	61·03	31	96	37	97	58	80	90
12	52	60·63	31	96	38	7·00	60	82	93
13	51	60·23	31	97	40	02	62	85	95
14	50	59·83	31	98	42	05	64	87	98
15	49	59·42	31	98	43	08	66	90	5·00
16	48	59·02	32	99	45	11	68	92	02
17	47	58·58	32	5·00	47	13	70	94	04
18	46	58·15	32	00	48	16	71	97	06
19	46	57·71	32	01	50	18	73	99	09
20	45	57·28	32	01	51	21	75	6·01	11
21	44	56·84	32	02	53	23	76	03	13
22	43	56·35	32	02	54	26	78	05	15
23	42	55·86	32	03	56	28	79	07	17
24	41	55·38	32	03	57	31	81	09	19
25	40	54·89	32	04	59	33	83	11	21
26	39	54·40	32	04	61	36	84	13	23
27	38	53·87	31	04	62	38	85	15	25
28	37	53·35	31	04	63	39	86	16	26
29	36	52·82	31	04	64	41	88	18	28
30	35	52·30	30	05	65	43	89	19	30

Mean AR 1825.	Sirius	Castor	Procyon	Pollux	α Hydræ	Regulus	β Leonis	β Virginis	Spica Virg
	h. m. s. 6 37 26·11	h. m. s. 7 23 25·30	h. m. s. 7 30 8·47	h. m. s. 7 34 35·85	h. m. s. 9 18 59·40	h. m. s. 9 59 2·78	h. m. s. 11 40 7·80	h. m. s. 11 41 34·98	h. m. s. 13 15 59·22
Oct. 1	+ 2·91″	+ 3·87″	+ 3·15″	+ 3·65″	+ 2·43″	+ 2·52″	+ 2·08″	+ 2·22″	+ 2·22″
2	94	90	18	69	45	54	09	23	23
3	97	93	21	72	48	56	11	24	23
4	99	97	23	75	50	58	12	25	23
5	3·02	4·00	26	78	52	60	13	26	23
6	05	04	29	82	55	62	14	27	23
7	08	07	32	85	57	64	15	28	23
8	11	10	35	88	59	67	16	30	24
9	14	14	38	91	62	69	18	31	24
10	16	17	41	95	64	72	19	33	24
11	19	21	44	98	67	74	21	34	25
12	22	25	46	4·01	69	77	22	36	25
13	24	28	49	04	72	79	24	37	26
14	27	32	52	07	74	82	25	39	27
15	30	35	55	11	77	84	27	40	28
16	32	39	58	14	79	86	28	42	29
17	35	42	61	17	81	88	30	43	30
18	38	46	64	20	84	91	32	45	31
19	41	49	67	24	87	94	34	47	32
20	44	53	70	27	89	96	36	49	33
21	47	56	73	31	92	99	37	51	34
22	50	60	76	34	95	3·02	39	52	35
23	52	63	79	38	98	04	41	54	36
24	55	67	82	41	3·01	07	43	56	37
25	58	70	85	45	03	10	45	58	38
26	61	74	88	49	06	12	47	60	39
27	64	78	91	52	09	15	49	62	40
28	66	81	94	55	12	18	51	64	42
29	69	85	97	58	15	21	53	67	43
30	72	88	4·00	62	18	24	56	69	45
31	75	92	03	65	21	27	58	71	46
Nov. 1	77	95	05	69	24	30	60	74	48
2	80	99	08	72	27	32	63	76	49
3	83	5·02	11	76	30	35	65	78	51
4	85	06	14	79	33	38	67	81	52
5	88	09	17	82	36	41	70	83	54
6	91	12	20	85	39	44	72	85	55
7	94	15	23	88	42	47	75	87	57
8	97	19	26	92	45	50	78	90	59
9	99	22	29	95	48	53	80	92	61
10	4·02	26	32	99	51	56	83	95	63
11	05	29	34	4·02	55	59	86	97	65
12	07	33	37	06	58	62	88	3·00	67
13	10	36	40	09	61	65	91	02	69
14	12	40	43	13	64	68	93	05	71
15	15	43	46	16	67	71	96	08	73
16	18	47	49	19	70	74	99	11	75
17	20	50	52	22	73	77	3·02	14	78
18	23	53	55	25	76	81	05	17	80
19	25	57	58	29	80	84	08	20	83
20	27	60	61	32	83	88	11	23	85
21	30	63	64	35	86	91	14	25	87
22	32	67	66	38	89	95	17	28	90
23	34	70	69	42	92	98	20	31	92
24	37	74	72	45	96	4·01	23	34	95
25	39	77	75	48	99	05	26	37	97
26	42	80	78	51	4·02	08	29	40	99
27	44	83	81	54	05	11	32	43	3·02
28	46	86	83	57	08	14	35	46	04
29	48	89	86	60	11	18	38	49	07
30	50	92	88	63	14	21	41	52	09

Mean AR 1825.	Arcturus h. m. s. 14 7 41·06	2α Libræ h. m. s. 14 41 12·92	α Cor. Bor h. m. s. 15 27 16·97	α Serpent. h. m. s. 15 35 39·42	Antares h. m. s. 16 18 41·57	α Herculis h. m. s. 17 6 40·44	α Ophiuchi h. m. s. 17 26 49·02	α Lyræ h. m. s. 18 31 1·01	γ Aquilæ h. m. s. 19 37 56·55
Oct. 1	+ 1·75″	+ 2·55″	+ 1·69″	+ 2·33″	+ 3·24″	+ 2·49″	+ 2·65″	+ 2·19″	+ 3·44″
2	74	55	68	32	23	47	63	16	42
3	74	54	67	31	21	45	62	14	40
4	73	54	65	30	20	44	60	11	39
5	73	53	64	28	18	42	58	09	37
6	73	53	63	27	17	41	57	06	35
7	72	52	62	26	15	39	55	04	34
8	72	52	61	25	14	37	53	02	32
9	72	51	60	24	13	35	51	1·99	30
10	72	51	59	23	11	33	49	97	28
11	72	50	58	22	10	32	47	95	27
12	72	50	57	21	09	30	46	92	25
13	72	50	56	20	08	29	44	90	24
14	72	49	55	20	07	27	42	87	22
15	72	49	54	19	06	26	41	85	21
16	72	48	53	18	05	24	39	82	19
17	72	48	52	18	04	23	38	80	18
18	73	48	51	17	03	22	37	78	16
19	73	48	50	17	03	21	36	76	14
20	73	48	50	17	02	20	35	73	13
21	73	49	49	16	01	18	33	71	11
22	74	49	48	16	00	17	32	69	09
23	74	49	48	15	00	16	31	66	07
24	74	49	47	15	2·99	15	30	64	06
25	74	50	47	15	98	14	29	62	04
26	75	50	46	14	97	12	27	59	02
27	75	50	46	14	97	11	26	57	00
28	76	51	46	14	97	10	25	55	2·99
29	76	51	46	14	96	09	24	53	97
30	77	52	46	14	96	08	23	51	95
31	77	53	46	14	96	07	22	50	94
Nov. 1	78	53	46	14	96	06	21	48	92
2	78	54	46	14	96	06	20	46	91
3	79	55	46	14	95	05	19	44	89
4	80	56	46	14	95	04	18	42	88
5	81	57	46	14	95	03	17	40	86
6	83	58	46	14	95	02	16	38	85
7	84	59	46	14	95	02	15	36	83
8	85	61	47	15	95	01	14	34	82
9	87	62	47	15	95	01	13	32	81
10	88	64	47	16	96	01	12	30	79
11	90	65	48	16	96	00	12	28	78
12	91	67	48	17	96	00	11	27	77
13	93	68	49	17	96	00	11	25	76
14	95	69	49	18	97	00	11	24	74
15	96	70	50	18	97	1·99	10	22	73
16	97	71	51	19	97	99	10	21	72
17	99	73	52	20	98	99	10	20	71
18	2·01	74	53	21	99	99	10	19	70
19	03	76	54	22	3·00	99	10	18	69
20	05	77	55	24	00	99	10	16	68
21	07	79	56	25	01	99	10	15	67
22	09	80	57	27	02	99	10	14	66
23	11	82	58	28	03	2·00	10	13	65
24	13	84	59	29	04	00	10	11	64
25	15	86	61	30	05	00	10	10	63
26	17	88	62	31	06	00	10	09	62
27	19	90	63	32	07	01	11	08	61
28	21	92	65	34	08	01	11	07	60
29	23	94	66	35	10	01	11	06	60
30	25	96	67	37	11	02	12	05	59

Mean AR 1825.	α Aquilæ h. m. s. 19 42 14·84	β Aquilæ h. m. s. 19 46 43·20	2 α Capri. h. m. s. 20 8 20·34	α Cygni h. m. s. 20 35 28·24	α Aquarii h. m. s. 21 56 47·77	Fomalhaut h. m. s. 22 47 57·67	α Pegasi h. m. s. 22 56 3·16	α Androm. h. m. s. 23 59 21·74
Oct. 1	+ 3·53″	+ 3·59″	+ 4·10″	+ 3·15″	+ 4·29″	+ 4·80″	+ 4·41″	+ 4·79″
2	51	58	09	13	28	80	41	80
3	50	56	07	11	27	79	41	80
4	48	55	06	08	26	79	40	80
5	46	53	04	06	25	78	40	80
6	45	51	03	03	25	78	39	81
7	43	49	01	01	24	77	39	81
8	41	47	00	2·99	23	76	38	81
9	40	45	3·98	96	22	75	38	81
10	38	44	97	94	21	75	37	81
11	37	42	95	92	20	74	37	81
12	35	40	94	89	19	73	36	81
13	34	39	92	87	18	72	36	80
14	32	37	91	84	17	71	35	80
15	30	36	89	82	16	70	34	80
16	29	34	88	79	15	69	34	80
17	27	33	86	77	14	68	33	80
18	25	31	84	74	13	67	32	79
19	24	29	83	72	11	66	31	79
20	22	27	81	69	10	65	30	79
21	20	26	79	67	09	64	29	78
22	18	24	77	64	08	63	28	78
23	17	23	76	62	07	62	27	77
24	15	21	74	69	05	61	26	77
25	13	20	72	66	04	60	25	76
26	12	18	71	63	03	59	24	76
27	10	17	69	51	01	57	23	75
28	09	15	68	48	00	56	22	74
29	07	14	66	46	3·98	54	21	74
30	06	12	65	43	97	53	20	73
31	04	11	64	41	96	52	19	72
Nov. 1	03	09	62	38	95	51	18	71
2	02	08	61	36	93	50	17	71
3	00	06	59	33	92	48	16	70
4	2·99	05	58	30	91	47	15	69
5	97	03	56	28	89	46	14	68
6	96	02	55	25	88	44	13	67
7	95	01	54	23	87	42	12	66
8	94	00	52	20	85	41	10	65
9	93	2·98	51	18	84	39	09	64
10	91	97	50	16	82	38	08	63
11	89	95	49	14	81	36	07	63
12	88	94	47	11	79	35	05	62
13	86	92	46	09	78	33	04	61
14	85	91	45	07	76	32	03	60
15	83	89	43	04	75	30	01	59
16	82	88	42	02	74	29	00	58
17	81	87	41	00	73	27	3·98	57
18	80	86	40	1·98	72	26	97	56
19	79	85	39	95	71	24	96	55
20	78	84	38	93	69	23	95	54
21	77	83	37	91	68	21	94	53
22	77	82	36	89	67	20	93	52
23	76	81	35	87	66	18	92	51
24	75	81	34	85	64	17	90	50
25	74	80	33	82	63	15	89	48
26	73	79	32	80	62	14	88	47
27	72	78	31	78	61	12	87	46
28	71	77	30	76	60	11	85	44
29	71	77	29	74	58	09	84	43
30	70	76	28	71	57	08	83	41

Mean AR 1825.	γ Pegasi h. m. s. 0 4 14·25	Polaris h. m. s. 0 58 17·50	κ Arietis h. m. s. 1 57 19·77	α Ceti h. m. s. 2 53 8·58	Aldebaran h. m. s. 4 25 53·44	Capella h. m. s. 5 3 46·61	Rigel h. m. s. 5 6 8·00	β Tauri h. m. s. 5 15 14·29	α Orionis h. m. s. 5 45 42·18
Dec. 1	+ 4·34″	+ 51·77″	+ 5·30″	+ 5·05″	+ 5·66″	+ 7·44″	+ 4·90″	+ 6·21″	+ 5·31″
2	33	51·18	29	05	67	46	92	22	33
3	31	50·59	29	05	68	48	93	24	35
4	30	50·00	28	05	68	49	94	26	37
5	29	49·41	28	05	69	51	95	27	38
6	28	48·82	27	05	70	53	96	29	40
7	27	48·21	26	05	71	55	97	30	41
8	26	47·60	26	05	71	56	98	31	43
9	24	46·99	25	05	72	58	99	33	44
10	23	46·39	25	04	73	59	5·00	34	46
11	22	45·78	24	04	74	60	01	35	47
12	21	45·13	24	04	74	62	02	37	49
13	20	44·48	23	04	75	63	03	38	50
14	18	43·82	23	03	76	65	04	39	52
15	17	43·17	22	03	77	66	04	41	53
16	16	42·52	21	03	77	67	05	42	54
17	15	41·85	20	02	77	68	05	43	55
18	14	41·18	20	02	78	69	06	43	56
19	13	40·50	19	01	78	70	06	44	57
20	12	39·83	18	01	78	71	07	44	58
21	11	39·16	17	00	78	71	07	45	59
22	10	38·45	16	4·99	79	72	08	46	60
23	08	37·74	15	99	79	73	08	46	61
24	07	37·02	14	98	79	74	09	47	62
25	06	36·31	13	98	79	75	09	48	63
26	05	35·60	12	97	79	75	10	49	63
27	04	34·91	11	97	79	76	10	49	64
28	03	34·21	11	96	79	77	10	50	65
29	02	33·52	10	95	79	78	11	51	65
30	01	32·82	09	95	79	79	11	52	66
31	3·99	32·13	08	94	79	80	11	52	67

Mean AR 1825.	Sirius h. m. s. 6 37 26·11	Castor h. m. s. 7 28 25·30	Procyon h. m. s. 7 30 8·47	Pollux h. m. s. 7 34 35·85	α Hydræ h. m. s. 9 18 59·40	Regulus h. m. s. 9 59 2·78	β Leonis h. m. s. 11 40 7·80	β Virginis h. m. s. 11 41 34·98	Spica Virg. h. m. s. 13 15 59·22
Dec. 1	+ 4·52″	+ 5·95″	+ 4·91″	+ 4·66″	+ 4·17″	+ 4·24″	+ 3·44″	+ 3·56″	+ 3·12″
2	53	97	93	68	20	27	47	59	15
3	55	6·00	96	71	23	30	50	62	17
4	57	03	98	74	26	34	53	65	20
5	59	06	5·01	77	29	37	56	68	22
6	61	09	03	80	32	40	59	71	25
7	63	11	05	83	35	43	62	74	28
8	65	14	07	86	38	46	66	77	31
9	66	16	10	88	40	49	69	81	34
10	68	19	12	91	43	53	73	84	37
11	70	21	14	94	46	56	76	87	40
12	71	24	17	96	49	59	79	91	43
13	73	26	19	99	51	62	83	94	46
14	75	29	21	5·02	54	66	86	97	49
15	77	32	24	05	57	69	89	4·01	52
16	79	35	26	07	60	72	93	04	55
17	82	37	28	09	63	75	96	07	58
18	84	39	30	11	65	78	99	10	61
19	86	42	32	14	68	81	4·03	13	64
20	89	44	34	16	71	84	06	16	67
21	91	46	35	18	74	87	10	20	70
22	93	48	37	20	76	90	13	23	73
23	95	51	39	23	79	93	16	26	76
24	98	53	41	25	82	96	20	29	80
25	5·00	55	42	27	84	99	23	32	83
26	02	57	44	29	87	5·02	26	35	86
27	03	59	45	31	90	04	29	38	89
28	05	61	47	33	93	07	32	41	93
29	06	62	49	35	95	10	35	45	96
30	07	64	51	36	98	13	38	48	4·00
31	08	66	53	38	5·00	16	41	51	03

Mean AR 1825.	Arcturus h. m. s. 14 7 41·06	2 α Libræ h. m. s. 14 41 12·92	α Cor. Bor. h. m. s. 15 27 16·97	α Serpent. h. m. s. 15 35 39·42	Antares h. m. s. 16 18 41·57	α Herculis h. m. s. 17 6 40·44	α Ophiuchi h. m. s. 17 26 49·02	α Lyræ h. m. s. 18 31 1·01	γ Aquilæ h. m. s. 19 37 56·55
Dec. 1	+ 2·28″	+ 2·98″	+ 1·69″	+ 2·38″	+ 3·13″	+ 2·02″	+ 2·12″	+ 2·04″	+ 3·58″
2	30	3·00	70	39	14	03	12	03	58
3	32	02	72	41	15	03	12	02	57
4	34	04	73	43	17	04	13	01	57
5	36	06	75	44	18	04	13	00	56
6	38	08	76	45	19	05	13	00	55
7	41	10	78	47	21	06	14	00	55
8	44	13	80	49	23	07	15	00	55
9	47	15	82	51	25	08	15	1·99	55
10	50	18	84	53	27	10	16	99	54
11	52	20	86	55	29	11	17	99	54
12	55	23	88	57	30	12	18	99	54
13	58	25	90	59	32	13	19	98	53
14	61	28	92	61	34	14	20	98	53
15	63	30	94	63	35	15	21	98	53
16	65	33	96	65	37	16	21	98	52
17	68	36	98	67	39	17	22	98	52
18	71	39	2·00	70	41	19	24	98	52
19	74	42	03	72	44	20	25	98	52
20	77	45	05	74	46	22	26	98	52
21	79	47	07	77	48	23	28	98	52
22	82	50	10	79	51	25	29	99	52
23	85	53	12	82	53	26	31	99	52
24	88	56	15	84	56	28	32	99	52
25	91	59	17	86	58	30	34	99	52
26	94	62	20	88	60	31	35	99	52
27	97	65	22	88	63	33	36	2·00	52
28	3·00	68	25	89	66	35	38	00	53
29	03	71	28	89	68	36	39	00	53
30	06	74	30	3·00	70	38	41	01	54
31	09	77	33	00	73	39	43	01	54

Mean AR 1825.	α Aquilæ h. m. s. 19 42 14·84	β Aquilæ h. m. s. 19 46 43·20	2 α Capricor h. m. s. 20 8 20·34	α Cygni h. m. s. 20 35 28·24	α Aquarii h. m. s. 21 56 47·77	Fomalhaut h. m. s. 22 47 57·67	α Pegasi h. m. s. 22 56 3·16	α Androm. h. m. s. 23 59 21·74
Dec. 1	+ 2·69″	+ 2·75″	+ 3·27″	+ 1·69″	+ 3·56″	+ 4·06″	+ 3·82″	+ 4·40″
2	68	74	26	67	55	05	80	39
3	67	73	25	65	54	03	79	37
4	67	72	24	63	52	02	78	36
5	66	71	24	61	51	00	76	35
6	65	71	23	59	50	3·99	75	34
7	65	70	23	57	49	98	74	33
8	65	70	22	56	48	96	73	31
9	65	70	22	54	47	95	71	30
10	64	69	22	53	46	94	70	29
11	64	69	22	51	45	93	69	28
12	64	69	21	50	45	91	68	27
13	64	69	21	48	44	90	67	25
14	63	68	21	47	43	89	65	24
15	63	68	20	45	42	87	64	23
16	62	68	20	43	41	86	63	21
17	62	68	20	42	40	85	62	20
18	62	68	20	41	39	83	61	18
19	62	68	19	40	38	82	60	17
20	62	68	19	39	37	81	59	16
21	62	68	19	38	36	80	58	14
22	62	68	19	36	36	78	57	13
23	62	68	19	35	35	77	56	11
24	62	68	18	34	34	76	54	10
25	62	68	18	33	33	75	53	08
26	62	68	18	32	32	73	52	07
27	62	68	18	31	31	72	51	05
28	63	69	18	30	31	71	50	04
29	63	69	18	29	30	70	49	02
30	63	69	19	28	30	68	48	01
31	64	69	19	27	29	67	47	00

Article III.

*Analysis of an Alloy of Gold and Rhodium from the Parting House, at Mexico.** By M. André Del Rio, Professor at the College of Mines, and Member of the Institution of Sciences at Mexico.†

In the year 1810, M. Cloud, Chemical Director at the Mint in Philadelphia, discovered an alloy of gold and palladium in two ingots of gold from Brazil. I have found another alloy here, containing rhodium, which, as yet, is unknown in Europe. At this favourable era we may expect an infinite number of discoveries, as the careful examination of a country so vast and richly endowed by nature as this is, proceeds. I am surprised that M. Cloud has not given in his analysis, either the specific gravity of his alloy, nor the proportions of the metals of which it is composed.

1. 199·2 grains of an alloy of gold, of this Parting House, of the specific gravity, according to Citizen Jean Mendez, of 15·4, left, after the action of aqua regia 1·28 of chloride of silver, = 0·97 silver. The gold of one-fourth part of the solution was separated by ether, when a galvanic current was observed, which occasioned the ether sometimes to swim on the surface of the aqua regia, as it naturally would do, and at others to assume a position below it; a phenomenon deserving closer investigation. Convinced that the gold was still alloyed with some other metal, a brittle button was obtained by means of borax which weighed 45·5 grains. This lost no weight by being boiled with nitric acid; when fused with nitre in a small platina crucible, a large quantity of the gold attached itself to the platina, and, besides, an efflorescence, formed of very small grains of a tin-white colour. The whole was treated with hot water, which being decanted off, and the residuum washed, a very heavy black powder remained, partly composed of very short and thin needles, and also another lighter powder of an olive-green colour. The filtered solution was yellow and blackened the filter, but on dying the colour became a clear olive-green. The yellow solution, saturated with nitric acid, left a cherry-coloured deposit, and gave with tincture of galls a dark yellowish-brown precipitate; a proof that it was not *osmium*.

The black powder being separated from the gold by quick-

* From the Annales de Chimie.

† This Memoir was communicated to M. de Humboldt, by M. Lucas Alaman, whose singular merit is duly appreciated by the *savans* of Europe, and who is at present Minister of the Interior of the Confederation of the Mexican States. M. del Rio, who studied at Paris, Freiberg, and Schemnitz, is well known by his labours in analytical chemistry, and by his Treatise on Mineralogy and Geognostic Tables.—*Note by the French Editor.*

silver, was dissolved in muriatic acid, and the solution assumed by boiling a fine orange colour. Citizen Mendez was unable to reduce the green powder before the blowpipe, but he observed that some specks detonated like nitre; a property of rhodium. This green powder, which I wished to treat separately, by transferring it with water from a capsule to a matrass, became black a second time, and I added it to the red solution, and boiled the whole afresh; although it appeared brown whilst hot it resumed its red colour on cooling. Sal ammoniac threw down from the concentrated solution, an orange-coloured powder, which, separated by decantation and sufficiently washed, was soluble in cold water, and still more so in hot, and gave by careful evaporation an infinite number of orange-coloured crystals. Being reduced alone, in the same crucible that was employed before, the gold was covered with a tin-white, blistered coating of rhodium.* By twice boiling potash and nitre in the same crucible, with the addition of water, the whole of the gold was exposed, and the lixivium saturated with sulphuric acid, gave 16 grains of the dark reddish brown deutoxide of rhodium, from which deducting 2·14 for oxygen, there remain 13·86 grains of rhodium for the 45·5 grains of alloy employed, or 30·4 per cent. I must not omit to state that the potash made very small holes in the crucible, and 131 grains of protoxide of platina were extracted from it, which were reduced in another crucible of earthenware, as perfectly as the nature of the crucible allowed.

The black powder was insoluble both in muriatic acid, and aqua regia; solution of potash dissolved it in part, and the residium gave many white metallic points by being heated with tallow in a small crucible. As all the points were not equally brilliant, the weight of the residuum was not added to the former product; I afterwards reflected that I had not employed sufficient heat, and that I ought to have added it just the same as that which was dissolved by the potash.

2. The greater part of the acid of the remaining three-fourths of the solution was distilled off, and the rest saturated with ammonia, not added in excess. The orange-coloured precipitate lost its red tint, and acquired a slight yellow ochre hue; wherefore, and because it became greenish by washing with warm water, I concluded, although it detonated like fulminating gold, that it was not absolutely pure. In fact, 10 grains mixed with oil, and fused with borax, gave a little white button † which was too brittle for pure gold, and internally had a whitish

* It was afterwards discovered that the blistered appearance was owing to the gold, as if it had endeavoured to quit the platina to combine with the rhodium. The latter always remained white—was that owing to the platina? I believe it to be eminently galvanic.

† This tendency of rhodium to cover the surface of the gold is very singular.

copper, or nickel colour; the flux left a glass, partly of a leek green, and partly of a cochineal red colour, and the little button weighed 5·9 grains. Its white surface soon changed to a tombac brown, and when fused with nitre it yielded a little button of pure gold, weighing 4·3 grains.* Other 10 grains, similarly fused, gave also a brittle button, which left a scoria of a brighter red colour, weighing 7·1 grains, and this by a second fusion, as before, gave a button of pure gold, which also weighed 4·3 grains. Thus, as 100 parts of ammoniuret of gold gave 43 parts of gold, 212 parts, the weight of the whole precipitate, would have given 91·16. Three-fourths of the solution must have contained 149·40 of the alloy, or deducting three-fourths of a grain of silver, 148·65.

Deducting the gold, there remain 57·49 parts of rhodium, or 38·6 per cent. of the alloy. The specific gravity of this alloy should be 15·91, but as it was actually only 15·4 it must have suffered expansion.

The remainder of the solution was distilled to dryness, and a dark brown residuum was obtained, which neither gave the changes of colour that characterise iridium by the action of muriatic acid, nor red crystals with ammonia; but it afforded an uncrystallizable double salt, of a flesh red colour, which, when dried, resembled discoloured, brown, frothy iron (*fer écumeux*). Citizen Mendez could not reduce it before the blowpipe, and obtained with borax only a yellowish green glass.

Thus we see that neither ether nor ammonia afford the most certain nor the most easy methods of separating rhodium. From this consideration I had recourse to what Dr. Wollaston says on the subject, that it is not capable of forming an amalgam with mercury.

3. For this purpose, Citizen Mendez cupelled, at two operations, a very brittle alloy of gold with rhodium and copper, weighing 133·7 grains, which gave him two little globules, one of which weighed only 53·87 grains, and the other 66·13. I treated the first with mercury and boiling water, and again by trituration in an iron mortar, when the whole amalgamated, except 2·5 grains of a bright olive green powder, which afterwards darkened in water, and became greenish black. Can the green powder be a deutoxide, intermediate between the black and the reddish brown, or puce coloured oxide, and the greenish black powder a hydrate? I have the highest esteem for M. Berzelius, but he himself desires that truth should be freely sought. As to the small button of alloy, however carefully I washed its amalgam, it always showed a black spot of rhodium at the bottom (*dans le fond*) after having been heated to redness. After fusion with nitre it weighed 49·7 grains, and its specific

* On this account I lined the bottom and sides of the crucible with an excess of borax, which has great tendency to vitrify rhodium.

gravity, according to Citizen Mendez, was 15. I suppose its specific gravity was diminished by the three-eighths of a grain of silver which it contained; but it is, at all events, clearly demonstrated that rhodium easily amalgamates with mercury, by the intervention of gold, although it will not do so alone. I must confess I was much deceived as to my principal object of knowing if this alloy be obtained at the Parting House from the fused silver ingots, or from the amalgamated silver, which would not a little have assisted us in ascertaining its local situations, and the ores from which it is derived, circumstances which at present we are ignorant of.

4. Having found by experiment that neither protosulphate of iron, nor oxalic acid, precipitate rhodium, I dissolved the button weighing 66·13 grains (whose specific gravity according to Citizen Mendez was 15·48) in aqua regia. The button when beat out under the hammer, for want of a flatting mill, presented tin-white spots on the yellow ground, showing that the alloy was not uniform; the chloride of silver which precipitated from the solution, contained half a grain of silver, which gives more than three quarters of a grain per cent. This clearly shows how necessary it is to use sulphuric acid to extract all the silver at the Parting House. I meant to take only one half of this solution, but for want of graduated tubes I took more, and precipitated it with protosulphate of iron; the reduced button weighed 30·7 grains, and its specific gravity was 19·07. On adding the protosulphate to the solution, it became as black as ink, and red by refraction in the sun, with much effervescence and disengagement of deutoxide of azote; but as soon as that ceased it resumed its former transparence. Having distilled it to dryness, to expel the nitric and muriatic acids, and added water, a large quantity of subsulphate of iron remained undissolved; by the addition of a little sulphuric acid and boiling the liquid, the whole dissolved, and the solution assumed a pale bright flesh colour; I then immersed in it a plate of iron, which became copper red, but by washing in distilled water, the coloured coating appeared very slight, and a very fetid odour was exhaled, which I know not how to describe. I have perceived the same odour on adding water to the alloy fused with potash. After filtering and washing, slightly flexible pellicles remained, which, when dry, had the colour of tombac, and weighed 10·6 grains. On attempting to reduce them entirely with borax (for, according to Thomson's Chemistry, even the protoxide may be reduced), I obtained merely a green glass. Thus, considering them as metallic (for if they were not absolutely, they were very nearly so) we shall have 25·4 per cent. of rhodium in the alloy, without reckoning what still remained alloyed with the gold. I had taken for that the lower half of the solution which had been left at rest for a long time. Could

it have contained a larger quantity of gold? I should be the more inclined to think so, because, from the fourth part of the same solution, precipitated by ammonia, which should have contained 16·5 grains of alloy, I obtained only 9·3 of gold; which gives 43 per cent. of rhodium in the alloy. The remaining solution had the colour of protosulphate of iron.

5. In this state of things, Citizen Mendez conceived the idea of adding sulphuric acid to a solution of 10 grains of another alloy (specific gravity = 16·8) in aqua regia, and distilling it to dryness. When all the muriatic acid had come over, and the liquid in the retort had assumed a very deep red colour, the receiver was changed; a yellow matter rose with the acid, and the gold remaining in the retort, had the appearance of aurum musivum. The yellow matter partly dissolved in water, colouring it first yellow, afterwards green, and a sub-trito-sulphate of rhodium, of a yellow ochre colour, deposited. On pouring water into the retort, a similar deposit was obtained, which was separated by decantation; the gold was then twice fused with potash and nitre; after the first fusion it left a very dark leek-green glass, and a brighter one after the second; so that it would be necessary to repeat the operation several times to obtain the gold perfectly pure; it weighed in the state in which we left it, 8·2 grains. We see, therefore, from what has been said, that rhodium alloys with gold in different proportions, the mean, according to what I have observed, being 34 of rhodium in 100 parts of the alloy; or more than one third.

I am sorry to be compelled to say that Dr. Wollaston is mistaken, in stating that the alloys of gold with rhodium are very ductile. The contrary has long been observed at this Parting House, and was attributed to the *sharpness of the acids* (*âcreté des acides*), as if we employed more than one, and they were very volatile, and very easily decomposed. We can now conceive that a brittle metal in so large a quantity must necessarily render the alloys it forms brittle also.

I imagine our practical men will not any longer assert, that *with a cupel, and two or three acids, any fraudulent mixture may be detected in gold*, now that they have this new instance of rhodium, in addition to those known before, of platina and palladium, and I hope that iridium will also some day play its part. As to the enormous losses at the Parting House, I understand that in former times long reports have been made concerning them, but experiment is the right method of discovering physical truths. The reports are like Spartan money, far greater in volume than in value.

I do not believe that the complete separation of rhodium can be effected by softening (*adoucissant*) the alloy with corrosive sublimate, although this method be more chemical than that of washing the *marquetas** of silver with soap and rags, as is done at Tasco, in order to remove the black powder, or oxide of another metal, which has much resemblance to selenium; at least Citizen Mendez and I have found, amongst the Tasco minerals, *biseleniuret of silver*, in small hexagonal tables, with rounded edges and angles, as if they had been fused; their colour was lead grey, they were very ductile, as may be seen in our journal, *the Sun*, No. 102, September 24, 1823. The object would be better attained by treating the alloy with sulphuret of antimony, on account of the greater affinity of rhodium for sulphur, and that is certainly the mode adopted by a certain person, who purified a quantity last year worth 1800 piastres, making a great secret of his process, as if this were the age of mystery, or the Mexicans resembled the inhabitants of Otaheite.

The preceding analysis only too plainly shows the wretched state of our laboratory in Mexico, after having been for thirty years under the direction of so distinguished a chemist as M. Elhuyar, the discoverer of Wolfram and Cerium. It is true that under the old government, this savant found himself obliged to become a man of business, undoubtedly much against his inclination; for it is impossible, that he who has once imbibed a taste for science can ever abandon it.

Mexico, Dec. 9, 1824.

We have translated the preceding paper almost verbatim from the article in the June number of the Annales de Chimie, for the present year. It is not, in some places, wholly free from obscurity, but whether the fault lie with the author or the French translator we cannot determine. We have given it, to the best of our ability, faithfully as we have found it. C.

Article IV.

Astronomical Observations, 1825.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Sidereal Time.

1825.	Stars.	Transits.
Aug. 22.	—*b* Ophiu.	17h 15′ 45·89″
22.	—Moon's First or West Limb	17 29 16·39

* The cakes, or balls of amalgam, after the mercury has been driven off by distillation.

1825.	Stars.	Transits.		
Aug. 22.	—283 Sagitt.	17h	46′	30·55″
22.	—4 Sagitt.	17	49	11·22
22.	—7 Sagitt.	17	52	12·66
22.	—386 Sagitt.	18	01	07·37
23.	—α Ophiu.	17	26	52·35
23.	—μ^1 Sagitt.	18	03	22·60
23.	—21 Sagitt.	18	15	00·11
23.	—Moon's First or West Limb	18	27	48·68
23.	—28 Sagitt.	18	35	51·98
23.	—30 Sagitt.	18	40	23·74
23.	—ν^1 Sagitt.	18	43	40·68
23.	—ν^2 Sagitt.	18	44	37·38
23.	—ξ^1 Sagitt.	18	47	01·10
23.	—o Sagitt.	18	54	15·95
23.	—π Sagitt.	18	59	25·71
24.	—α Ophiu.	17	26	52·33
24.	—d Sagitt	19	07	28·32
24.	—g^1 Sagitt.	19	11	35·82
24.	—g^2 Sagitt.	19	20	35·05
24.	—Moon's First or West Limb	19	23	45·15
24.	—ϵ^1 Sagitt.	19	30	46·14
24.	—ϵ^2 Sagitt.	19	32	34·84
24.	—f Sagitt.	19	36	13·49
24.	—57 Sagitt.	19	42	06·17
24.	—g Sagitt.	19	48	05·70
25.	—g Sagitt.	19	48	05·53
25.	—381 Sagitt.	19	55	28·73
25.	—a^2 Capric.	20	08	01·29
25.	—β^2 Capric.	20	11	15·13
25.	—Moon's First or West Limb	20	16	47·61
25.	—172 Capric.	20	22	41·06
25.	—194 Capric.	20	25	42·09
25.	—13 Capric.	20	27	36·68
25.	—τ Capric.	20	29	33·45
25.	—325 Capric.	20	41	07·15
30.	—μ Piscium	23	50	24·45
30.	—35 Piscium	00	06	02·96
30.	—Moon's Second or East Limb	00	14	07·42
30.	—45 Piscium.	00	16	45·52
30.	—51 Piscium	00	23	26·84
30.	—δ Piscium	00	39	41·22

Occultations of Stars by the Moon.

Sept. 4.	Immersion of 2 κ Taurus	0h	17′	19·1″	Sidereal Time.
	Immersion of 1 κ Taurus	0	32	39·1	

N. B. The Immersion of *b* Sagittarius, inserted in the *Annals* for last month, occurred on the 26th of July.

Article V.

On the Seat of Vision. By Mr. J. Wallan.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Birmingham, Sept.* 14, 1825.

The following appearances being quite new to myself, I communicate them for insertion in the *Annals*, that, provided the observations of others are found to correspond with my own, they may be placed to their proper account.

The moon being about 22 degrees above the western horizon, having a small plano-convex lens in my hand, which was applied close to my right eye, on looking at that body I perceived a faint nebulous circle of light, and within the margin of this, and almost on the right edge of it, a perfect image of the moon, whose apparent size was somewhat less than the moon itself, viewed with the naked eye. The light of this perfect image was only so much greater than the circle, as to enable me to distinguish it for a perfect picture of the planet. On applying the glass to my left eye the effect was precisely the same, except that the place of the perfect picture of the moon varied a little in this instance from its situation in the former, being a trifling degree nearer to the centre of the nebulous light.

Although it might be deemed most prudent not to attempt at accounting for this appearance until it had been more scrutanized, and the effect correctly verified by a number of individuals, I cannot help concluding at once, from this slight difference in the observations made with each of my own eyes, that the perfect picture was reflected from the base of the optic nerve, and the circular image from the retina; and I draw this conclusion from a knowledge that there is a trifling variation in the axes of my own eyes, although not apparent to any one except myself; as well as that there is in each a difference in their capacity for distinct vision,—this being, in fact, more or less the case with every one.

Having observed the same effect on several successive nights, and during two sucessive lunations, it does not appear by any means probable that there was the least illusion in this appearance more than would take place with other spectators similarly situated; and the seat of vision being a point still under the ban of dispute with philosophers, I make no apology for the inference now drawn, but will zealously expunge it from my catalogue of maxims, if the reverse can still be proved to be true by direct experiment.

As I have proceeded thus far with these remarks, it may be considered as equivalent to a retreat from the inquiry to relinquish it in its present stage; I shall, therefore, add a few other facts in support of the base of the optic nerve being the chief agent in our particular sensations,—still leaving the correct solution of these matters, however, open to further inquiry.

In every act of vision it may be proved by observation, that whether we employ one only, or both, of our eyes in conjunction, while looking towards any object, there is a *particular*, as well as a *general* sensation, both of which are occasioned by the constitution of our organs of vision. The particular sensation is this, that the eye discovers *distinctly* but a single point only in the act of employing it; this point being comparatively small or great, according as the object contemplated is near the eye, or remotely distant from it. The general sensation is this, that the eye indistinctly discovers a very considerable space, which is greater or less according to the peculiarities of its constitution, or the quantity of light to which it is exposed; that is, according to the convexity of the cornea, and the dilatation and contraction of the pupil. The particular sensation, it will be obvious, is here referred to the base of the optic nerve, and the general sensation to the retina, resting upon the internal coat of the eye.

Here, however, it will be necessary to state other reasons for these conclusions, than those reflections from the interior of the eye upon the lens already alluded to in observing the moon, which, notwithstanding, are of themselves sufficiently obvious; but it may, first of all, be proper to show in what manner it can be proved that there *is* a particular, as well as a general sensation,—a circumstance, although extremely simple in itself, yet, never before mentioned, that I am aware of, by any writer. This may be done by making a minute centre, and describing a few circles round it, with radii of any number of parts of an inch. With a small test of this kind it will be found that at the distance of between three and six inches, according to circumstances, the central point only of this diagram can be distinctly seen at the same instant, and that the eye may be directed round the smallest of the circles without receiving a particular sensation of the centre. The same thing may be observed, however, in reading, when it will be found that we can distinctly trace but one letter at a time, and scarcely this, and that it is only by directing the eye upon each letter in succession that we are enabled to comprehend the sense, except so far as we are otherwise assisted by memory, in ascertaining the whole from a part.

I now proceed to offer a few remarks respecting the probable *cause* of a particular and a general sensation arising on directing the eye towards any object; for which purpose I shall first allude to a circumstance towards which my attention has been frequently directed, and afterwards towards one which was taken notice of in the last century by a number of philosophers, and which has generally been referred to an insensibility in the base of the optic nerve.

I have frequently had occasion to remark that objects are perfectly seen only, and most distinctly observed by the right eye on the left side of the field of view, and by the left eye on the right side of it.* I take it for granted, in the first place, that there is a *cause* existing *within* the eye why this should be the case, and the thing to be ascertained is, where the real seat of this cause is situated. Now, if the cause of our particular sensations were in the axis of the eye, every person would appear to squint excessively when looking with both his eyes at any object placed but a trifling way beyond the point of distinct vision, which is not the case; nor would the right eye then perfectly discover a point at the utmost limit towards the left hand, because it could not be strained so far as that its axis should form so great an angle with a perpendicular upon a right line drawn through the centres of *both* the eyes. This much the right eye can do however towards the left hand, but not towards the right, as well as that the left eye can do the same thing towards the right hand, but not towards the left. If, therefore, this is not performed by directing the *axis* of the eye towards the point observed, it must be done by placing the eye in some other position which is capable of producing the effect.

Leaving this argument as it stands, and referring to that experiment alluded to by which one of any three dark spots is lost on directing one eye towards that formed on either side of the centre, having the objects placed at an appropriate distance, it will be found that the right hand object of the three is lost by the right eye, and the one on the left hand by the left. From this circumstance, it has been concluded that the base of the optic nerve is insensible, because the undiscovered spot is exactly opposite the place at which the nerve is inserted in the eye; but the real cause is because the sensibility of the optic nerve is already occupied in conveying a particular sensation of the spot towards which the eye is directed, and which falls under the opposite angle, with the axis of the eye to that formed by the spot which is lost.

Here, however, there is one great peculiarity to be taken notice of, which, although remarkable, is an additional proof of the truth of these observations. The perfect picture of the moon, observed by directing the eye upon the plano-convex lens was found to be situated exactly in the place where one of the three dark spots mentioned above is lost. But this picture of the moon is reflected *from* the eye upon the lens, while that one of the dark spots which is lost is *opposed* to the eye under the same angle with the axis under which this picture is trans-

* See Dr. Wollaston's paper on the semi-decussation of the optic nerve, in the Philosophical Transactions.—*Ed.*

mitted. The effects are therefore the same exactly in their tendency, and have every appearance of arising from the same cause similarly situated; namely, the base of the optic nerve, which is inserted at the back of the eye under the angle in question.

Although the whole of these remarks are stated as conclusive evidences of facts, it is by no means intended that no possible objection may be urged against the propriety of them, and I am perhaps the more readily induced to make this concession from a knowledge of other peculiar appearances, and which were noticed at the same time that those were observed which have been hitherto the subject of consideration.

While paying attention to the effect produced by directing my eye towards the moon, on removing the lens from one eye to the other I many times found a dark spot, sometimes surrounded with colour, frequently situated near the centre of the nebulous light, but often in other places; and nothing except the eccentricity of the situations of these spots would have prevented me from placing the spots observed upon the sun's disc by astronomers, to the account of the same illusion of vision. This eccentricity being however considerable, I cannot wholly permit myself to ascribe to a similar deception the spots observed upon the sun, from which its whole phenomena of rotation, and the position of its axis, &c. with regard to the ecliptic has been inferred; and yet it behoves us to be extremely circumspect in making our deductions in cases where the subjects are so continually prolific of deceptions. At the same time, however, that this apology for our imperfections is given, I cannot, at present, conceive it possible that any effects, actually travelling from the substance of a luminous body like the sun, can be visible to us. So far as the moon is concerned, in which the same forms are always recognised in the same relative places, the probabilities of truth are very different, besides that the characters of the two bodies differ essentially from each other; and it certainly appears evident that nothing less, either than an interposing body between the earth and the sun, or, what is highly probable, an illusion of vision similar to the one above-mentioned, could produce the effect of a dark spot on the face of the body in question. If we even admit that these spots are openings in the sun's atmosphere, as some have imagined, or that they are chasms in its body, how great must be the extent of these chasms or openings to allow of our discovering them at distances so immense? Leaving gratuitous assumptions out of the question altogether, however, it will appear evident, on an unprejudiced consideration of the matter, that we can no more discover the surface of the sun with the best instruments than we can discover with the naked eye, the centre of this earth by means of a pit, whose depth equals its semi-diameter. J. W.

Article VI.

Summary View of the Application of the Electro-chemical Theory to Chemical Phænomena. By M. Ferrè.*

Researches on the chemical influence of electricity have multiplied the facts which tend to prove that it is an essential agent in the combination of bodies, and many celebrated philosophers have endeavoured to show by ingenious speculations that it is the only source of chemical action. To prove this, however, we should inquire, if, on applying these ideas to known chemical phenomena, the results accord with the data derived from observation. This is the object of the present article, but it is first necessary to recapitulate briefly, the fundamental notions which properly constitute the electro-chemical theory.

According to this theory the particles of two elements which enter into chemical union are endowed with opposite electricities, by whose mutual attractions their combination is effected. If the intensities of the two electricities be equal, the compound will be neutral; if unequal, the compound will be acid, if the negative electricity be in excess, and alkaline if the positive. But admitting that electrical attractions are the source of chemical combination, we must suppose, for the union of the particles to be permanent, that they retain their respective electricities after combination, and consequently that the electricities cannot quit the particles to unite with, and neutralize each other; otherwise we cannot imagine, how the particles can quit one combination to enter into another, since, by losing their electrical state in the first, they must also lose, at the same time, all tendency to contract a second.

Action of Acids and Alkalies on Water.

As water combines with equal facility with both acids and alkalies, bodies which are endowed with opposite electrical properties, and which water, in whatever quantity, never neutralizes, it is evidently a neutral compound, and consequently can have no inherent tendency to unite with other bodies. However, since it actually does combine with a great number it must assume by their action sometimes a positive, and sometimes a negative state, as otherwise if electricity be the cause of affinity, it could not enter into chemical union with them at all. Since its integrant molecules exhibit no electricity, it can only be derived from the elementary particles, on whose opposite electricities the action of bodies dissolved by the water must be exerted. Thus the negative electricity of acids must attract the

* Abridged from the Annales de Chimie, xxviii. p. 417.

positive electricity of the hydrogen, and repel and consequently liberate a portion of the negative electricity of the oxygen, and hence the latter acquires a tendency to enter into fresh combinations, and is more readily detached from the hydrogen by other bodies having an affinity for it. Thus many metals which cannot alone take oxygen from water, decompose it rapidly by the intervention of weak acids.

Alkalies, on the contrary, being positively electrified, attract the oxygen of water, and repel its hydrogen, during which a part of the electricity of the latter is set free, and it thus acquires a tendency to abandon the former. In this manner the hydrogen of water combines with chlorine and iodine, which alone could not take it from its oxygen, at least whilst all the physical conditions remain unaltered.

The prevailing chemical theory is wholly insufficient to explain rationally the influence of acids and alkalies in promoting the decomposition of water in the cases above-mentioned. It would be erroneous to attribute it to their affinity for the oxides or acids about to be formed, for if we suppose the force which tends to unite them to be inherent in the molecules, we must admit that it can exert itself only when they are formed, unless they be simple. This cause, therefore, can have no effect till that which is attributed to it has already been produced. It is, in like manner, by diminishing the reciprocal action of the elements of water by the attraction of one of them, and the repulsion of the other, and thus setting free a part of their concealed electricity, that acids and alkalies facilitate the decomposing powers of the voltaic apparatus; and hence also the rapidity of its action is increased in proportion to their energy. Salts dissolved in water, acting, as we shall presently see, as acids or alkalies, produce analogous effects.

Reciprocal Action of Acids and Oxides.

When an acid combines with an oxide, if the free antagonist electricities of their molecules be capable of mutually balancing each other, it is evident that no change can ensue in the union of their respective elements. But if that of one of them, the positive electricity of the oxide, for example, be comparatively feeble, the acid, from its predominating negative electricity, causes its developement by attracting that element which is endowed with it, and repelling the other; and this influence may go so far as to determine their partial separation. Thus many oxides are reduced to a lower state of oxidation by the action of acids; for instance, the deutoxide of barium is reduced to the state of protoxide by the action of muriatic acid.

The alkalies, on the contrary, by their action on certain oxides which perform the part of acids with respect to them, separate a portion of the metal, and raise the remainder to a higher degree of oxidation. Thus potash causes protoxide of tin to pass to the state of deutoxide. In other cases, when the alkalies merely liberate a portion of their positive electricity by repulsion, they render them capable of absorbing a new quantity of oxygen; such seems to be their action on the peroxide of manganese. What has been said above of the influence of acids and alkalies on the decomposition of water is applicable to this partial decomposition of metallic oxides by their agency. This phenomenon appears equally inexplicable by ascribing it to a play of affinities, but it teaches us why metals cannot combine with acids, unless they be in the state of oxides. Since an acid, whose free electricity predominates greatly over that of an oxide, cannot unite with it without eliminating a portion of its oxygen, so much the rather must a metal whose molecules possess the full energy of their own electricity, when acting on an acid, tend to separate its elements rather than combine with it direct, as we often experience. Hence the necessity that its positive electricity should be partly neutralized by the negative electricity of the oxygen before the combination can take effect.

In the same way, a fact somewhat embarrassing to the electro-chemical theory may be easily explained; namely, why the capacity of saturation of oxides increases with the proportion of oxygen. Their positive electricity being in the inverse ratio of the proportion of that principle, we might suppose that the quantity of acid necessary to saturate them should follow the same ratio, but we find that as the proportion of oxygen in the oxides increases, its negative electricity destroys, by repulsion, a proportionately greater part of that of the acid, whose quantity, for an equal quantity of metal, must thus increase in the ratio of the increase of that principle.

On the Mutual Action of Salts by the Intervention of Water.

All salts are not soluble in water. An indispensable condition of solubility is their non-neutrality, for the one being a neutral compound, another equally neutral can have no action on it. But it is not equally true that we are to conclude that because a salt is insoluble, it must, therefore, be neutral, for the cohesion of its molecules may render it insoluble, although it be not neutral.

When one of these compounds, therefore, dissolves in water, it acts as an acid or an alkali by an excess of positive or negative electricity, and consequently attracts one of its elements and repels the other. In this way we may account for a fact, not hitherto satisfactorily explained by any theory, namely, the mutual decomposition of salts by the intervention of water. When two saline solutions are mixed together, the element, whose repulsion has liberated a part of the electricity in the dissolving liquid of one of them, must determine to that endowed with a contrary electricity in the dissolving liquid of the other. The two constituent molecules of the water becoming thus liberated, act in each solution at the instant they separate in an opposite manner on the elements of the salts, like the two poles of the pile; the oxygen repels the acid, which, on the contrary, is attracted by the hydrogen, but it attracts the alkali, and with it determines to the hydrogen of the other solution, with which it re-forms water, whilst the alkali combines with the acid that it finds there, and produces a new salt.

It is the change, therefore, between the principles of the molecules of the water which determines that of the elements of the salts. This change also takes place, as is well known, when water is decomposed by the voltaic pile, and it is in like manner by these means that it favours the disunion, with the assistance of that instrument, of the elements of compounds which it holds in solution. Thus it is observed that the decomposition of saline compounds is always accompanied by that of the water.

It follows from what has been said, that two saline solutions cannot exist together without an exchange ensuing between the elements of the two salts; this is contrary to the opinion hitherto entertained. It has been supposed that the exchange does not take place unless an insoluble salt which precipitates be formed, and Berthollet attributed the decomposition to the insolubility itself. But it has been justly remarked, that the force of cohesion to which the insolubility is owing has no effect till the decomposition has taken place, and consequently cannot be the cause of it. Hence it is only a secondary cause, which renders it permanent and manifest, whereas otherwise it is continual and latent.

There are phenomena which prove the reality of this latent action between two saline solutions, in which no precipitate is formed, as in the action of the carbonates of potash and soda on insoluble salts. The decomposition is never complete, but always stops at a certain point, although a portion of the carbonate still remains in solution. This arises from the formation of a soluble salt by the union of the alkaline base and the acid of the insoluble salt, whose quantity continually increases with the progress of the decomposition. The phenomena mentioned above are then developed between the two salts in solution, and this reciprocal action opposed that of the alkaline carbonate on the insoluble salt. It at the same time renders that which the soluble salt resulting from the decomposition would exert on the insoluble salt, also derived from the same source, impossible, for in the decomposition of sulphate of barytes, for instance, by carbonate of potash, the soluble sulphate that is formed, might, after the separation of this carbonate, decompose that of the barytes; but this decomposition is also always incomplete in consequence of the formation of carbonate of potash which is again dissolved.

Of Organic Chemical Phænomena.

We have seen combinations effected between bodies whose molecules are naturally endowed with free opposite electricities, and others in which only one of the combining substances possessing acid or alkaline properties, developes by its influence the electricity of the other, and renders it a sort of accidental acid or alkali, the induced state being only momentary and conditional, and ceasing with the influence that occasioned it. We have now to consider a third kind of combination between bodies, some of which only possess alkaline or acid properties, as in the preceding instance, but develope them permanently in the others; in a word, real acids or alkalies are formed, and thus two compounds are produced instead of one. The definitive compound is preceded by the formation of another, which merely assists in forming part of the first.

A remarkable instance of this kind of combination is seen in the action of alkalies on fatty substances. The molecules of the former not finding a free negative electricity in those of the latter capable of neutralizing their own positive electricity, develope it, as in their action on water, by attracting such of their elementary molecules as are negatively electrified, and repelling the others; but water being formed of only two constituent molecules, it is evident that this influence could not produce a new compound. Organic substances, containing a greater number of elements, they are capable, by a change in their relative disposition, of so arranging themselves as to form compounds in which the negative electricity predominates, and consequently are able to neutralize the positive electricity of the alkalies.

This mode of combination is very different from that which gives rise to inorganic compounds. In fact, in their formation the constituent molecules, left to their reciprocal action, are free to obey their tendency to combine molecule with molecule, and the combination is thus always binary, or produced by the action of only two forces. The proportions of their elements depend solely on this binary disposition of their molecules and their number. Their electrical state has no influence on their proportions; but in the formation of acids by the action of alkalies on fatty substances, their constituent molecules are no longer abandoned to their sole reciprocal action; they are regulated by the influence of the positive electricity of the alkalies, which opposes their tendency to combine molecule with molecule, and obliges them to unite in such numbers, and to assume such relative disposition, as shall produce compounds, whose electrical state is capable of neutralizing that which acts upon them. In fact the negative electricity of the molecules attracted, being partly concealed by the positive electricity of the alkalies, at the moment the new compounds are formed, they are forced so to arrange themselves with the others as that they may retain an excess of it.

It is evident in this case that the proportions of the elements of these compounds must depend on the electrical state that they assume. It is important to notice this peculiarity, for the difference of the properties of organic substances depending solely on the different proportions of their elements, the prodigious variety of the former must lead us to infer that the cause which determines the latter cannot be the same as in inorganic substances, which combine in very limited proportions.

All the substances of the first class seem to owe their formation to a mode of combination analogous to that we have examined; for instance, let us first take the substances that are produced by the act of digestion, by which the food is partially converted into chyme, and the latter into chyle, which is effected by the liquors poured out by the excretory organs into the intestinal canal. It has been observed that the substances which are there injected are in a short time acidified. This confirms our theory, since all the liquors poured into the intestinal canal are alkaline. The chyme and the chyle, therefore, are merely salts composed of those alkaline liquors and of the acids developed by their influence in the food for the chyme, and in the latter for the chyle.

The act by which the organs give rise to the liquors they secrete, differs from the preceding merely inasmuch as the acid or alkaline products resulting from the influence of the particular matter which composes each of them on the blood, do not combine with it. Hence all the secreted liquors are alkaline or acid. This is not the only example of the kind. Fermentation, whether vinous or acetous, is an analogous phenomenon, for, as in secretion, the products formed do not combine with the matter which determined their formation, at least if we may judge by the small quantity of ferment that disappears during the operation.

Let us say one word on the causes of the spontaneous decomposition of organic substances, which are derived from the mode in which their elementary molecules are disposed with respect to their electrical state. It is not the same with all molecules of the same nature. In vegetable substances, for instance, the oxygen never being in sufficient proportion to form water with the hydrogen and oxide of carbon, or carbonic acid with the carbon, some of the molecules of the latter must be endowed with positive electricity, and tend to combine with the oxygen, and the others with negative electricity, and act more especially on the hydrogen. The azote of animal substances must be similarly circumstanced; but not so the oxygen and hydrogen, the former being negative, and the latter positive, with respect to all the others.

Hence we see that organic substances must have a constant tendency to be transformed into a certain number of inorganic compounds; for in one of them, composed of four elements, the positively electrical molecules of carbon tend to form carbonic acid with the oxygen, and the negatively electrical carburetted hydrogen with the hydrogen; whilst a part of the latter may also produce water with the oxygen or ammonia with the azote, &c.

The cause of the spontaneous decomposition of organic substances is, therefore, manifestly derived from the natural tendency of their molecules to form binary compounds; the effects of which tendency are only momentarily suspended, and prevail when circumstances favour it by setting them at liberty, amongst which a slightly elevated temperature is one of the most effectual. Then the organic compounds are succeeded, as the last results, by inorganic binary compounds, such as water, carbonic acid, ammonia, &c. Such is the outline of the theoretical considerations to which I have wished to call the attention of chemists. More numerous applications, by multiplying the facts on which they are founded, would undoubtedly have increased their interest, but were incompatible with the narrow limits to which I am obliged to confine myself.

Article VII.

An Account of Experiments on the Velocity of Sound, made in Holland. By Dr. G. Moll, Professor of Natural Philosophy in the University of Utrecht, and Dr. A. Van Beek.*

Sir Isaac Newton's formula, expressing the velocity of sound,

$$c = \sqrt{\frac{g\,p}{D}}$$

has since his time been investigated and demonstrated by several first-rate mathematicians. Actual experiments however on this velocity, instituted in various countries, and under different circumstances, went to prove that the celerity of sound, found by experiment, is about one-sixth greater than can be deduced by theory.

The celebrated Laplace accounted for this difference between experiment and theory, by showing that it could be attributed

* Abstracted from the Philosophical Transactions for 1824, Part II.

to the heat evolved by the compression of the particles of air which is effected by the undulations of sound. It was found impossible to determine the quantity of heat thus evolved, by the compression which sound occasions in the particles of the air; and therefore it was deemed expedient to multiply Sir Isaac Newton's formula by a constant factor $\sqrt{1 + k}$, the value of which was determined by experiment. Sir Isaac's formula thus altered, became

$$c = \sqrt{\frac{p\,g}{D}} \cdot \sqrt{1 + k}.$$

Thus, by the experiments of the French Academicians of 1738, the most accurate on this subject of that time, the value of k was found equal to 0,4254. It is plain that this correction of the original formula is merely empirical, and dependant on the accuracy of experiments, which in 1738, had certainly not attained the perfection which is required at present.

In consequence, this formula was thus altered by Laplace

$$\sqrt{\frac{g\,p}{D}} \cdot \sqrt{\frac{c'}{c}}$$

in which c' is the specific heat of the air under a constant pressure, and c is the specific heat of the air under a constant volume.*

My friend Dr. Van Rees, Professor in the University of Liege, gave a demonstration of this correction $\sqrt{\frac{c'}{c}}$, which will be subjoined to the present paper,† and which may be compared with that of Mr. Poisson.‡ The value of $\frac{c'}{c}$ was determined by Laplace from experiments of Messrs. Laroche and Berard,§ and found equal to 1,4954; but later and more accurate experiments of Messrs. Gay Lussac and Welter brought it to 1,3748.

Another cause of the difference between actual experiments on the velocity of sound and its theory, exists in the variable force of the wind, which either accelerates or retards the velocity of sound, according to the direction from which it is blowing. It appears that this cause of error may be annihilated in the following manner. Let sounds be excited exactly at the same time on both ends of a basis, and let two observers stationed on these ends, measure the velocity with which sounds travels from one end of the basis to the other. It is quite clear that the action of the wind must necessarily accelerate the velocity of the sound excited at one end of the basis, as much as it will retard that at

* Laplace in Ann. de Phys. et Chim. tom. iii. p. 238.
† Dissertatio de celeritate soni, Traject. 1818.
‡ Annales de Phys. et de Chim. Mai 1823, d. 5.
§ Ibid. Annales de Chimie, tom. lxxxv. p. 72.

the other end, and thus the medium of these two velocities will give the velocity in tranquil air. This method was not adopted by the French Academicians of 1738, in their experiments between Monthlery and Montmartre. Cannon was fired at one of these stations, whilst the observers were at the other, and thus the results remained affected by the whole effect of the wind. It was found expedient therefore to repeat these experiments with more accuracy, and this was executed with great precaution on Mr. Laplace's proposal, by Messrs. Arago, Prony, Mathieu, Bouvard, Humboldt, and Gay Lussac. The experiments were made in 1822, on the basis of Monthlery and Villejuif. In two successive days, the 21st and 22d of June, 1822, seven shots were fired on both stations, and observed on the other; the difference of time in which the corresponding shots were fired at both stations not exceeding five minutes, and from these seven corresponding shots the result was deduced.

These experiments having never been made in this country with any thing like sufficient accuracy, His Royal Highness Prince Frederick, second son of His Majesty the King of the Netherlands, and Master General of the Ordnance, was pleased to approve of our proposal of repeating the same, and to authorise Lieutenant-Colonel Kuytenbrouwer, and the officers and men of the battalion of Artillery under his command, to give us every assistance in their power, and to take an actual part in these experiments.

As fitted places to make these experiments, two elevated spots were selected on the extended heaths of the Province of Utrecht. One of these is a small hill between the town of Naarden and the village of Blaricum, and called the *Kooltjesberg;* the other is somewhat higher, and situated on the right of the road from Utrecht to Amersfoort, and very near the last town. Both places were distinctly visible from one another, and the distance was between 17000 and 18000 metres (9664 fathoms). Our time was kept by two time-keepers, which the Minister of Marine had kindly furnished us with; one made by Arnold, the other by our countryman Mr. Knebel. But the exact interval between the observation of light, and the perception of sound, and consequently the velocity of sound, was measured with small clocks with conical pendulums. They are made at Wesel by Mr. Wilhelm Pfaffius, and proved remarkably well adapted for this purpose. It is well known that Huigens laid down the properties of the conical, or centrifugal pendulum, but if we are not mistaken, they were employed for similar purposes for the first time by the German philosopher Benzenberg.* These clocks with a conical pendulum divide the 24 hours of the day

* Some account of these clocks is given in Gilbert's Annalen d. Physik, 1804, B. 16, p. 494; and New Series, vol. v. p. 333.

in 10,000,000 parts, and one of the indexes gives $\frac{1}{100}$ part of a decimal second. This index or second hand remains quiet, whilst the watch work continues moving as long as a certain spring is not pressed down with the finger; and on removing the finger, the index is reduced to rest in the identical moment. Thus the index being at 0, the spring is pressed down by the observer at the very instant the light of the opposite station is observed; the index continues moving till the report of the shot is heard, when the finger is withdrawn, and the index stopped instantaneously. The number of turns and fractions of a turn of the index shows the time elapsed between the fire and the report. There was a conical or centrifugal clock on each station; besides these, each station was furnished with a good barometer, carefully compared with a standard barometer of Mr. Dollond, several good thermometers made by Messrs. Dollond and Newman, besides a sufficient number of excellent telescopes of Dollond's, and so placed on stands adapted for the object as to bring the opposite station without trouble in the field of the telescope. The moisture of the air was determined for the first time in such experiments by Mr. Daniell's hygrometer, one of which was placed at each station. The direction of the wind was determined by very good vanes contrived by the Artillery officers. At each station a twelve pounder and a six pounder was planted, and the instruments were disposed in, or in the vicinity of tents erected for the purpose. Professor Moll, with Lieutenants Renault and Dilg, was stationed at the *Kooltjesberg*. Dr. Van Beek, with Lieutenants Sommerton, Van Den Bylaardt and Seelig were on the other station, which is commonly called *Zevenboompjes*, or seven trees, from the circumstance of seven trees being planted on this lonely elevation. Several gentlemen cadets of the Artillery, and several students of the University, were at both stations employed in observing the different instruments.

The barometers and thermometers were of course observed in the open air; Mr. Daniell's hygrometers were also placed in the open air; and the light of a candle reflected from the surface of the ball of the hygrometer, gave the means of observing the deposition of dew with great accuracy.

It was deemed of the utmost importance that the shots on both stations should be fired at as nearly the same moment as possible. To obtain this, the following plan was adopted. At $7^h\ 55'$ P. M. by the chronometer of Zevenboompjes, a rocket was fired at Zevenboompjes, which being observed at the other station of *Kooltjesberg*, was immediately answered by another rocket from the latter place. This was the signal that on both stations every thing was ready for observation. At $8^h\ 0'\ 0''$ by the chronometer of *Zevenboompjes*, a cannon shot was fired on that station, whilst the observers at Kooltjesberg took as exactly as possible the time on their chronometer when the light was observed. A second shot was fired at Zevenboompjes at $8^h\ 5'\ 0''$ P. M. by the chronometer of that station, and the time at which the light was seen was carefully taken down by the chronometer of Kooltjesberg. By these means the difference of the two chronometers at both stations, in a distance of about nine miles, was ascertained with great accuracy; and in order to show that this preparatory investigation was made with due care, a cannon shot was fired on both places at the moment when the chronometer of Zevenboompjes marked $8^h\ 10'\ 00''$. If the lights of both shots were seen exactly at the same time, it was a proof that the difference of both time-keepers was known, and that experiments might be safely made.

We own that we did not suppose before hand, that it could be possible to fire continually guns at a distance of nine miles exactly at the same second; but the very great attention and ability of our artillery men overcame this difficulty. Between our shots at the two stations there was never a greater difference than $1''$ or $2''$, whilst this difference in the experiments of the French philosophers of 1822, went to five minutes. This exact correspondence in the firing of the guns was obtained in the following manner. At each station an officer had the chronometer placed before him on a small table very near the gun; a non-commissioned officer or gentleman cadet stood ready with the port fire near the touch-hole; and at the instant required the officer holding the chronometer pressed the arm of the person who was to fire the gun, which went off at the very moment. With a little practice they were certain to fire the gun at any given second.

The first night of our experiments, the 23d, 24th, and 25th of January, 1823, we experienced the same annoyance of which the French philosophers had to complain the first night of theirs. The report of the shots of Zevenboompjes was not heard at all at the station of Kooltjesberg. But at Zevenboompjes all the shots of Kooltjesberg were distinctly heard. After the first night we constantly used the metal twelve pounders loaded with 6lbs of gunpowder. The 26th of January all the shots were heard at Kooltjesberg, but none were perceived at the opposite place. But the wind shifting the following night, a good number of corresponding or simultaneous shots were distinctly heard on both stations. The particulars of the experiments made in these different days will be found in the tables annexed to this paper. The disappointment we met with on the first days was however not entirely fruitless; we were convinced by it, that none but exactly corresponding shots can be of use in determining the velocity of sound. The result of the observations of 25th and 26th of January, when the reports were heard at one station only, and reduced to 0° temperature of the centigrade scale, and dry air, give differences of $\frac{1}{53}$, whilst the observations of 27th and 28th of January, when shots were distinctly heard on both stations, had only a difference of $\frac{1}{300}$ from each other.

The time which sound employs to travel from one station to another being duly ascertained, we proceeded to measure the distance between both stations. The distances of the steeples of Utrecht and Amersfoort, Utrecht and Naarden, and Naarden and Amersfoort being accurately known, we measured angles on our stations between these steeples, and on each steeple between the other steeples and the stations. Thus the distance was calculated by four different triangles, and the greatest difference between these calculations was $2^m,45$ or eight feet, which appeared of no consequence in these experiments. The distances of the different steeples which we took for our basis, result from the very exact geometrical survey of General Krayenhoff.*

From these different data we found, by calculations of which the detail will be given hereafter, that in our experiments at a temperature of 32° Fahrenheit, or 0° of the centigrade scale, the velocity of sound is $332^m,049$, or 1089,7445 English feet per sexagesimal second. A table showing the comparison of our experiments, with the observations of other philosophers, is also annexed to this paper.

Experiments on the Velocity of Sound on the 27th June, 1823, *compared with Theory.*

Having thus far stated the means by which the distance between the stations of Kooltjesberg and Zevenboompjes was ascertained, and the rate determined of the clocks by which the velocity of sound was measured, I will now proceed to give the experiments which were made on the 27th of June, and compare the result with theory. The following table contains the time which sound employed to travel over the basis on the 27th of June, when 22 shots were simultaneously fired, and equally seen and heard on both stations. The first column of this table shows the number of the shot, the second the time which sound employed to travel from Kooltjesberg to Zevenboompjes, as observed on the latter station, and the third column the time which sound employed to come from Zevenboompjes to Kooltjesberg, also observed on the latter place.

* Précis des Opérations Géodésiques et Trigonométriques en Hollande, par le Général Krayenhoff.

Experiments on the Velocity of Sound, made the 27th June, 1823.

I.	II. Sound travelled from Kooltjesberg to Zevenboompjes.	III. Sound travelled from Zevenboompjes to Kooltjesberg.
1	52,90″	51,17″
3	52,69	50,89
4	52,71	50,68
5	52,92	50,80
6	52,84	50,86
7	53,04	50,89
8	52,89	51,01
9	52,79	51,00
11	52,83	50,99
12	52,77	50,96
13	52,79	51,10
14	52,99	51,07
16	52,90	51,08
17	52,64	51,28
18	52,90	51,21
19	52,87	51,18
20	52,92	51,33
22	52,91	51,38
23	52,64	51,35
24	52,57	51,32
25	52,90	51,14
26	52,96	51,01
	1162,37	1123,70
	1123,70	
	2286,07	and $\frac{2286,07}{44} = 51,96''$

Thus, taking the mean of all the observations, we have the velocity with which sound travelled along our basis free of the accelerating or retarding effect of wind, on the 27th of June, 1823, equal to 51″,96. And as the basis was equal to 17669,28 metres, or 9664,7044 fathoms, we have the velocity of sound, found by experiment as above, equal to 340,06 metres, or 1116,032 English feet per second.

Now, whilst these 22 shots were fired, the mean temperature of the air was

$$\left.\begin{array}{rl} \text{At Zevenboompjes} & = 11°,21 \\ \text{Kooltjesberg} & = 11\ \ 11 \\ \hline \text{Mean temperature on both stations} & \} \ 11\ \ 16 \end{array}\right\} \begin{array}{l}\text{Of centrigade scale.} \\ = t\end{array}$$

The mean altitude of barometer corrected of the effect of capillarity, and reduced to the temperature of 0° of centrigrade scale, was as follows:

Station of Zevenboompjes	$0^{m},7439$
Kooltjesberg	0 7456
Mean altitude of barometer	0 74475 $= p$.

The mean tension of aqueous vapour in the atmosphere, as determined by Mr. Daniell's hygrometer, was at

Station of Zevenboompjes	= 0,00901235 metres.
Kooltjesberg	= 0,00949378
Mean tension of aqueous vapour,	0,00925307 $= f$.

The effect of gravity, calculated for mean latitude of Amersfoort and Naarden, by the formula

$$g = (g)\,(1 - 0{,}002837 \text{ cos. } 2\,l)$$
$$= \frac{9808{,}8}{1{,}000378864}\Big\{1 - 0{,}002837 \text{ cos. } 2\,\{52^\circ\ 13'\ 33'',35\}\Big\}$$
$$g = 9812{,}03 = \text{effect of gravity in lat. } 52^\circ\ 13'\ 33'',35.$$

The ratio of the specific heat of the air when the volume is constant, to the specific heat of air at a constant pressure, or $\frac{c'}{c}$, is, according to the experiments of Gay Lussac and Welter, equal to $1{,}3748 = \frac{c'}{c}$.

In Sir Isaac Newton's formula $\sqrt{\frac{g\,p}{D}}$, by which the velocity of sound is expressed, D is the density of air, that of mercury being taken for unit.

By Biot's and Arago's experiments, the density of perfectly dry air was found at $0^{m},76$ barometrical pressure to be equal to unity divided by 10466,82.

But when the barometrical pressure alters and becomes p, and the temperature becomes t, we have by the law of Mariotte

$$D = \frac{p}{10466{,}82 \times 0^{m},76\{1 + t\,.\,0{,}00375\}}$$

And introducing into this formula the correction for the aqueous vapour existing in the air, and calling F the tension of aqueous vapour existing in the air, we find

$$D = \frac{p - \frac{3}{8}F}{10466{,}82 \times 0^{m},76\{1 + t\,.\,0{,}00375\}}$$

This value of D being substituted in Sir Isaac's formula, we have the velocity of sound by theory

$$V = \sqrt{\frac{gp}{D}} = \sqrt{\frac{g\,p\,.\,10466{,}82 \times 0_{m},76\,\{1+t\,.\,0{,}00375\}}{p - \frac{3}{8}F}}$$
$$= \sqrt{\left\{10466{,}82 \times 0^{m},76\,\{1+t\,.\,0{,}00375\}\right\}\frac{g\,p}{p-\frac{3}{8}F}}$$

According to Laplace, this formula must be multiplied by the square root of the ratio between the specific heat of air at a constant volume, and the specific heat of air at a constant pressure. Thus the final formula for the velocity of sound, given by theory, is

$$V = \sqrt{\left\{10466{,}82 \times 0^{m},76\,\{1+t\,.\,0{,}03375\}\right\}\frac{g\,p}{p-\frac{3}{8}F}} \times \sqrt{\frac{c'}{c}}.$$

Substituting in this formula the quantities stated above, theory gives the velocity of sound for the state of the atmosphere on the 27th of June, 1823, when the experiments were made, V = 335,14 metres, or 1099,885 English feet; but the velocity as obtained by experiment was 340,06 = 1116,032 feet.

Difference between theory and experiment the 27th of June, 4,92 metres = 16,147 feet.

Experiments on the Velocity of Sound on 28th of June, 1823, *compared with Theory.*

On the 28th of June, 1823, fourteen simultaneous shots were equally seen and heard on both stations; the following table contains the results.

Number of shots.	Sound travelled from Kooltjesberg to Zevenboompjes in	Sound travelled from Zevenboompjes to Kooltjesberg in
3	51,81″	52,12″
4	51,94	52,10
5	51,77	51,28
6	51,98	52,51
7	52,17	52,46
8	52,15	52,28
9	52,25	53,10
10	52,18	50,17
12	52,40	52,19
14	52,27	52,62
15	52,27	51,66
17	52,23	51,52
18	52,49	51,99
19	52,56	51,60
Sum	730,47	727,60

The mean result by experiment on the 28th of June, 1823, is $\frac{730'',47 + 727'',60}{28} = 52,07$, in which time sound travelled along the basis of 17669,28 metres, or 57988,2264 English feet. Thus, the mean velocity of sound on the 28th of June in 1″, is 339,34 metres = 1113,669 English feet.

The mean temperature, when these experiments were made, was at

	Centigrade scale.
Zevenboompjes	10°,07
Kooltjesberg	11 36
Mean temperature	11 215 = t

Mean height of the barometer, corrected for capillarity, and reduced to 0° of centigrade scale,

Zevenboompjes	0,7476 metres.
Kooltjesberg	0,7487
Mean barometer, or p =	0,74815
Mean tension of aqueous vapour by Mr. Daniell's hygrometer F =	0,00840465

These quantities being substituted in the formula, we have the velocity of sound, by theory, on the 28th of June, 1823, V = $335^m,10$ metres = 1099,753 English feet; by experiment, $339^m,34$ metres = 1113,669 feet.

Difference between theory and experiment 4,24 metres = 13,916 feet.

Thus it appears by the experiments both of the 27th and 28th of June, that sound travels faster than its theoretical calculation.

The 27th of June, difference of experiment and theory	$4^m,92$
28th of June	$4^m,24$

The difference between the experiments of 27th and 28th of June, is but of $0^m,62$, or 2,3629 feet; that is about $\frac{1}{472}$ of the mean result of the experiments of both days.

The French philosophers found a difference between their experiments of 23d and 24th of June, 1822, of $\frac{1}{90}$. But the difference of $\frac{1}{472}$, which we obtained, if we reduce the observations of both days to what they would have been in perfectly dry air, and in temperature of 0° *cent.* is still remarkably lessened. The formula by which the velocity of sound in given hygrometrical circumstances, and a given temperature of the air, is reduced to what it would be in dry air of 0° *cent.* temperature, calling U′ the velocity of sound in dry air of 0° temperature; U the velocity of sound at a tension of aqueous vapour = F, is as follows:

$$U' = \frac{U}{\sqrt{\{1+0,00375\,t\}}} \times \sqrt{\{1-0,37651)\frac{F}{p}}.$$

The 27th of June, 1823, we had

$U = 340^m,06 = 1116,032$ English feet
$t = 11°,16$ *cent.*
$F = 0,00925307$
$p = 0,74475$ metres.

Substituting these quantities in the formula, we have

$U' = 332^m,38 = 1090,827$ English feet.

The 28th June, 1823, we had

$U = 339^m,34 = 1113,669$ feet
$t = 11°,215$
$F = 0,00840465$;

which being substituted in the formula, we have

$U' = 331^m,72 = 1088,661$ English feet.

Thus the difference between the observations of both days, when reduced to dry air, and 0° *cent.* is $0^m,66 = 2,166$ feet; or $\frac{1}{503}$ of the mean of the observations of both days. It appears also, that by our experiments of the 27th and 28th of June, 1823, the mean velocity of sound in air perfectly dry, and at 0° temperature, was $332^m,05 = 1089,744$ feet in a second.

Experiments on the 25th of June, when the Shots were not reciprocal.

The following experiments will I trust prove, that in experiments on the velocity of sound, such observations can only be relied on in which the shots on both stations were reciprocal, that is fell within the same second in both places, and were equally heard and seen on both stations. The 25th of June, the cannon fired at Zevenboompjes was not heard at Kooltjesberg, but at Zevenboompjes the report of the guns fired at the other place was distinctly perceived. The following table shows the time preterlapsed between the light and report, as observed at Zevenboompjes.

	Number of shots.	Time between light and report.	
1823. 25th June	1	52,31″	Observations at the station of Zevenboompjes, guns fired at the station of Kooltjesberg.
	2	52,59	
	4	52,47	
	7	52,20	
	8	52,47	
	10	52,17	
	12	52,27	
	14	52,52	
	15	52,54	
	16	52,43	
	17	51,91	
	19	52,50	

Sum 628,39, which being divided by twelve, the number of observations, gives the passage of sound along the basis 52″,37. Thus the mean velocity in 1′ was $337^m,39$ = 1107,268 feet.

The mean temperature at the time of these experiments

		Centigrade.
at Zevenboompjes	....	7°, 41
Kooltjesberg	....	8 , 54
Mean temperature of the air	...	7 ,975 = t

Mean height of the barometer corrected for capillarity, and at 0° *cent.*

at Zevenboompjes	....	0^m,7522
Kooltjesberg	....	0 ,7538
Mean barometer	....	0 ,7530 = p

Mean tension of aqueous vapour in the air,

at Zevenboompjes	....	0,00737444
at Kooltjesberg	....	0,00706966
Mean tension	....	0,00722205 = F,

which quantities substituted in the formula, we have for temperature 0° *cent.* and in perfectly dry air the velocity of sound U′ = 331,85 metres = 1089,087 feet.

Experiments of 26th June, 1823, *when the Shots were not reciprocal.*

The 26th of June, the following shots were seen and heard at Kooltjesberg and fired at Zevenboompjes, but no shots from the first station were heard at the latter.

Number of shots.	Time between light and report.	
1	50,20″	Guns fired at Zevenboompjes, heard and seen at Kooltjesberg.
2	50,80	
3	51,44	
4	52,20	
5	51,10	
9	50,11	
11	50,99	
12	50,81	
13	51,00	
14	51,01	
16	51,12	
Total of 12 shots	560,78	The mean of which is 50″,98

which gives a velocity of 346,59 metres = 1137,134 feet in 1″.

The temperature was at that time

at Zevenboompjes 11°, 57
Kooltjesberg 12 , 54

Mean temperature .... 12,055 = t.

Mean atmospheric pressure at Zevenboompjes 0^{m},7493.
Mean atmospheric pressure at Kooltjesberg 0^{m},7512.
Mean pressure of atmosphere 0,75025 = p.
Mean tension of aqueous vapour at Zevenboompjes 0,00892922
at Kooltjesberg 0,01011376
Mean tension of aqueous vapour .... 0,00952149 = F

Calculating by this datum we shall have the observed velocity of sound in 1″ reduced to dry air and 0° temperature V = 338^{m},20 = 1109,927 feet; but the experiments of the 25th gave V′ = 331^{m},85 = 1089,087 feet. Difference 6^{m},35 = 20,840 feet per 1″ between the experiments of the 25th and 26th of June, in which the shots were not reciprocal. This difference is about $\frac{1}{53}$ of the mean of both observations. But the 27th and 28th of June, when the shots were reciprocal, the difference between the results of both days were only 0^{m},66 = 2,166 feet, that is about $\frac{1}{503}$ of the mean result of the observations.

From the comparison of these results we may safely infer, that only such shots will answer the purpose for which these experiments are made, which are exactly fired at the same instant on both stations.

It is in this respect, I imagine, that our experiment may claim some attention, as the very great care and ability of our artillery men enabled us to have the guns fired within the interval of one second.

A Table showing the Results of Experiments on the Velocity of Sound as observed by different Philosophers.

Names of observers.	Time when made.	Country where made.	Length of basis in feet.	Velocity of sound per second in feet.	
Mersenne		France	—	1469·88	1
Florentine philosophers	1660	Italy	5905·8	1184·44	2
Walker	1698	England	2624·8	1305·83	3
Cassini, Huigens, &c.		France	6906·50	1151·63	4
Flamsteed and Halley		England	16405·0	1141·78	5
Derham	1704 & 1705	England	5249·6 to 6562	1141·78	6
French Academians	1738	France	75177·55 and 93593·8	1092·57 at 32° F.	7
Blanconi	1740	Italy	7874·0	1043·35	8
La Condamine	1740	Quito	67401·58	1112·25	9
La Condamine	1744	Cayenne	129366·54	1174·59	10
T. F. Mayer	1778	Germany	3702·40	1105·69	11
G. E. Muller	1791	Germany	8530·6	1108·97	12
Epinoza and Banza	1794	Chili	53627·94	1168·50	13
Benzenberg	1809	Germany	29765·23	1092·57 at 32° F.	14
Arago, Mathieu Prony	1822	France	61065·97	1086·0	15
Moll, Van Beek, and Kuytenbrouwer	1823	Netherlands	5797290·76	1089·7445 at 32° F. dry air.	16

1. Mersenne de Arte Ballistica, Prop. 39.
2. Tentamina Experim. Acad. del Cimento, L. B. 1738, Part II. p. 116.
3. Philos. Trans. 1698, No. 247.
4. Duhamel, Hist. Acad. Reg. L. II. Sect. 3, Cap. II.
5. Philos. Trans. 1708 and 1709.
6. Ibid. ibid.
7. Mem. de l'Academie des Sciences, 1738 and 1739.
8. Comment. Bononienses, vol. ii. p. 365.
9. La Condamine Introduction Historique, &c. 1751, p. 98.
10. Mem. de l'Acad. Royale des Sciences, 1745, p. 488.
11. J. T. Mayer, Praktische Géométrie Göttingen, 1792, B. 1, p. 166.
12. Muller, Götting. Gelehrt. Anzeige, 1791, St. 159 et Voigts Magazin, &c. B. 8, St. 1, p. 170.
13. Annales de Chimie et de Phys. t. vii. p. 93.
14. Gilbert's Annalen, neue Folge, B. v, p. 383.
15. Connoissance des Tems. 1825, p. 361.

Article VIII.

On Copper Sheathing.

We copy the following answer to an article which appeared, it seems, in the Plymouth Journal a short time since, from the Devonport Telegraph of Sept. 3 :—

Sir H. Davy's Protectors.

"We cannot descend to personalities with the *Plymouth Journal* and its vulgar auxiliaries—a style of writing which we thought had departed with its former editor. We repeat our conviction, that the article originally complained of was calculated to convey a wrong impression to the world, and we know it has in more than one case produced an injurious effect. The impression derived from that article was, that Government had abandoned Sir H. Davy's plan *altogether,* which is *contrary to the fact;* and our cotemporary has been compelled by *us* to admit, that *all ships in a good condition in ordinary are to be fitted with protectors.* Our assertion was, that the application of protectors was *suspended for sea-going ships,* but that they were to be *applied to ships in ordinary,* and we cited the Royal Sovereign as an instance. Let our readers compare this assertion with the following public order, *copied from the Plymouth Journal,* and we are sure they will feel satisfied that we have done that which was right—right for the public service, and right for the fame of Sir H. Davy—*the true and equitable mean which ought at all times to be observed:—*

" '*Public Order, July* 23.

" 'In pursuance of an order from the Lords Commissioners of the Admiralty of the 19th inst. we direct and require you to consider it as a general rule, that *no sea-going ship* is to be fitted with Sir Humphry Davy's protectors, and that when such ships, in good condition, come into dock from time to time to refit, the protectors now upon them are to be removed.

" 'The protectors are, however, *to be applied to ships in good condition in ordinary,* and when such ships are brought forward for service, the protectors are to be removed, and the copper cleaned.' "

" In addition to this order, another was issued here on Monday last, breathing the same spirit as the former, and affording an additional confirmation of our views, for an additional confirmation it must be regarded, when *it extends the application of protectors even to sheer-hulks and receiving-ships.* This order, however, having been furnished *confidentially* from high authority, we do not feel ourselves at liberty to publish it, but any one interested in the inquiry may, no doubt, see it in the Dock-yard.

" We ask for nothing unfair. We ask only to have these orders compared with the original article in the *Journal,**—*the*

* (Extract from the Plymouth Journal.)

" '*Failure of Sir H. Davy's Plan for the Protection of Ships' Bottoms.*—The plan, some time since recommended by Sir H. Davy, to prevent the *oxidation* of copper on ships' bottoms, and which was adopted by Government with a laudable zeal for the interests of *sciences,* has not been found to produce the expected benefits. In the instance of one of his Majesty's ships, which was fitted *four* years ago on Sir H. Davy's plan, and which is now undergoing repair in this Dock-yard, it appears that the galvanic influence of the iron has indeed prevented the *oxidation* of the copper, but the bottom of the ship is found, as in the case of wood sheathing, to be foul with weeds and *barnacles,* to provide against which copper bottoms were originally adopted. We shall next week more particularly allude to the nature of the process, meanwhile *we understand that orders have been received to discontinue the fitting of his Majesty's ships on Sir Humphry Davy's principle.*' "

only article with which we have to do—and every honest man must say, *justice was not done.* Let it also be remembered, that by far *the major part of our navy* is now in a state of ordinary, and likely so to continue for many years to come ; and that to by far the greater part of the ships in that state the orders above given actually apply ; and that, therefore, in an economical point of view, the application of protectors to so many ships must be of immense national advantage. *It was only on Tuesday last that protectors were applied to the Saturn at this yard,* a circumstance which our cotemporary, with his accustomed candour, carefully conceals, although it seems not at all at a loss for instances hostile to the plan. It is for this reason that we envy not the feelings which dictated the original paragraph, and we repeat our first assertion, that no friend of Sir H. Davy could have written it, and when the *latest* history of these protectors shall be given to the world, the author of that article will have the honour of being classed with some other worthies of the same stamp ; he will form, with the Editor of the *Times* and Mr. Samuel Deacon,* an unenvied triumvirate. We shall defer any farther remarks until the publication of Sir H. Davy's next paper."

Through the kindness of Sir Robert Seppings, we are enabled to subjoin the second order alluded to by the author of the preceding observations. It is dated Aug. 27, 1825 :—

"In addition to our warrant of the 23d ult. respecting the application of Sir H. Davy's mode of protecting copper on the bottoms of ships in good condition intended to lie in ordinary, we direct and require you to cause it to be applied also as opportunities offer to the stationary ships, such as sheer-hulks, receiving-ships," &c. &c.

It is not for us to question the propriety of the measures adopted by the Lords Commissioners of the Admiralty, though we cannot help still thinking that by a due adjustment of the proportion of the *protecting* to that of the *copper* surface, the mode may yet be found perfectly applicable to *sea going ships,* as well as to those in ordinary, &c. It seems to us to be one of those cases in which the *theory* is so obviously correct, that whatever difficulties may occur in the earlier attempts to reduce the method to practice, there *must* be certain circumstances which, when once discovered, will ensure complete success. What those circumstances are can only be determined by reflection and experiment. Sir Humphry Davy has already done much, and we do hope that every facility will be afforded him for continuing and perfecting his labours on this nationally momentous subject. He has victoriously contended with difficulties in our estimation far greater than any that await him in

* See *Annals of Philosophy,* pages 141 and 364.

this investigation, and we confidently predict that his keen and indefatigable genius will ultimately triumph over every present obstacle.

The two Admiralty Orders quoted above show the sense which that Board entertains of the value of Sir Humphry Davy's discovery with respect to ships in ordinary; and the intelligent author of the article in the Devonport Telegraph has justly and forcibly insisted on its immense importance in that point of view alone. But in the opinion of the *Plymouth Editor,* Sir Humphry Davy's plan is a *failure.* "Thy wish was father, Harry, to thy thought," though why it should be so, we know not; that is no business of ours:—so, without further comment, to his wishes and his thoughts we leave him.—C.

Article IX.

Analysis of the Seleniurets of the Eastern Harz. By M. Henry Rose.*

The seleniferous minerals, whose analysis is subjoined, were discovered by M. Zinken, mine-engineer to the Duke of Anhalt-Bernburg. They are found in the eastern part of the Harz, at two places, situated at a small distance from each other, one of which is near Zorge, in the veins of iron which traverse the argillaceous schist and diorite: these seleniurets are disseminated in magnesian carbonate of lime. The other point is near Tibzerode, in the veins. The seleniurets at the latter place are found in larger quantity, dispersed also in magnesian carbonate of lime, and are frequently accompanied by small quantities of native gold.

M. Zinken observed the presence of selenium in these minerals in 1823. He had the goodness to send me a large quantity of the seleniurets, of which I have analysed only five varieties, the rest not being sufficiently pure to be calculated for a quantitative analysis.

I analysed these minerals by means of chlorine. I converted all the metals they contain into chlorides, and separated the chloride of selenium, which is volatile, from the rest which are fixed. I did not use nitric acid, or aqua regia, to dissolve these minerals, because they always contain lead, and consequently I should have been obliged to precipitate the oxide of lead by sulphuric acid; but in order to obtain the whole of the sulphate of lead, it would have been necessary to evaporate the liquid to dryness, and to heat the dry mass, to drive off all the free acids; which would have rendered it impossible to determine the quan-

* From the Annales de Chimie.

tity of selenium. On the contrary, by precipitating the selenium from the acid solution by sulphurous acid, we do not obtain the whole of it, because a small quantity of seleniate of lead, and even of sulphate and chloride of lead, are thrown down at the same time.

The apparatus I employed in these analyses is nearly the same as that which M. Berzelius used in his analysis of grey nickel. I welded two tubes to a small glass bulb; one of the tubes was of small diameter, and four inches long; the diameter of the other was much larger, and its length twelve inches. Having bent the latter to a right angle, near the middle, I weighed the whole apparatus, then introduced the pulverised mineral, and weighed it again. The smaller tube was joined to an apparatus in which chlorine was very slowly disengaged, and the gas was dried by chloride of calcium. The bent tube passed into a bottle, filled two-thirds with water, traversing a perforated cork, which did not close the bottle hermetically, and dipping only a few lines deep into the water.

The apparatus being filled with chlorine, the bulb was very gently heated by a spirit-lamp. Chloride of selenium formed and sublimed. Protochloride of selenium forms at first, and flows through the tube, as an orange coloured liquid, into the water in the bottle, where it deposits selenium, the greater part of which is afterwards redissolved by the chlorine which traverses the liquid. Afterwards scarcely any thing but perchloride of selenium is formed, which has great resemblance to the perchloride of phosphorus; it condenses in the tube, and would choak it up if its diameter were not pretty considerable. It is necessary very frequently to volatilise the chloride which condenses in the tube near the bulb, by the flame of a small lamp, and thus make it pass over into the water in the bottle, which, if the quantity be large, is somewhat difficult. The chlorine must be very slowly liberated; for if the bubbles of gas rise too rapidly through the water, the chloride of selenium which they contain has not time to be decomposed by the water, and a portion escapes by the little aperture in the cork undecomposed.

I caused chlorine to pass over the mineral for half a day; all the metals were then perfectly changed into chlorides. The operation was at an end when chloride of selenium ceased to be formed. The bulb was then cautiously cooled lest the glass should crack by the cooling of the fused chloride of lead. When cold I cut off the part of the wide tube which still contained chloride of selenium, and dropped it into the liquid in the bottle. After having washed it, I weighed the tube with the bulb containing the fixed chlorides. If the mineral contained iron, a portion of the chloride of iron was found in the tube, and the remainder with the fixed chlorides.

It is necessary not to overheat the bulb, lest a portion of chloride of lead should be volatilized.

The selenium was thrown down from the liquid in the bottle by sulphite of ammonia, muriatic acid having been previously added to the liquid. The selenium was collected on a filter, dried, and weighed. Although it is very easy to throw down the whole of the selenium from a solution of selenic acid by sulphurous acid, it is nevertheless very difficult to precipitate it from a solution of chloride of selenium in water, through which a current of chlorine has been passed for a long time. The liquid must be digested a great while with sulphite of ammonia, and frequently boiled, in order to obtain the whole of the selenium. The iron was precipitated from the liquid, after the separation of the selenium. The seleniurets which I analysed are the following:—

1. *Seleniuret of Lead.*—This was the most frequent seleniuret in the minerals I received. It has internally so great a resemblance to sulphuret of lead as not to be easily distinguished from that substance. Its colour is lead grey, its lustre very metallic; it occurs in masses imbedded in magnesian carbonate of lime, from which, however, it may be very easily freed for analysis by digestion in weak muriatic acid. Its fracture is saccharoidal; the granulations are of various fineness, the coarsest distinctly exhibiting a lamellar cleavage, the directions of which, however, I could not determine: it is brittle and soft.

This mineral, purified by diluted muriatic acid, gives no sublimate, nor fuses, when heated by the blowpipe in a small tube closed at one end. Heated in an open tube, a small quantity of selenium sublimes, and hygrometrical crystals of selenic acid are formed at the same time. The whole assay becomes surrounded by fused yellow oxide of lead, and the tube is filled with the peculiar odour of selenium. On charcoal it fumes, and tinges the flame of the lamp blue by the combustion of selenium. For some distance round the assay, the charcoal is covered with sublimed oxide of lead, but no metallic lead is produced without the addition of soda. The fluxes discover nothing but lead, though some specimens give indications of iron, and one presented traces of copper.

After all the experiments I had made with the blowpipe, a quantitative analysis was almost useless; only seleniuret of lead could produce these phenomena. However, I analysed a very pure specimen, in which not the slightest trace of either iron or copper could be discovered by the blowpipe. 3·221 grammes (50 grains) of the purified mineral treated with chlorine, gave 3·104 grammes (48 grains) of chloride of lead, equivalent to 2·313 grammes (35·8 grains) of lead, or 71·81 per cent. The chloride of lead dissolved entirely in water without leaving any residuum of chloride of silver, or any portion of the mineral undecomposed. Another portion of the mineral weighing 3·327 grammes gave 0·918 gr. of selenium, or 27·59 per cent. These results agree pretty well with the calculated composition of the mineral, according to which seleniuret of lead is composed of 72·3 lead and 27·7 selenium.

To ascertain whether this mineral contain any trace of sulphur not discoverable by the blowpipe, a portion was treated with chlorine, and the sublimate made to pass into water. The liquid was rendered very acid with muriatic acid, and muriate of barytes dropped into it; but not the slightest precipitate of sulphate of barytes could be perceived. The same experiment was repeated with several other specimens of seleniuret of lead, and always with the same result.

2. *Seleniuret of Lead, with Seleniuret of Cobalt.*—M. Zinken sent me only one specimen of this mineral, adding that it has great resemblance as to its elements to a mineral found at Clausthal, which Mr. Hausmann named *Kobaltbleierz.* Externally it resembles seleniuret of lead, and like it is disseminated in magnesian carbonate of lime, from which it may be freed by diluted muriatic acid. Its nature is easily detected by the blowpipe. It gives a sublimate of selenium when heated in a small tube closed at one end, and evinces the presence of cobalt by fusion with the fluxes on charcoal. In other respects it behaves before the blowpipe like seleniuret of lead. By treating 1·782 grammes (28 grains) with chlorine, I obtained 0·56 grammes (8·65 grains) of selenium, and a little iron. The fixed chlorides dissolved entirely in water without leaving any residuum. The liquid was evaporated to dryness after the addition of sulphuric acid; the dry mass heated to expel the excess of acid, and mixed with water. It left 1·668 gramme (25·8 grains) of sulphate of lead, equivalent to 1·139 gramme (17·6 grains) of lead. I then threw down the oxide of cobalt by caustic potash, but the filtered liquid still contained a little cobalt, which was precipitated by hydrosulphuret of ammonia. The whole quantity of cobalt obtained was 0·056 gramme (0·87 grain), and it still contained traces of lead and iron, which were not separated from it. The result of the analysis per cent. is,

Lead	63·92
Cobalt	3·14
Selenium	31·42
Iron	0·45
Loss	1·07
	100·00

The composition of this mineral appears to be analogous to that of iron pyrites, and may be called *cobaltiferous seleniuret of lead.*

3. *Seleniuret of Lead with Seleniuret of Copper.*—Two specimens of the minerals sent by M. Zinken were externally precisely alike. They were of a lead grey colour, not crystallized; had a saccharoidal fracture, and were composed of nearly homogeneous masses. They were surrounded by magnesian carbonate of lime, but not, as seleniuret of lead generally is, disseminated through it. The two specimens were distinguishable from each other by the difference in their behaviour before the blowpipe. Both fuse pretty easily on charcoal in a small matrass, but one more readily than the other. The least fusible melts like sulphuret of antimony by the heat of a small spirit lamp. In other respects, they behave alike before the blowpipe. They give no sublimate when heated in a small matrass, but in an open tube they afford, like seleniuret of lead, both selenium and selenic acid. The assay fuses, and is surrounded by yellow oxide of lead. With the fluxes they give very distinct traces of copper.

The least fusible was analysed by chlorine, like the other seleniurets. The fixed chlorides were dissolved in water, and the oxide of lead was precipitated by sulphuric acid, with the precautions detailed in the analysis of the cobaltiferous seleniuret of lead. The solution from which the lead was separated was mixed with caustic potash, and boiled to throw down all the oxide of copper. The analysis of this mineral gave in 100 parts,

Selenium	29·96
Iron, with a trace of lead	0·44
Lead	59·67
Iron	0·33
Copper	7·86
Undecomposed mineral	1·00
Loss	0·74
	100·00*

59·67 of lead combine with 22·86 of selenium to form seleniuret of lead. If we suppose the copper to be combined with an atom of selenium, the 7·86 would require 4·93 of selenium, which leaves an excess of selenium. If we imagine the copper to be combined with two atoms of selenium, as in the seleniuret of copper which is formed by passing seleniuretted hydrogen into solutions of oxide of copper, the 7·86 would require 9·86 of selenium, in which case there is a deficiency of selenium. It appears probable, therefore, that the copper exists in the mineral as a seleniuret and a biseleniuret, and that the two bear a simple

* The solution of the mineral in nitric acid gave no precipitate with muriate of barytes; consequently it contains neither sulphur nor silver.

proportion to each other. But as the mineral is not crystallized, I cannot venture to pronounce decidedly on its composition.*

I am convinced that a part, at least, of the copper in this mineral may contain two atoms of selenium, although no selenium sublimes, when an assay is heated in a small matrass, as we might expect it to do. I fused seleniuret of lead with seleniuret of copper, prepared by heating copper filings with selenium, and made the mass red-hot, so that it could not contain any excess of selenium. The alloy of these seleniurets fused rather more readily than seleniuret of copper alone. I found that I could add a considerable quantity of pure selenium to this alloy, without its being sublimed by heat. The alloy merely became more fusible, and that in proportion to the quantity of biseleniuret that it contained.†

4. *Seleniuret of Lead with Seleniuret of Copper, in a different Proportion.*—The other more fusible specimen likewise gives no sublimate when heated in a small matrass, provided it be pure. A large quantity of the mineral, of a violet colour, gave, however, a black sublimate by heat which had the appearance of selenium, but afforded globules of mercury, when heated with soda in a matrass, and proved to be a seleniuret of mercury. The deeper the violet colour of these specimens, the larger is the quantity of seleniuret of mercury that they contain, but I did not analyse them, in consequence of the variable quantities of that seleniuret. For the purpose of analysis, I selected portions that had not a violet colour, which gave per cent.

Selenium	34·26
Copper	15·45
Lead	47·43
Silver	1·29
Oxides of lead and iron	2·08
	100·51‡

I have not deducted the oxygen of the 2·08, the weight of the oxides of iron and lead, which is the cause of the slight excess that I obtained.

47·43 of lead combine with 18·13 of selenium to form seleniuret of lead, and 15·45 of copper with 9·69 of selenium to form seleniuret of copper, and with 19·38 to form the biseleniuret. What I have already said of the probable composition of the

* A second analysis gave me 57·13 lead, and 9·56 copper; the latter, however, contained some iron which was not separated from it.

† Some sulphurets exhibit similar phenomena. A compound of one atom of cobal with four atoms of copper would lose sulphur by being heated in a matrass. It, however, loses nothing, if it contain arseniuret of cobalt, as in the grey cobalt.

‡ On repeating the analysis I obtained 14·23 per cent. of copper, 50·27 lead, and 1·09 silver. The difference between these two analyses is greater than should exist between two analyses of a crystallized mineral.

other mineral may be applied to this. It appears also that the same proportion obtains between the two seleniurets of copper.

The proportions of the elements of these two minerals are not perhaps definite. We may call the least fusible one, which also contains the smallest quantity of copper, *cupriferous seleniuret of lead;* and the other, which has a larger quantity of copper, and is more fusible, *seleniuret of lead and copper.*

5. *Seleniuret of Lead with Seleniuret of Mercury.*—The analyses of this compound were more troublesome than those of the other seleniurets, because the seleniuret of mercury is not combined with the seleniuret of lead in any definite proportion. Different portions of the same specimen are so unequally composed that two pieces of the same mass gave very different results. A seleniuret of lead which contains no seleniuret of mercury, cannot be distinguished by its external appearance from those seleniurets which contain either much or little of it. They have the same colour, are only found massive, and disseminated in bitter spar. Some specimens have a small grained, saccharoidal fracture; others are coarse grained, and afford parts which have a pretty distinct triple cleavage, according to the planes of the cube. I observed in several specimens of this mineral that the seleniuret of lead most remote from the bitter spar, contained the greatest quantity of seleniuret of mercury, and that that in immediate contact with it was quite free from it. When the mineral has a distinct cleavage, only the latter presents lamellar parts: the first (that in contact with the bitter spar) is always fine grained saccharoidal. It is easy to ascertain whether the seleniuret of lead contains much seleniuret of mercury, or not; for the pure seleniuret of lead does not fuse, and gives no sublimate when heated in a small matrass; but if it contain seleniuret of mercury, the latter rises, and forms a very crystalline sublimate, the quantity of which is proportionate to that of the seleniuret of mercury in the mineral. If it be large, the assay boils up strongly at first, whilst the seleniuret of mercury sublimes, and only infusible seleniuret of lead is left. A small portion of seleniate of mercury is usually formed by the action of the air in the matrass, which is rather more volatile than the seleniuret. The latter may be wholly converted into seleniate by heating the assay in an open tube. The fused seleniate of mercury forms yellowish drops which have some resemblance to the oxide of tellurium, whose presence I suspected in these minerals before I had satisfied myself that they contain mercury. The presence of mercury is detected by heating the mineral in a small matrass with a little dry carbonate of soda, when the mercury sublimes. It is also sometimes obtained, as well as the seleniuret of mercury, by heating the mineral without the soda, but in the latter case, its production is owing to the bitter spar which decomposes the assay.

I chose, for a quantitative analysis, small cubic morsels, whose specific gravity was, by one experiment, 7·876; by another 7·804. I analysed them, like the rest, by chlorine, but I was obliged to vary the process, in consequence of the chloride of mercury subliming with the chloride of selenium. Corrosive sublimate was always formed in these analyses, but never any calomel, and consequently the product was always wholly dissolved by the water in the receiver. Corrosive sublimate has some resemblance to perchloride of selenium, but is less volatile, and forms long brilliant needles, which are never found in chloride of selenium.

I made several unsuccessful attempts to separate the oxide of mercury from the selenic acid contained in the liquid in which those two substances were dissolved. M. Berzelius has already observed that the caustic alkalies or their carbonates only imperfectly separate selenic acid from oxide of mercury, and in fact cannot be wholly precipitated from its solution in other acids by the alkalies. I endeavoured to obtain the mercury of the mineral by mixing the latter in powder with dry carbonate of soda, or carbonate of lime, and heating the mixture; but it was not easy to obtain the whole of the mercury by this method.

Hydrosulphuret of ammonia, however, throws down the whole of the mercury from its alkaline solutions, and, according to my experiments, even a great excess of the hydrosulphuret does not redissolve the sulphuret of mercury, at least in the cold. I did not expect this result, because the preparation of cinnabar, in the moist way, depends on the solubility of sulphuret of mercury in hot hydrosulphuret of potash. 1·556 gramme (24 grains) of the mineral gave 1·168 gramme (17·6 grains) of chloride of lead which contain 0·87 gramme (13·5 grains) of lead. The whole of the chloride dissolved perfectly in the water.

The liquid in which the volatile chlorides were dissolved was mixed with caustic ammonia, and then hydrosulphuret of ammonia added in excess. Sulphuret of mercury fell down which, when collected on a weighed filter, and carefully dried, weighed 0·306 gramme (4·8 grains). I did not analyze this sulphuret, but as the hydrosulphuret of ammonia employed had been recently prepared it could not contain any free sulphur. It contained, therefore, 0·264 gramme (4 grains) of mercury. The liquid from which the mercury had been separated was acidulated by muriatic acid, and heated to expel the sulphuretted hydrogen. The sulphuret of selenium was oxidated by aqua regia, and the solution added to the liquor from which the sulphuret of selenium (*mercury*?) had been precipitated. The selenium was reduced by sulphite of ammonia after the nitric acid of the aqua regia had been decomposed as far as possible by muriatic acid. The quantity of selenium obtained was 0·389 gramme (6 grains.) The result of the analysis, therefore, per cent. is,

Selenium	24·97
Lead	55·84
Mercury	16·94
Loss	2·25
	100·00

The loss is too great to allow us accurately to determine the composition of the mineral, but I have reason to believe that it consisted principally of selenium. 55·84 of lead combine with 21·39 of selenium, and 16·94 of mercury with 6·63 of selenium. We may, perhaps, imagine it to consist of an atom of seleniuret of mercury and three atoms of seleniuret of lead.

I am satisfied, however, that the seleniuret of lead in this mineral is not combined with the seleniuret of mercury in any definite proportion, but that the two seleniurets are capable of uniting (like isomorphous substances) in all proportions without affecting the form of the compound; for I treated 0·9 gramme (13·9 grains) of the same specimen from which I selected the portion for the first analysis with chlorine, carefully selecting the cubic pieces which had precisely the same external appearance as those which I had analysed in the first instance, and I obtained only 0·33 gramme (5 grains) of chloride of lead, equivalent to 0·246 gramme (3·8 grains) of lead. If we calculate the composition of the mineral according to this result, we obtain the following as the proportions of its elements, wholly different from the other.

Selenium	27·98
Lead	27·33
Mercury	44·69
	100·00

In addition to the preceding minerals, M. Zinken has also sent me another containing selenium, lead, copper, and a good deal of silver. I have not, however, submitted it to a quantitative analysis, because the specimen was mixed not only with magnesian spar, but also with copper pyrites, from which I could not separate it. The copper pyrites moreover covered the whole mass in the form of mammellæ, and contained, itself, a large portion of selenium.

Article X.

A Table of Chemical Equivalents.

[With few exceptions, the following numbers are taken from Dr. Thomson's "Attempt to establish the First Principles of Chemistry by Experiment."]

Acid, acetic	50
(c. 1 w.)	59
arsenic	62
arsenious	54
boracic	24
(c. 2 w.)	42
carbonic	22
chloric	76
perchloric	92
chloriodic	196
chlorocarbonic	50
chlorocyanic	62
chromic	52
citric	58
(c. 2 w.)	76
columbic	152
fluoboric	32
fluoric	10
formic	37
fluosilicic	26
gallic	62
hydriodic	125
hydrocyanic	27
hyposulphurous	24
hyposulphuric	36
iodic	104
malic	70
manganesous	52
manganesic	60
molybdous	64
molybdic	72
muriatic	37
nitric (dry)	54
(liquid sp. gr. 1·5, 2 w.)	72
nitrous	46
oxalic	36
(c. 4 w.)	72
perchloric	92
phosphoric	28
phosphorous	20
saclactic	104
selenic	56
succinic (anhydrous crystals)	50
sulphuric	40
(liquid, sp. gr. 1·4838, 1 w.)	49
sulphurous	32
tartaric	66
(c. 1 w.)	75
titanic	48
tungstic	150
uric	72
(c. 2 w.)	90
Alum (c. 25 w.)	487
Alumina	27
sulphate	67
subsulphate (2 acid + 3 base)	116
Aluminum	19
Ammonia	17
acetate	67
(c. 7 w.)	130
arseniate	79
bicarbonate (2 w.)	79
borate	41
(c. 2 w.)	59
carbonate	39
citrate	75
fluoborate	49
hydriodate	142
iodate	131
molybdate	89
muriate	54
nitrate	71
oxalate	53
(c. 2 w.)	71
phosphate (c. 2 w.)	63
phosphate	37
sesquicarbonate (1 w.)	59
succinate (c. 2 w.)	85
sulphate	57
(c. 1 w.)	63

sulphite	49
tartrate	83
Antimony	44
chloride	80
iodide	168
oxide	52
deutoxide	56
peroxide	60
sulphuret	60
hydrosulphuret	69
tartarized (emetic tartar, c. 3 w.)	363
Arsenic	38
sulphuret (realgar)	54
sesquisulphuret (orpiment)	62
Azote	14
chloride	158 ?
carburet (cyanogen)	26
iodide	46
oxide	22
deutoxide	30
Barium	70
chloride	106
iodide	104
oxide	78
peroxide	86
phosphuret	82
sulphuret	86
Barytes	78
acetate	128
(c. 3 w.)	155
arseniate	140
arsenite	132
carbonate	100
chlorate	154
chromate	130
citrate	136
hydrate (1 w.)	87
iodate	242
muriate (c. 1 w.)	124
nitrate (anhydrous crystals)	132
oxalate	114
phosphate	106
phosphite	98
succinate	128
sulphate	118
sulphite	110
tartrate	144
Bismuth	72
chloride	108

oxide	80
acetate	130
arseniate (2 w.)	160
carbonate	102
citrate	138
iodide	196
nitrate (c. 3 w.)	161
oxalate	116
phosphate (3 w.)	135
phosphuret	84
sulphate	120
sulphuret	88
tartrate (5 w.)	191
Borax (c. 8 w.)	152
Boron	8
Cadmium	56
chloride	92
oxide	64
acetate (c. 2 w.)	132
carbonate	86
iodide	180
nitrate (c. 4 w.)	154
phosphate (1 w.)	101
phosphuret	68
sulphate (c. 4 w.)	140
sulphuret	72
Calcium	20
chloride	56
iodide	144
oxide (lime)	28
phosphuret	32
sulphuret	36
Calomel	236
Carbon	6
bisulphuret	38
chloride	42
perchloride	120
subchloride	48
hydruret	7
bihydruret	8
subhydruret	13
hydrochloride	50
oxide	14
phosphuret	18
Cerium	50
oxide	58
peroxide	62
Chlorine	36
oxide	44

deutoxide	60
peroxide	68
Chromium	28
oxide	36
deutoxide	44
Cobalt	26
chloride	62
iodide	150
oxide	34
peroxide	38
acetate	84
arseniate (4 w.)	132
carbonate	56
nitrate	88
oxalate	70
phosphate	62
phosphuret	38
sulphate	74
(c. 7 w.)	137
sulphuret	42
Columbium	144
Columbic acid	192
Copper	64
chloride	100
perchloride	136
iodide	188
oxide	72
peroxide	80
acetate	130
(c. 6 w. com. verdigris)	184
binacetate	180
(c. 3 w. distilled verdigris)	207
subacetate (1 acid + base)	210
carbonate	102
(2 w. malachite)	111
nitrate	188
phosphate	136
phosphuret	76
bisulphate (c. 10 w.)	250
sulphuret	80
Corrosive sublimate	272
Cyanogen	26
Fluorine	2
Glucinum	18
Glucina	26
Gold	200
chloride	236
perchloride	272
iodide	324
oxide	208
peroxide	224
sulphuret	248
Hydrogen	1
arsenietted	39
carburetted	7
bicarburet	13
bicarburetted (olefiant gas)	8
sesquicarburet (napthaline)	10
selenietted	41
sulphuretted	17
bisulphuretted	33
Hydruret of phosphorus	13
bihydruret	14
Iodine	124
Iridium	30
chloride	36
oxide	38 ?
peroxide	46 ?
Iron	28
chloride	64
perchloride	82
iodide	152
oxide	36
peroxide	40
carbonate	62
sulphate	76
(c. 7 w.)	139
sulphuret	44
persulphuret	60
Lead	104
chloride	140
oxide	112
deutoxide	116
peroxide	120
acetate	162
(c. 3 w.)	189
sub-binacetate	274
sub-tritacetate	386
arseniate	174
benzoate	232
carbonate	134
chlorate	188
chromate	164
subchromate	276
citrate	170
nitrate	166
oxalate	148
phosphate	140
phosphuret	116
sulphate	152

sulphuret	120
tartrate	178
Lime	28
acetate	78
arseniate	90
benzoate	148
carbonate	50
chlorate	104
chloride	64
chromate	80
citrate	86
fluate	38
hydrate (1 w.)	37
muriate (c. 5 w.)	110
nitrate	82
oxalate	64
phosphate	56
succinate	78
sulphate	68
(c. 2 w.)	86
tartrate	94
Lithium	10
chloride	46
iodide	134
oxide	18
sulphuret	26
Lithia	18
carbonate	40
nitrate	72
phosphate	46
sulphate	58
Magnesium	12
chloride	48
oxide	20
sulphuret	28
Magnesia	20
ammonia-phosphate	93
carbonate	42
hydrate	29
muriate	57
nitrate	74
phosphate	48
sulphate	60
(c. 7 w.)	123
Manganese	28
chloride	63
oxide	36
deutoxide	40
peroxide	44
acetate	86
benzoate	156
carbonate	58
oxalate	72
phosphate	64
phosphuret	40
sulphate	76
(c. 5 w.)	121
Mercury	200
chloride	236
perchloride	272
iodide	344
periodide	448
oxide	208
peroxide	216
nitrate	262
pernitrate	270
sulphate	248
persulphate	256
bipersulphate	296
sulphuret	216
persulphuret	232
Molybdenum	48
oxide	56
Nickel	29
chloride	65
iodide	153
oxide	37
peroxide	41
acetate	87
arseniate	99
carbonate	59
nitrate	91
oxalate	73
phosphate	65
phosphuret	41
sulphate	77
(c. 7 w.)	140
sulphuret	45
Nitrous oxide	22
Nitric oxide	30
Oxygen	8
Palladium	56
oxide	64
Phosphorus	12
chloride	48
perchloride	84
carburet	18
sulphuret	28
Platina	96
chloride	132

perchloride	168
oxide	104
peroxide	112
sulphuret	112
persulphuret	128
Potassium	40
chloride	76
iodide	164
oxide	48
peroxide	64
phosphuret	52
sulphuret	56
Potash	48
acetate	98
arseniate	110
binarseniate (c. 1 w.)	181
benzoate	168
carbonate	70
bicarbonate (c. 1 w.)	101
chlorate	124
chromate	100
bichromate	152
citrate	106
hydrate (1 w.)	57
iodate	212
molybdate	120
nitrate	102
oxalate	84
binoxalate	120
quadroxalate	192
phosphate	76
succinate	98
sulphate	88
bisulphate (c. 2 w.)	146
tartrate	114
bitartrate (c. 1 w.)	189
Rhodium	44
oxide	52
peroxide	60
Selenium	40
Silica	16
Silicium	8
Silver	110
chloride	146
iodide	234
oxide	118
suboxide	173
acetate	168
arsenite	172

arseniate	180
carbonate	140
chlorate	194
chromate	170
molybdate	190
nitrate	172
oxalate	154
phosphate	146
phosphuret	122
sulphate	158
sulphuret	126
Sodium	24
chloride	60
iodide	148
oxide	32
peroxide	36
phosphuret	36
sulphuret	40
Soda	32
acetate	82
(c. 6 w.)	136
arseniate	94
binarseniate (c. 5 w.)	201
benzoate	152
carbonate	54
(c. 10 w.)	144
bicarbonate	76
sesquicarbonate (c. 2 w.)	84
chlorate	108
chromate	84
citrate	90
hydrate (1 w.)	41
molybdate	104
nitrate	86
oxalate	68
phosphate	60
(c. 12 w.)	168
succinate	82
sulphate	72
(c. 10 w.)	162
tartrate	98
tartrate and potash	210
Strontium	44
chloride	80
oxide	52
phosphuret	56
sulphuret	60
Strontian	52
acetate	102

carbonate	74
chlorate	128
chromate	104
citrate	110
hydrate (1 w.)	61
muriate (c. 8 w.)	161
nitrate	106
oxalate	88
binoxalate	124
phosphate	80
sulphate	92
tartrate	118
tungstate	172
Sulphur	16
bicarburet	38
chloride	52
iodide	140
phosphuret	28
Sulphuretted hydrogen	17
Bisulphuretted hydrogen	33
Tellurium	32
chloride	68
oxide	40
Tin	58
chloride	94
perchloride	130
oxide	66
peroxide	74
phosphuret	70
sulphuret	74
persulphuret	90
Titanium	32
oxide	40
Titanic acid	48
Tungsten	126
oxide	142
Tungstic acid	150
Uranium	208
oxide	216
peroxide	224
Water	9
Yttrium	34
Yttria	42
Zinc	34
chloride	70
oxide	42
acetate	92
carbonate	64
chlorate	118
nitrate	96
oxalate	78
phosphate	70
phosphuret	46
succinate	92
sulphate	82
(c. 7 w.)	145
Zirconium	40
Zirconia	48

Article XI.

Remarks upon Dr. Christison's Memoir, "On the Detection of minute Quantities of Arsenic in mixed Fluids." By R. Phillips, FRS. L. and E. &c.

In the seventh volume of the *Annals*, New Series, I published a paper on the subject of arsenic, one object of which was to render the methods usually resorted to more easy of application, and another that of pointing out a method of destroying the colour of fluids suspected to contain it, by means of animal charcoal.

The Edinburgh Medical and Surgical Journal (July, 1824), contains a communication on the detection of minute quantities of arsenic, by Dr. Christison, Professor of Medical Jurisprudence. The author of this paper, after mentioning Orfila's method of decolorizing suspected fluids by the action of chlorine, observes, "Mr. Phillips, on the other hand, proposes to boil the suspected fluid with animal charcoal; and says, he has found that port wine, gravy soup, infusion of onions, or the *liquor arsenicalis*, may be rendered in that way sufficiently colourless for the application of the most delicate tests." The author then continues, "these processes do not seem to have been yet subjected to experiment by other chemists. I can conceive no other reason, at least, why the former in particular has been so long under the public eye without notice or criticism; for no one can have made fair trial of them, without being convinced that their application is confined by such narrow limits, and that, within those limits, they are liable to so many fallacies, as must render them almost entirely useless in medico-legal researches." With Dr. Christison's criticism upon Orfila's process I shall not interfere; but I will freely inquire, whether his remarks upon the method which I propose are entitled to the fairness to which their author lays claim; and it may be useful in this investigation to examine how far Dr. Christison is correct upon points on which he can have but little apology for error: this may serve as a guide to determine the value of his experiments, and of the inferences deduced from them.

After stating that the best substance for reducing arsenic* on the small scale is the black flux, he adds in a note, "almost all authors on chemistry and medical jurisprudence recommend, as an alternative, a mixture of charcoal powder and potass." On this subject, I have referred to the following well known authorities, viz. Black, Henry, Murray, Paris and Fonblanque, Brande, and Ure; and they mention no other substance than black flux for the purpose of reduction. Duncan and Aikens advise the use of charcoal, or carbonaceous matter; Beck recommends black flux made of carbonate of potash and charcoal, and the same mixture is advised by Mr. A. T. Thomson; while Smith and Orfila, as far as I have examined, are the only persons who employ a mixture of charcoal and potash.

In the note from which this passage is quoted, there is another assertion which appears to me to be very incorrect; it is that "the charcoal of the black flux is not necessary in the process; and subcarbonate of potass might therefore answer as well, but it is seldom so dry." Now if this were fact, it would be an important addition to our knowledge, for it would save the introduction of charcoal into the tube, and prevent it from being mistaken for sublimed arsenic. I did indeed find that when arsenious acid was heated with carbonate of potash, some metallic arsenic sublimed; and this arose from the conversion of part of the arsenious acid into arsenic acid, one portion of the arsenic taking oxygen from the other. That this is the true

* I take this opportunity of stating that I find my method of using an uncoated tube and a spirit lamp in the process of reduction is not original; for I have since observed that Mr. Brande recommends the same plan in his Manual.

explanation of what happened is rendered probable by an experiment of Dr. Wollaston's, as related by Dr. Thomson, in which he found that arsenious acid when heated with lime was converted into arsenic and arseniate of lime. This method, therefore, cannot be adopted with propriety, for as extremely minute portions of arsenious acid are usually operated upon, a very considerable proportion of it must remain in combination with the potash in the state of arsenic acid, and which would render the experiment of sublimation more decisive by increasing the quantity of sublimed metal, if charcoal were present to decompose the arsenic acid, or to prevent its formation; added to this, unless the heat be greater than required when charcoal is used, there also remains a large quantity of arsenious acid in combination with the potash; but after the long application of a strong red heat, and when the quantity of carbonate of potash used was five times greater than that of the white arsenic, arseniate of potash only appeared to remain in the crucible.

Having now shown that Dr. Christison's opinions on the methods of reducing arsenious acid are inaccurate, and that his statements of the advice of authors on that subject are incorrect, I shall proceed to notice his animadversions upon my proposal for using animal charcoal. The author twice asserts that I propose to boil the suspected fluid with animal charcoal. If Dr. Christison had not actually quoted the passage in which I describe the process in question, I should conclude that he had never read it, but had acquired an imperfect knowledge of it from hearsay. He does, however, quote it, and no mention whatever is made of *boiling* the suspected fluid; my words are, "I mixed some of it with animal charcoal." The fact is, that I merely agitate the mixture, as I presently again more particularly mention, and without heating it at all. Having shown what Dr. Christison has added to the process, I shall now notice what he has omitted. It is well known that animal charcoal contains muriatic salts; in order to apply the silver test, I direct that it should be washed. This necessary part of the operation must have been totally neglected by Dr. Christison: alluding to a solution which he had decolorized, I presume by *boiling* with animal charcoal, he says, "Lime water has no effect, the copper test produces an exceedingly scanty azure blue, and the silver test an abundant cream white precipitate." Now this abundant precipitate was evidently chloride of silver formed from the salt contained in the animal charcoal.

Thus then has Dr. Christison committed two errors, either of which would have been fatal to the experiment, and then assures his readers that "it was hardly necessary to search for the cause of the failure of Mr. Phillips's process;" and this is what the author considers "a fair trial." It is, perhaps, proper to inform Dr. Christison that I was acquainted with Mr. Thomson's experiments on the separation of arsenious acid from solution by means of charcoal, and I have guarded against it to a great extent by subjecting the solution to its action for a very short time, and without the application of heat. I was also aware that some fluids are incapable of being decolorized; and I stated the fact in my paper, without indeed naming them, from motives which may I think be understood.

I shall now state a few experiments which are, I think, conclusive as to the power of animal charcoal. I dissolved one grain of arsenious acid in 500 grains of port wine; to the solution when cold, I added 100 grains of washed animal charcoal, and agitated the mixture for about two minutes, and then filtered it. The solution was nearly colourless; I diluted a portion of it with water, and on the addition of a solution of sulphuretted hydrogen, the characteristic yellow colour appeared, although the arsenious acid formed only $\frac{1}{2500}$ of the solution. The copper test also readily indicated its presence when of the same strength, and the silver test readily detected the arsenic when so far diluted as to form only $\frac{1}{10000}$ of the solution.

I have already admitted that there are cases in which animal charcoal is powerless; but unless we are to attempt nothing because all cannot be performed, I retain my opinion that it may be useful, and as it decolorizes so deep a coloured fluid as port wine with facility, little doubt can, I think, be entertained of its efficacy upon the less usually intense colour of the fluid contents of the stomach.

There are some other parts of Dr. Christison's paper which would require notice if I were entering upon a general discussion as to the best tests to be employed; and I can by no means agree with him that the copper and silver tests "are the two most inaccurate and most fallacious of the common reagents;" on the contrary, I think, they may be used in many cases with great advantage, especially with the precautions and modifications which have been recommended by various chemists.

In concluding these observations, I readily acquit Dr. Christison of intentional misrepresentation, but I trust he will repeat the experiments, and candidly state the results, so as to compensate for the carelessness of which I have just cause to complain.

Article XII.

Suggestions for an improved Construction in the Air-pump. By Mr. Joseph Herries, Joiner. (With a Plate.)

(To the Editors of the *Annals of Philosophy.*)

Gentlemen, *Edinburgh, July* 15, 1825.

My attention having been called to the construction of the Air-pump, during the valuable lectures delivered in the schools of arts in this city, of which I am a student, some alterations have occurred to me as likely to render the instrument more perfect, and as these have been approved of by some persons of science to whom I communicated them, and who have informed me that they believe them to be new, I take the liberty of sending the annexed description and drawing for the purpose of laying them before the public, through the medium of the *Annals*. I am, Sir, your obedient servant,

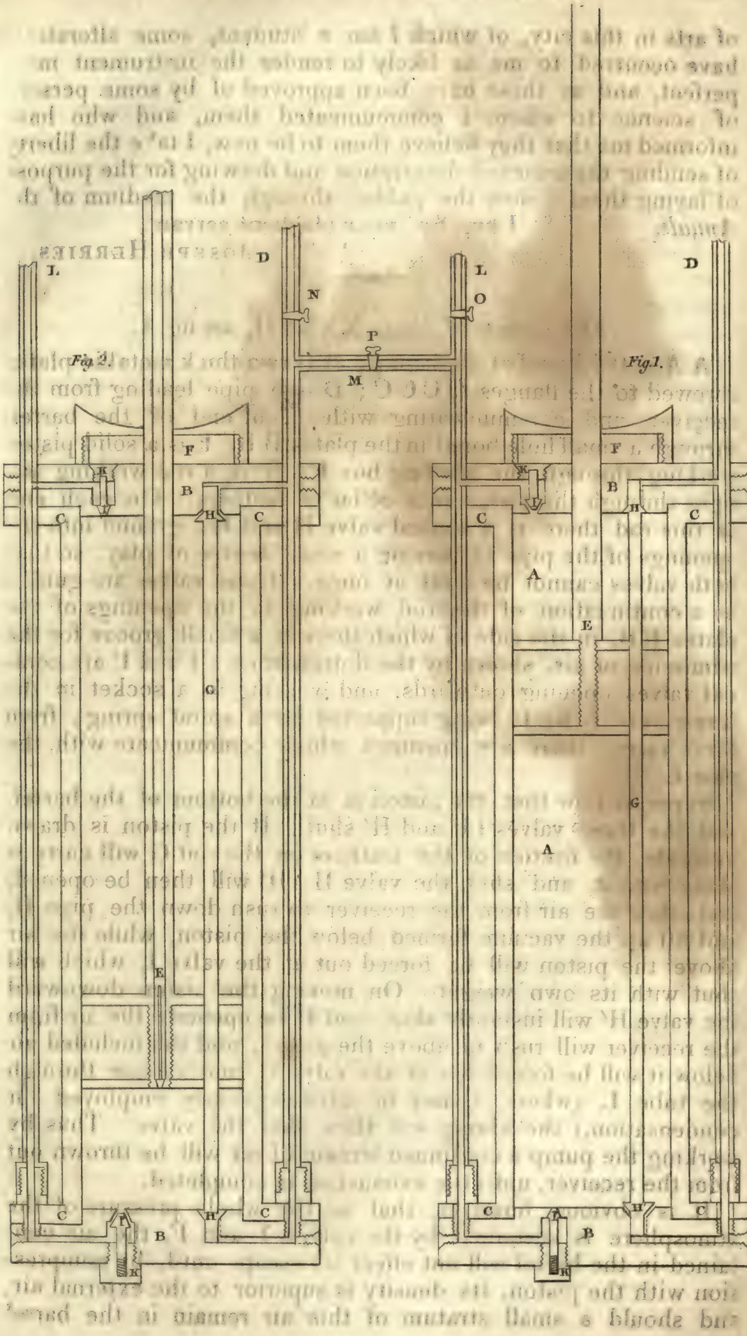
Joseph Herries.

Description of Plate XXXVII, *see* fig. 1.

A A is the barrel of the pump; B B two thick metallic plates screwed to the flanges C C C C; D is a pipe leading from the receiver, and communicating with each end of the barrel, through a small hole bored in the plates B B; E is a solid piston working through the stuffing box F; G is a rod working air-tight through the piston in a collar of leathers. On each end of this rod there is a conical valve H and H′, ground into the openings of the pipe D, having a small degree of play, so that both valves cannot be shut at once. These valves are guided by a continuation of the rod working in the openings of the plates B B, in the side of which there is a small groove for the admission of air, shown by the dotted lines. I and I′ are conical valves opening outwards, and working in a socket in the screw nuts K K, I′ being supported by a spiral spring; from these valves there are openings which communicate with the pipe L.

Suppose now that the piston is at the bottom of the barrel, and the three valves I I′ and H′ shut. If the piston is drawn upwards, the friction of the leathers on the rod G will carry it along with it, and shut the valve H; H′ will then be opened, and allow the air from the receiver to rush down the pipe D, and fill up the vacuum formed below the piston, while the air above the piston will be forced out at the valve I, which will shut with its own weight. On moving the piston downward the valve H′ will instantly shut, and H be opened; the air from the receiver will rush in above the piston, and the included air below it will be forced out at the valve I′, and escape through the tube L, (where it may be advantageously employed for condensation,) the spring will then shut the valve. Thus by working the pump a continued stream of air will be thrown out from the receiver, until the exhaustion is completed.

It is obvious, however, that as the whole pressure of the atmosphere is sustained by the valves I and I′, the air contained in the barrel will not effect its escape until, by compression with the piston, its density is superior to the external air, and should a small stratum of this air remain in the barrel



Engraved for the Annals of Philosophy, for Baldwin, Cradock & Joy, Octr. 1st 1825 excluded, its immediate access [illegible] must retard the exhaustion, and ultimately [illegible] the power of the pump.

To remedy this inconvenience, and [illegible] more perfect, I have added another [illegible] fig. [illegible], having the same letters of reference in the description [illegible] together by a connecting [illegible] these pipes there are three stop-cocks placed as [illegible] in the drawing. The pipes D D are connected together [illegible] L are also connected [illegible]

When this [illegible] and Q are opened, and P [illegible] in the usual way by [illegible] made to ascend while the [illegible] the pump will exhaust [illegible] that [illegible]

When the [illegible] as far as can be [illegible] air is drawn out, and [illegible] upon the stop-cock P and shut [illegible] Q, and continue working the pump.

The communication [illegible] being cut off by the cock [illegible] fig. 2, by the cock N, and the communication between these [illegible] easily seen by [illegible] ation of the plates, that the effect of fig. 2 must be [illegible] the air out of that part of the pipes D and [illegible] the stop-cocks N and Q. Consequently th[illegible] xternal air will be entirely removed from the [illegible] and allow the air in the barrel of fig. 1 to escape [illegible] ascending and forming a vacuum ready to receive [illegible]

By this contrivance the receiver may be almost [illegible] emptied of its contents. It is not, however, supposed that [illegible] connected barrel will be necessary, and on this supposition [illegible] I would recommend that the [illegible] forced downwards, the included air will escape through the valve, which, if [illegible] will sufficiently prevent its escape; and, of course, the pipe L with the valve [illegible] not be required.

unexcluded, its immediate access to the receiver must retard the exhaustion, and ultimately set a limit to the power of the pump.

To remedy this inconvenience, and render the machine still more perfect, I have added another barrel, figure 2, (having the same letters of reference in the description) which is joined together by a connecting tube M. On these pipes there are three stop-cocks placed as shown in the drawing. The pipes D D are connected together and enter the receiver as one. L and L are also connected for condensation.

When this double pump is worked the stop-cocks N and O are opened, and P is shut. The pumps may be wrought in in the usual way by a rack and pinion, the one piston being made to ascend while the other is descending. In this condition the pumps will exhaust each individually with a double stroke, so that two continued united streams of air will be thrown out.

When the exhaustion has proceeded in this way as far as may be judged necessary, say until $\frac{19}{20}$ths of the included air is thrown out, and a still more perfect vacuum be required, open the stop-cock P and shut the cocks N and O, and continue working the pumps.

The communication between the external air and figure 1, being cut off by the cock O, and between the receiver and fig. 2, by the cock N; and a communication being opened between these pipes, it will be easily seen by a slight examination of the plates, that the effect of fig. 2 must be to exhaust the air out of that part of the pipes D and L, situated below the stop-cocks N and O. Consequently the pressure of the external air will be entirely removed from the valves I and I′, and allow the air in the barrel of fig. 1 to escape freely; for as the one piston is ascending and forcing out air, the other is descending and forming a vacuum ready to receive it.

By this contrivance the receiver may be almost completely emptied of its contents. It is not, however, supposed that for common purposes, in an accurately constructed pump, this connected barrel will be necessary, and on this supposition (where a condensing apparatus is not required), I would recommend that the piston rod be formed of an open tube (see fig. 2), having a conical valve at the bottom, so that when the piston is forced downwards, the included air will escape through the valve, which, if wrought with oil, will sufficiently prevent its escape; and, of course, the pipe L. with the valve I′ will not be required.

Article XIII.

On the Analysis of Atmospheric Air by Hydrogen. By John Dalton, Esq. FRS. &c.

(To Richard Phillips, Esq.)

RESPECTED FRIEND, *Manchester, Sept.* 17, 1825.

According to my promise I transmit the results of some late experiments on the analysis of atmospheric air by hydrogen. My chief object was to find under what circumstances the union of the oxygen and hydrogen, by the electric spark, is complete, that is, so that one or both of the gases are entirely consumed; and in what cases either no union takes place or a partial one, leaving portions of both gases still in mixture in the residue.

From a memoir of M. M. Humboldt and Gay-Lussac (Ann. de Chimie, 53, 1805,) we learn that one volume of hydrogen, mixed with two or nine volumes of oxygen, gives the same loss by electricity, namely, 1·46; but if mixed with 9·5 oxygen, the loss is only ·68; and this loss diminishes rapidly till the oxygen becomes 16, when there is no loss at all. They found that if the surplus gas was azote or carbonic acid, the loss was not much different; but they do not seem to have ascertained this with precision.

It is right to observe that the hydrogen I used was obtained in the usual way from zinc and dilute sulphuric acid, and was received in bottles filled with as pure rain water as I could procure; the bottles were filled with the gas, and not more than one-third of the gas of each bottle was used; the hydrogen was free from atmospheric air, except what was expelled from the water by the hydrogen bubbling into the bottle: this quantity of atmospheric air, however, must be something; yet, on firing 10 measures of hydrogen with oxygen, the diminution is usually 14·6 to 15.

The mixtures of gases fired at once were commonly about 150 measures, each measure being the volume of one grain of water. The eudiometer has six inches in length, correspondent to 150 measures; and all the experiments were made over water.

The atmospheric air I mostly used was procured in the country, and was found by frequent trials to contain almost exactly 21 per cent. of oxygen. This is not the case at all times. I once found the oxygen as high as 21·15 per cent. from an average of many experiments; it was on the 8th of January last, when the barometer was 30·9, wind N.E. and very moderate, after three days of calm and gentle frost. But the general state of the atmosphere yields only 20·7 or 20·8 per cent. of oxygen. All the results below must be considered as averages of four or five experiments.

Tabular Arrangement of the Experiments.

Measures of oxygen.	Measures of mixtures.	Azote. Hydr.		Measures.
1 to	14·0	=(3·76 + 10·24)	Does not explode.	
1	13·0	=(3·76 + 9·24)	Fires imperfect.	Loss 2·7 .... o and h
1	12·0	=(3·76 + 8·24)	Fires perfect.	Ditto. 3·0 .... h
1	11·0	=(3·76 + 7·24)	Ditto.	Ditto. 3·0 .... h
1	10·0	=(3·76 + 6·24)	Ditto.	Ditto. 3·0 .... h
1	9·0	=(3·76 + 5·24)	Ditto.	Ditto. 3·0 .... h
1	8·0	=(3·76 + 4·24)	Ditto.	Ditto. 3·0 .... h
1	7·0	=(3·76 + 3·24)	Ditto.	Ditto. 3·0 .... h
1	6·0	=(3·76 + 2·24)	Ditto.	Ditto. 3·0 .... h
1	5·86	=(3·76 + 2·10)	Ditto.	Ditto. 3·0 .... n
1	5·76	=(3·76 + 2·00)	Ditto.	Ditto. 3·0 — .... o
Hydr.		Oxy.		
or 1	2·38	=(1·88 + 0·5)	Ditto.	Ditto. 1½ — .... o
1	3·0	=(2·37 + 0·63)	Ditto.	Ditto. 1½ .... o
1	4·0	=(3·16 + 0·84)	Ditto.	Ditto. 1½ .... o
1	5·0	=(3·95 + 1·05)	Ditto.	Ditto. 1½ .... o
1	6·0	=(4·74 + 1·26)	Fires rather imperfect.	Ditto. 1½ — .... o
1	7·0	=(5·53 + 1·47)	Fires imperfect.	Ditto. 1·0 .... o and h
1	8·0	=(6·32 + 1·68)	Ditto.	Ditto. 1·0 — .... o and h
1	9·0	=(7·11 + 1·89)	Ditto.	Ditto. 0·75 .... o and h
1	10·0	=(7·90 + 2·10)	Ditto.	Ditto. 0·60 .... o and h
1	11·0	=(8·69 + 2·31)	Ditto.	Ditto. 0·45 .... o and h
1	12·0	=(9·48 + 2·52)	Ditto.	Ditto. 0·30 .... o and h
1	13·0	=(10·27 + 2·73)	Ditto.	Ditto. 0·20 .... o and h
1	14·0	=(11·06 + 2·94)	Does not fire sometimes; fires at others.	Ditto. 0·20 .... o and h

N. B. Those experiments marked *h* denote that hydrogen remained in the residue after detonation; those marked *o* denote oxygen; and that marked *n* denotes that neither of the two was found.

On this table it may be remarked; 1. The rapid transition from no detonation to a perfect one, when the oxygen is near a minimum; 2. The slow transition at the bottom of the table when the hydrogen is near the minimum; and 3. That there is no imperfect combustion about the middle of the table; either the oxygen or the hydrogen, or both, are always entirely gone. But there is one anomaly that calls for explanation,—the neutralizing proportions of oxygen and hydrogen appear to be 1 to 2·1, when it is well known that they are nearly, if not exactly, as 1 to 2. This is occasioned, no doubt, by the impurity produced in the hydrogen; first, in the reception of it in bottles with water charged with atmospheric air; and, secondly, in the subsequent passing of it two or three times through water in the process for detonation. That this is the true reason is confirmed by the loss being only 3, and not 3·1 in that case;

and by the subsequent proof that more azote exists in the residue than ought to do, on the supposition the hygrogen was quite pure.

Such persons as are not familiar with this kind of experiment, and wish to repeat any of them, will do well to remember that whenever oxygen and hydrogen are mixed in *nearly the saturating proportions*, the mixture should stand some time (five minutes,) in order to allow of the perfect diffusion of the gases, before the spark is given.

I remain, yours truly, JOHN DALTON.

ARTICLE XIV.

ANALYSES OF BOOKS.

Journal of the Academy of Natural Sciences of Philadelphia, Vol. iv. Part 2.

THIS volume contains 22 papers on various subjects of Natural History, and is illustrated with eight plates, two of which (tab. 20 and 21) engraved by Mr. A. Lawson, of Philadelphia, from drawings by Lesereur and Mr. A. Rider, are the most beautifully executed plates of natural history subject which we have seen from any American art, not even excepting the celebrated plates of the *Wappity* and *Marmot,* by the same artist. These plates are lent to the society by Mr. Ord.

There are three papers relative to *Mammalia.*

1. An Account of a New Species of the Genus *Arvicola.* By George Ord, p. 305.

Arvicola raparius. Snout thick, blunt; eyes small; ears middle-sized; tail less than half the length of the body. This species is fond of the seeds of wild rice and oats, *zizania aquatica.*

2. A new Genus of Mammalia proposed, and a Description of the Species upon which it is founded. By T. Say and G. Ord, p. 345.

Genus NECTOMA; character, teeth $\frac{8}{8}$, cutting $\frac{2}{2}$, grinders $\frac{3\,.\,3}{3\,.\,3}$, with deep radicles; tail hairy; toes 4-5.

This genus is allied to Arvicola, and, indeed, Dr. Harlan has placed the species in that genus.

N. floridana. Snout elongated; eyes and ears very large; tail longer than the body. t. 21; length from snout to vent $7\frac{1}{2}$ inches; tail $6\frac{1}{4}$ inches.

This animal was first described by Mr. Ord in the Bulletin of the Philamaton Society as *Mus Floridanus.*

3. Description of a new Species of Mammalia whereon a Genus is proposed to be founded. By T. Say and G. Ord, p. 352.

N. G. Sigmodon. Teeth eight above, eight below, cutting two in each jaw; grinders six in each jaw, nearly equal with radicles, and with very deep alternate folds towards the summit; tail hairy; feet simple; toes 5-5, fifth front toes very small, clawed.

This genus is allied to *Arvicola,* and indeed Dr. Harlan has placed the species in that genus.

S. hispidum. Head thick; snout elongated; eyes rather large; ears large, round; tail nearly as long as the body; hairs of the upper part of the body long, coarse; length of body six inches; tail four inches. In the young species the black predominates, and in the adult the yellow. East Florida.

All the four papers on *Ornithology,* are by the late Prince Charles Bonaparte, certainly the best American ornithologist, whose recent death we have to lament.

The first is Observations on the Nomenclature of Wilson's Ornithology. This is the continuation of a series of papers in which the author corrects the synonima quoted, and adds numerous others, and corrects the systematic arrangement of the beautiful work of Wilson which he commenced completing.

The others are Descriptions of 13 new Species of South American Birds, p. 350, 370, and 387.

1. *Fringilla xanthorea.* Dusky; rump yellow; primaries edged with green; tail tipped with white; length $4\frac{1}{8}$ inches; bill like *F. serinus.* Rio Janeiro. The bird was tame, and sang like a canary, and, like other Antarctic birds, sang most in the winter.

2. *Monassa fusca.* Fuscous brown; shaft of the feathers yellow; primaries and tail feathers not spotted; throat spotted with white; chest with a black band. Tamatia brun. *Vaillant H. N. des Barbus,* t. 43. Tail with twelve feathers.

3. *Picus rubricollis, Gmelin,* Var.

4. *Dendrocalaptes Angusturostris, Vieill.* Fulvous brown, beneath white. All the feathers (except those of the throat) edged with black; beak elongated, slightly arched, compressed; length $7\frac{1}{2}$ inches.

5. *Fringilla flaviola, Lin.*

6. *Tanagra, flava, Gmelin.* Sericeous yellow; knees, throat, chest, middle of the abdomen, wings, and tail, yellow; primaries and tail edged with greenish blue; length $5\frac{1}{4}$ inches. Lindo bello, *D'Azara.* Tanagra cholroptera? *Veillot.* T. cayana, or *fem.? Desmarest.*

7. *Muscicapa violenta,* n. s. Tail six inches long, deeply forked; body grey, beneath white; head black; feather of the vertex go den yellow at the base. Tyranus Violenta, *Vicillot.*

8. *Muscicapa tæni optera,* n. s. Grey wings and tail; black

throat, abdomen; a broad band on the wings and apex of the tail white. Tyranus cinereus, *Vieillot.*

9. *Muscicapa pullata*, n. s. Grey wings, and forked tail black, outer edge of the outer feather white. This bird is a Platyrhyncos of *Desmarest.*

10. *Caprimulgus semitorquatus, Gmelin.* Black, and minutely speckled with red and white; first four primaries spotless, with an oblique red central band; neck with a white crescent beneath.

11. *Rallus nigricans, Vieill.* Greenish brown; back and wings olive brown; rump and tail black. Ypacaha obscuro, *Azara.*

12. *Garrulus ultramarinus.* Blue, beneath whitish grey; tail equal; length 13 inches.

13. *Icterus* (Cassicus) *melanicterus.* Black crested; rump and primaries, back and tail yellow; middle tail feathers entirely, the outer black on the exterior side; length $11\frac{1}{2}$ inches.

There are two papers on Ichthiology.

1. Description of Four New Species of the Linnæan Genus *Blennius*, and a New *Exocetus.* By W. W. Wood, p. 278.

Blennius geminatus. Head with a three-rayed beard over each eye; body with several pairs of brownish spots on the sides, above which are confluent marks on the back, extending a little way upon the dorsal fin; dorsal fin with an irregular blackish spot in front, D. 27, P. 13, V. 2, A. 11, C. $14\frac{2}{2}$ imperfect rays; length $2\frac{3}{8}$ inches, depth $\frac{1}{2}$ inch. Charleston Harbour.

Blennius punctatus. Head with a bifurcated cirrhus over each eye; dorsal fin with an irregular blackish spot between the first and third rays; body thickly covered with small blackish spots, which are confluent on the sides; caudal fin, with five obscure brownish spots, D. 27, P. 14, V. 3, A. 18, C. $11\frac{1}{2}$ imperfect rays; length three inches; depth 1 inch. Charleston Harbour.

Pholis novemlineatus. Body with nine whitish longitudinal bands; dorsal fin with an irregular longitudinal band; dorsal fin with an irregular blackish spot between the first and second rays, remainder of the fin clouded with dusky brown. D. 30, C. $12\frac{2}{3}$, N. 20, V. 2, P. 13; length $3\frac{1}{4}$; depth 1 inch. Charleston Harbour.

Pholis quadrifasciatus, n. *t.* 17, *f.* 1. Dorsal fin not joining the tail; body with four distinct brownish bands, and an interrupted obscure broad band on the neck; belly with four yellowish spots over the anal fin, ventral fin, fasciated with brown. D. 27, C. 9, A. 15, P. 11, V. 2. Length two and a half, depth six-eighths of an inch.

Exocetus appendiculatus, *t.* 17, *f.* 2. Lower jaw with a long trifuriated beard, the middle branch largest, extending about two-thirds the length of the body, the lateral branches very short. P. 13, D. 14, C. 18, 19, V. 6, A. 7. Length five and a quarter inches.

In *Erpetology* there are six papers, by Mr. Say and Dr.

Harlan, the latter gentleman appears to pay particular attention to this class of animals, and has given some interesting remarks on their internal organization, which will be inserted amongst the Zoological Notices in our next number.

1. On the *Fresh Water* and *Land Tortoises* of the United States. By Thomas Say, p. 203.

In this paper Mr. Say enumerates 15 species, one of which is new; and he has named, but not characterized two new genera; one for the Box Tortoise, under the name of *Cistula*, but this has been long established in the works of Merrem (which the American naturalists do not appear to know) under the name of *Terrapena*, and the other *Chelonura*, for the *Testudo Serpentina* of *Shaw*.

To the description of the species he has added some interesting remarks, relative to the habits of the animals, and the use made of them by the Americans.

The new species is *Emys biguttata*, Say. Shell oblong, oval, slightly contracted in the middle of each side, anterior marginal scuta, very narrow, linear, occiput with two very large fulvous spots; upper jaw naked; lower acute; tail rather long, simple. Length 4, breadth 3 inches.

2. Description of Three New Species of *Coluber*, inhabiting the United States. By T. Say, p. 237.

Coluber amœnus. Above brownish or blackish, beneath bright red, tail short, with an abrupt solid conic tip. Pensylvania. Plates 124—134, scales 24—38. Length 10 inches, tail 1½ inch. Var. a dark slate colour above.

C. rigidus. Dark fuscous or blackish; beneath the yellow, with two black lines. Southern States. Plates 111, scales 51. Length 20, tail 4 inches.

C. septemvittatus. Brownish, with three blackish lines, beneath yellow, with four blackish lines. Pensylvania. Plates 142, 143, scales 70—73. Length 10, tail 2½ inches.

The rest of the papers are by Dr. Harlan, the first of which is a " Notice of the Plesiosaurus, and other Fossil Reliquia, from the State of New Jersey, p. 232, where the author points out a new species of this interesting genns, and the three remainder are descriptions of the following new species of reptiles, two of which belong to a new genus, under the name of *Cyclura*, which was noticed in the last number, where it should have been placed directly after *Uromastrix*.

Cyclura carinata, n. s, t. 15. Crown of the teeth dentated, infra orbitar ridge with a row of horny scales; dorsal crest; warty between the scapula and over the sternum; scales of the body uniform, square, small, slightly imbricate, unarmed; scales of the legs and feet ending in minute spines; tail above keeled, middle slightly compressed, spiny bands ending four inches from the end of the tail.

Cyclura teres, t. 16. Teeth small, uniform and pointed; dorsal crest wanting only over the sacrum; scales of the sides, thighs and legs; bristled with minute spines; tail cylindrical, tapering, spiny rays, separated by two rows of depressed scales, without spines above. Spines on the rings nearly equal, extending to the end of the tail.

The *Lacerta acanthura* of Shaw is another species of this genus, and called Cyclura Shawii, by Mr. *Gray*. The genus is nearly allied to Uromastrix of *Merrem.*

Agama vultuosa, t. 19. Body grey; neck with a longitudinal pleat; tail round, long, scales rhomboidal, keeled, front of the back and hind part of the head slightly crested. Length 10 inches. This species belongs to the sub-genus *Calotis.*

Agama cornuta, t. 20. Body depressed, ovate, rough, above brown, variegated, beneath whitish; head above quadrangular; tail half as long as the body. Arkansas. Length four inches. This species is allied to Agama orbicularis, and belongs to the sub-genus *Tapayia.*

Sept. sexlineata, n. s, t. 18, f. 2. Body whitish, with three dark punctulated lines on each side, extending from the neck to the middle of the tail; head with 12 irregular unequal scales; toes two, unequal. Length four inches. This species forms the second species of the genus Bipes.

Scincus bicolor, n. s, t. 18, f. 1. Above fuscous, beneath silvery white, with two longitudinal white lines on each side; tail round; toes 5. Length 9½ inches.

There is only one paper relative to Amphibia, consisting of a Description of a New Species of Salamander. By W. W. Wood, p. 306.

Salamandra punctalissima, n. s. Greyish, entirely covered with numerous black dots, extremities long and slender; tail a little longer than the body. Length 3½ inches.

This volume contains Two Papers on Mollusca, one a very interesting paper, on the Float of *Jarthina*, of which we will give an extract among the Notices, and the other, a Description of a New Species of Modiola. By Thomas Say, p. 368.

Modiola opifex, n. t. 19, f. 2. Oval, reddish brown, anterior hinge margin, flattened, cordate; inside rather irridescent. Minorca, breadth one a half, length one-fifth of an inch. This species is allied to *M. Discors* by its shape. It lives in a cavity formed of fine agglutinated sand attached to shells.

In *Radiata*, Mr. Say has given an exceeding interesting paper, on Two Genera and several Species of Crioidea, p. 289, in which he forms a New Family.

N. G. Caryocrinites, *Say*. Column cylindrical, perforated by a tubular alimentary canal, pelvis formed of four plates, costals six, supporting the scapula, from which the arms proceed. This genus should be placed between the genera of

Cyathocrinites, and Actinocrinites; it contains two species given to Mr. Say, by Dr. Bigsby, who has lately brought one specimen of them to this country.

1. *C. ornatus.* Costals, four pentagonal and two hexagonal. 2. *C. loricatus.* Costal, five pentagonal and one hexagonal.

Mr. Say proposed a New Family should be formed for the *Kentucky Arterial fossil* of Parkinson, which he designates thus:

Fam. Blastoidea. Column composed of numerous articulating segments, supporting at its summit a number of plates connected, so as to form a calyciform body, containing the vicera. Arms none. Branchia arranged in Ambulacræ. This family appears to be a link between *Crioidea* and *Echinidæ.* It only contains one genus, established by Mr. Say, in Siliman's Journal, called,

1. Pentremite. Column cylindrical, perforated with irregular side arms, articulating surfaces radiated. Pelvis of three unequal species, two pentagonal, and one quadragonal. Scapulæ large, very deeply necked for the reception of the tips of the radiating ambulacra, obliquely truncated at the extremities, each side for the reception of one side of a subrhomboidal plates, or interscapular; Ambulacræ 5. Radiating from the summits, and ending at the tip of the necks of the scapula, each with a longitudinal, indented, line and numerous transverse striæ which end in a marginal series of pores, for the transmission of respiratory tubes (?) summits with five rounded opening (ovaries) and an angulated central one (mouth and anus).

1. *Pentremite globosa.* Body subglobular; sutures with parallel impressed lines. Bath, England. 2. *Pentremite pyriformis.* Body oblong; pelvis gradually attenuated. 3. *Pentremate florealis.* Pelvis ending abruptly, nearly horizontal. Encrinites florealis, *Schloth. petrif.* Kentucky Asterias Fossil, *Park, Org. Rem.* ii. t. 13. Kentucky, Common.

There are only two papers on Entomology, the first a Description of new Hymenopterous Insects, collected in the expedition to the Rocky Mountains, performed under the command of Major Long, by Thomas Say, p. 307; in which he describes two species of Gryllus. 1 Acheta, 1 Tridactylis, 6 Pectatoma, 2 Cydnus, 4 Carcus, 5 Tygæus, 1 Acanthia, 1 Tingis, 1 Aradus, 2 Reduvius, 2 Cerixa, 8 Cicada, 1 Tulgera, 2 Flata, 1 Delphax, 2 Ceriopis, 7 Tettigonia; but for the description of them, we must refer to the work itself. The second paper in this department is a Description of a New Species of *Trilobite.* By J. J. Bigsby, MD. p. 365. Found at Lockport, New York, and named after Lieutenant Bolton, of the Royal Engineers.

Paradoxus Boltoni, t. 23. Oval blind; surface with small tubercles, and striæ; clypeus rounded before; exterior angles extending in a broad spine; abdomen 14 jointed, segments recurved, falcate; tail membranaceous and serrated.

The remaining paper in the volume is the only one on Mineralogy, entitled, "Observations on the Zinc Ores of Franklin and Sterling, Sussex County, New Jersey." By G. Froost, MD. p. 220.

The first mineral described is a siliceous oxide of zinc, which externally appeared to have undergone a partial fusion. It agrees pretty well with the European species in the chemical composition, but it presents the primitive form of a strait rectangular four-sided prism, with a square base, or a cube. It only differs from the analysis given by Klaproth in containing three per cent. of oxide of manganese. Its specific gravity is 3·98 to 4·15.

It also contains some Observations on the Red Oxide of Zinc, on Jeffersonite, and on Carbonate of Zinc.

It is with pleasure we observe the regularity with which this work appears, and the excellent manner in which it is conducted. Considering that each of these volumes appears every six months, it cannot but greatly illustrate the natural history of America, to have such a number of species described, and their habits made known, in that short space of time.

Article XV.

SCIENTIFIC NOTICES.

Mineralogy.

1. *Petrifactions of Mount Carmel.*

The Rev. Pliny Fisk, American Missionary to Jerusalem, sent a quantity of Minerals from Palestine to Professor Hall, who has described them in the American Journal of Science. We copy a small portion of his paper:—

In a letter to the writer, Mr. Fisk remarks, "I had heard very often, that on one of the summits of Mount Carmel there were very curious petrifactions of fruit. The Arabs said, there were watermelons, and many sorts of smaller fruit, so perfect that, at first sight, you would take them for actual fruit. In my late journey from Jerusalem to this place, (Beyroot,) I determined to investigate this matter; and, with two Arabs who knew, or at least pretended to know, where the watermelons were to be found, I ascended the mountain. We found no watermelons, but we found, in the mountain which is formed of calcareous stone, some very curious formations, of which I send you several samples. I am not surprised that the ignorant Arabs should have mistaken them for petrified fruit."

They are, indeed, very extraordinary siliceous concretions. A number of fragments of different sizes were forwarded, together with one entire concretion. This I shall describe. It is about the magnitude of a twelve pound cannon-ball; not a perfect globe, and yet not deviating widely from that form. Its surface is a light, ash gray, and formed of chalky carbonate of lime, which effervesces on application of the nitric acid. It bears some resemblance in its aspect, to the nodules of flint taken from chalk quarries, and exposed a considerable time to the action of the elements.

By a smart blow of a hammer, it was divided in the middle. The interior thus laid open to the light presented several interesting substances. The outer layer, nearly an inch in thickness, consists of a yellowish gray hornstone, having a smooth fracture, and yielding sparks, easily and abundantly, with steel. This surrounds a thin stratum of very beautiful milk-white chalcedony. In the centre of the concretion is an irregular cavity, lined with very perfect crystals of limpid quartz. On one side of the cavity is a mass, an inch in diameter, of a light coloured friable limestone.

All the concretions are hollow; but the cavities in the different specimens are surrounded by different materials. In one, the inner surface is composed of translucent, and almost transparent botryoidal chalcedony. In another, the surface of the botryoidal chalcedony is covered with a white, smooth, unctuous, siliceous matter. In a third, it is surmounted by a countless number of elegant, pearly, microscopical crystals of quartz. In a fourth, is a small mass of semi-opal, containing cavities.

Allusion is unquestionably had to these stones, in a paragraph of Dr. Clarke's Travels. "Djezzar Pacha, of Acre," says he, "informed us that upon Mount Carmel, he had found several thousand large balls, and never could discover a cannon to fit them." In a note it is added, "We supposed that by these balls Djezzar alluded to mineral concretions of a spherical form, found in that mountain. As the Turks made use of stones, instead of cannon-shot, it is probable that Djezzar, who was in great want of ammunition, had determined upon using the stalagmites of Mount Carmel for that purpose." When I first read Clarke, I had not the most distant expectation of ever having the pleasure, personally, to examine specimens of these singular stones.

Professor Hall concludes by observing, that from the specimens sent him by Mr. Fisk, and the remarks of various travellers, it may be inferred that a large portion of Palestine is of limestone formation.

2. *On the flexible or elastic Marble of Berkshire County.*

Prof. Dewey describes this American marble as follows:—

"It has various colours, nearly white with a reddish tinge, gray, and dove-coloured. Some of it has a fine grain; other specimens are coarsely granular and have a loose texture. It is not uncommon for one side of a loose block to be flexible, while the other part is destitute of this property. It takes a good polish, and appears to be carbonate of lime, and not a magnesian carbonate.

"It is well known that Dolomieu attributed the flexibility of the marble he examined to its *exsiccation,* and that Bellevue ascertained that *unelastic* marble might be made elastic by exsiccation. The flexible marble of this county, however, loses this property in part on becoming *dry.* When it is made thoroughly wet by the operation of sawing or of polishing, it must be handled with great care to prevent its breaking, and the large slabs of it cannot be raised with safety unless supported in the middle as well as at the ends. The existence of this property is doubtless dependent upon the same general causes in marble as in other dense bodies.

"From the extensive view of marble given in Rees' Cyclopædia, flexible marble appears to be a rare mineral. One of the specimens I have lately obtained is to be sent by the Austrian Consul to the Imperial Cabinet of Vienna. As more specimens may doubtless be obtained at a reasonable expense, I would gladly aid those mineralogists who desire to procure specimens for their cabinets."—(American Journal of Science.)

3. *New and extraordinary Minerals discovered in Warwick, Orange County, New York.*

The following is an abstract from Dr. Samuel Fowler's paper, in the American Journal of Science:—

Every thing extraordinary in the valleys of Sparta, Franklin, and Warwick, belongs to the formation of crystalline limestone, which, perhaps, has no parallel in any other region of the world. Even Arendel and Wroe are inferior in mineral riches to this crystalline calcareous valley.

While recently exploring this formation, I made a discovery in the township of Warwick, Orange county, N. Y. of minerals, the most extraordinary for magnitude and beauty, which have ever yet come to notice. What will be thought of *Spinelle pleonasie,* the side of one of whose bases measures three to four inches, or twelve to sixteen inches in circumference? These crystals are black and brilliant, sometimes aggregated, at other times solitary; at this locality seldom or ever less than the size of a bullet. Some are partly alluvial, their matrix decomposing, but when unaltered they are found associated with what has never yet been described, namely, *crystals* of serpentine, slightly rhomboidal prisms of a magnitude parallel with the crystals of spinelle, often greenish and compact, at other times tinged yellow by an admixture of brucite.

These crystals bear not the smallest resemblance to the *mar-*

molite of Nuttall, erroneously referred to serpentine, on the mere ground of chemical affinity, by Mr. Vanuxem.

The magnitude of other crystals at this place (Warwick) is equally surprising as that of the spinelles. Crystals of *scapolite,* terminated, are to be found, each of the six faces of the prisms measuring four inches—or a circumference of twenty-four inches, or even more. They are of course rough and corroded; but the smaller prisms, often with narrow replacements on the edges, are very perfect and almost transparent—all of these slightly tinged with green.

In a very singular bed, subordinate to, and indeed in the crystalline limestone occuring in the form of a breccia of the old red sandstone, red graphic granite, and white feldspar, I have found partly diaphanous, softish, green octahedral crystals of considerable magnitude for which I know of no ascertained character. They appear almost similar in substance to *steatite,* being easily cut by a knife. They are not however found, as the *spinelle* of this locality, in carbonate of lime. Considering therefore this mineral as new, I propose to call it *Pseudolite,* in allusion to its affinity to the pseudomorphous crystals of steatite.

The following analyses are by Professor Gmelin, of Tubingen, in Wurtemberg:—

4. *Lepidolite.*

Silica	52·254
Alumina	28·345
Ox. of Manganese	3·602
Potash	6·903
Lithion	4·792
Fluoric Acid	3·609
	99·505

5. *Helvin.*

Silica	33·258
Glucine	12·089
Oxydule of manganese	31·817
Protoxide of iron	5·564
Sulphuret of manganese	14·000
	96·728
Loss by ignition	1·555

Zoology.

6. *On Lamouroux's New Division of the Animal Kingdom.*

Lamouroux, on the 7th of February, 1825, presented to the Linnæan Society of Calvados, a Treatise on a new Distribution of the Animal Kingdom, in which he proposed to divide it into two groups, thus:

1. *Aerozoces.* Living in air or water; organs of respiration double; water rarely useful, sometimes injurious; skeleton composed of articulated pieces; head always distinct; organs of locomotion formed of jointed pieces; lateral opposite, parallel, or in pairs, that is to say, symmetrical; nervous system dendroidal, very apparent, composed of a moniliform spinal marrow, each knot or joint of which received two trunks of the principal nerves; reproduction by union of the two sexes, separate on the different individuals. Dioicous.

2. *Hydrozoces.* Living in the water, or in a damp air; organs of respiration simple, or indistinct; water indispensable to all the individuals in all ages and in all states; skeleton not interrupted or wanting; head sometimes apparent, usually wanting; organs of locomotion never jointed, nor symmetrical, often wanting; nervous system slightly apparent, often invisible, without any spinal marrow, sometimes radiating very rarely with a cephalic ganglia. Reproduction by the union of unesexual being in some groups (Dioicous); by the union of bisexual in others (Hermaphrodite); and without sexual union in others (Agamous). In the last the reproduction is oviparous, gemmiparous, or fissiparous.

The first of these groups contains the *Vertebrata* and *Annulosa* of Cuvier, and the second the Mollusca Radiata and zoophites of the same author. *Lamx. Bul. Sci. Nat.* 1825.

The division is exactly the same as that proposed by Mr. W. S. Macleay, in his paper on certain Laws which regulate the Arrangement of Insects and Fungi, which was reprinted in the *Annals*, vol. vi. p. 324, and abridged into the Bulletin of Sciences for 1824.

7. *On the Horn of Plenty, a Variety of the Common Garden Snail.*

A most beautiful specimen of the monstrosity of the common garden snail (*Helix aspersa*) called the Cork screw, or Horn of plenty, on account of the whorles being separate from each other, so as exactly to represent the figures of the latter, was discovered a few months ago in a garden in Devonshire.

This monstrosity was first described by Born in his description of the shell in the Museum of Maria Theresa, where he formed it into a genus under the name of *Corni* (*Born Mus.* 362, t. 13, f. 10, 11, and *Vignette.* p. 361), and gave three good figures of the shell. Chemnitz added *monstrosum* to the named and copied figure of Born. Shaw, in his Naturalist's Miscellany, figured the shell, and under the name of *Cornucopiæ Helicina* (xiv. t. 518). Gmelin and Schroeter considered it as a species of *Serpula,* calling it *S. cornucopia,* and Mr. Dillwyn used the latter name, but confounded it with *Serpula helicina* of the Portland cabinet, which is *Magilus antiquus* of Lamarck, and does, as Mr. Humphreys describes, live in stones and corals; and Mr. Dillwyn, arguing from this fact, observes, that "the habitat which Mr. H. has given precludes the possibility of its being a distorted land shell." Ferussac, in his Synopsis of the Species of Snails, does not refer to these synonima, nor take any notice of the monstrosity.

Article XVI.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

An Essay on the Geological and Chemical Phenomena of Volcanoes, being the Substance of Two Lectures read before the University. By C. Daubeny, MD. FRS. Professor of Chemistry at Oxford.

Botanical Sketches of the Twenty-four Classes of the Linnean System, with Fifty Specimens of English Plants, &c. Post 8vo.

A Treatise on Epidemic Cholera, or Sketches of the Diseases of India, including Statistical and Topographical Reports, &c. By James Annesley, of the Madras Medical Establishment. 8vo.

A Practical Treatise on Poisons; forming a Comprehensive Manual of Toxicology. By J. G. Smith, MD. 8vo.

On the Digestive Functions, and the various Complaints incident to their disordered State; with a General View of Curative Dietetics. By J. A. Paris, MD. 8vo.

The Economy of the Eyes, Part II. of Telescopes, with an Abstract of the Practical Parts of Sir W. Herchell's Writings on Telescopes, Double Stars, &c.

An Anatomical Description of the Ligaments as connected with the Joints, with Observations on the Injuries to which they are liable. By Bransby B. Cooper, Esq. Lecturer at Guy's Hospital. Royal 4to. Plates.

JUST PUBLISHED.

Antediluvian Phytology, illustrated by Fossil Remains of Plants peculiar to the Coal Formation of Great Britain. By Edmund Tyrell Artis, FSA. and FGS. Royal 4to. 2*l.* 10*s.*

A Century of Surgeons on Gonorrhæa, and on Strictures of the Urethra. 12mo. 7*s.*

A Voyage towards the South Pole, in 1822—4; containing an Examination of the Antarctic Sea, to the 74th Degree of Latitude, &c. By James Weddell. 8vo. Plates. 18*s.*

The Works of the late Matthew Baillie, MD. with an Account of his Life. By James Wardrop. 2 vols. 8vo. 25*s.*

Report on the Mines in the Eastern Division of Hayti, and the Facilities of Working them. By W. Watton. 2*s*. 6*d*.

Report of W. Chapman, Esq. Civil Engineer, on the Manchester and Dee Ship Canal. Plates. 4*s*.

Medical Researches on the Effects of Iodine in Bronchocele, Paralysis, &c. By Alex. Manson, MD. 8vo. 12*s*.

Manuale Medicum, or Medical Pocket Book, for the Use of Students. By H. L. Sanders. 12mo. 5*s*.

An Introduction to the Use of the Stethoscope, with its Application to the Diagnosis in Diseases of the Thoracic Viscera, &c. By W. Stokes, MD. 6*s*. 6*d*.

Article XVII.

NEW PATENTS.

G. H. Lyne, John-street, Blackfriars-road, machinist and engineer, and T. Stainford, Grove, Great Guildford-street, Southwark, smith and engineer, for improvements in machinery for making bricks.—Aug. 23.

W. Parr, Union-place, City-road, Middlesex, for improvements in the mode of propelling vessels.—Aug. 27.

J. Bowler, Nelson-square, Blackfriars-road, Surrey, and T. Galan, Strand, Middlesex, hat-manufacturers, for improvements in the manufacture of hats.—Aug. 27.

C. Mercy, Edward's-buildings, Stoke Newington, Middlesex, for improvements in propelling vessels.—Sept. 8.

W. Jefferies, London-street, Radcliffe-cross, brass-manufacturer, for a machine for impelling power without the aid of fire, water, or air.—Sept. 15.

J. A. Teissier, Tottenham-court-road, for improvements in steam-engines.—Sept. 15.

C. Dempster, Lawrence Pountney Hill, for improved cordage.—Sept. 15.

G. H. Palmer, Royal Mint, civil engineer, for a new arrangement of machinery for propelling vessels through the water to be effected by steam or any other power.—Sept. 15.

A. Eve, South, Lincolnshire, carpet-manufacturer, for improvements in manufacturing carpets, which he intends to denominate Prince's Patent Union Carpet.—Sept. 15.

I. Lukens, Adam-street, Adelphi, machinist, for an instrument for destroying the stone in the bladder without cutting, which he denominates Lithontrepton.—Sept. 15.

Sir T. Cochrane, Knt. (commonly called Lord Cochrane) of Tonbridge Wells, Kent, for a new method of propelling ships, vessels, and boats, at sea.—Sept. 15.

C. Jacomb, Basinghall-street, wool-broker, for improvements in the construction of furnaces, stoves, grates, and fire-places.—Sept. 15.

ARTICLE XVIII.

METEOROLOGICAL TABLE.

1825.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
8th Mon.							
Aug. 1	E	30·10	30·08	92	62	—	
2	S E	30·09	30·08	82	55	—	05
3	S	30·08	29·73	80	62	—	05
4	S W	29·73	29·65	77	55	—	36
5	S W	29·76	29·65	75	55	·95	10
6	W	29·80	29·76	72	55	—	70
7	S W	29·81	29·80	72	53	—	
8	W	29·84	29·81	72	55	—	20
9	W	29·85	29·84	73	53	—	
10	S W	30·10	29·85	72	50	—	15
11	S W	30·10	30·08	74	52	·85	
12	S W	30·08	29·77	74	58	—	
13	S W	29·77	29·59	70	57	—	38
14	N W	29·63	29·59	72	55	—	03
15	N W	29·94	29·63	68	57	—	12
16	N W	29·98	29·94	73	52	—	
17	W	30·13	29·98	79	59	—	01
18	N W	30·31	30·13	72	56	·83	
19	N W	30·45	30·41	69	44	—	
20	N	30·45	30·42	72	52	—	
21	N W	30·42	30·40	84	51	—	
22	E	30·40	30·27	78	55	—	
23	N E	30·27	30·20	79	50	—	
24	N E	30·24	30·22	84	49	—	
25	N E	30·25	30·24	81	49	—	
26	S E	30·25	30·23	78	52	·92	—
27	E	30·23	30·14	62	58	—	56
28	N W	30·18	30·14	70	60	—	02
29	S W	30·21	30·18	70	58	—	20
30	S E	30·21	30·21	80	60	—	
31	E	30·21	30·20	85	54	·40	
		30·45	29·59	92	44	3·95	2·93

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Eighth Month.—1. Fine. 2. Showery. 3. Fine. 4, 5. Showery. 6. Rainy: some lightning with thunder about three, p.m. 7. Fine. 8. Showery. 9. Fine. 10. Showery. 11, 12. Fine. 13. Rainy. 14. Fine. 15. Cloudy. 16—20. Fine. 21. Fine: sultry. 22—26. Fine. 27. Rainy. 28. Cloudy. 29. Showery. 30—31. Sultry.

RESULTS.

Winds: N, 1; NE, 3; E, 4; SE, 3; S, 1; SW, 8; W, 4; NW, 7.

Barometer: Mean height

For the month.................................... 30·049 inches

Thermometer: Mean height

For the month.................................... 65·064°

Evaporation.................................... 3·95 in.

Rain.................................... 2·93

Laboratory, Stratford, Ninth Month, 26, 1825. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

NOVEMBER, 1825.

Article I.

On a Digest of the Plans of Ships in the British Navy. By John Major, Foreman of Chatham Yard, late of the School of Naval Architecture.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Chatham Yard, Oct.* 2, 1825.

Among the many plans that may be had recourse to for attaining a knowledge of the principles of naval architecture, it has appeared to me that none is so likely to produce the desired effect as a digest of the plans of ships in the British navy. By this is meant an analysis of their forms and equipments, and a comparison of their elementary compositions with the sea service of the ships.

To speak more particularly, I think the following elements of every sea-going ship in the British navy, if calculated and generally made known, would throw more light on this subject than any courses of experiments on resistance, on models of ships, or than any theoretical deductions alone, though conducted by the first rate mathematical genius. They are, the channel service, foreign and light displacements, or the weight of the whole ship when fitted for channel service, foreign expeditions, and the weight of the hull; the principal dimensions, viz. the length on the load water line, breadth and draught of water; the areas of the load water plane and midship section; the place of the centre of gravity of the displacement, or its distance from the load water line and the middle of the length of the ship; the centre of gravity of the ship and its contents, obtained by an experiment, which is here appended; the height of the metacentre at the mean height of ports out of water; the length of masts, and size of the sails, so that the whole surface of canvass, set with different strengths of wind, might be seen, together with the centre of effort of such sail; the weight of the metal on each deck, of the masts, rigging, ballast, water, and provisions; the moment of the guns out of water, or their weight multiplied into the distance of their common centre of gravity from the water, which is the best criterion of their force. The force of stability at 10° of inclination ought also to be calculated by Atwood's method, and it would serve in the experiment for finding the centre of gravity of the ship. Analytical research might be carried further than this at a more advanced period of naval architecture in this country, and ought to be; but at present, perhaps, the above outline should not be exceeded. When the analysis is interesting, Dr. Inman's calculation for ascertaining the form between wind and water to make the ship revolve round a longitudinal axis ought to be applied.

By documents in use at the Navy Office, the dimensions of the ship, of their masts, and the number of guns and men, with the draught of water, and an incorrect estimate of the tonnage, are already officially noted. The accounts of the ships there obtained are, however, from the infant state of the science of naval architecture in this country, not very minute, or adequately descriptive. It is impossible for one person to obtain by private calculation enough data to guide him sufficiently in designing ships, yet nothing more than what is stated is the result of official duties.

Although much has been done by the present naval administration in introducing scientific knowledge into our dock yards by the appointment of the students from the School of Naval Architecture to offices in them, yet it has not become the official duty of any one officer to be concerned in the theoretical constructions of ships. It, therefore, happens that the above elements recommended, are by no means generally known, some of them not at all, and most of those supposed to be so, imperfectly: the error is, therefore, as bad as ignorance; and hence has arisen the practice of building from foreign ships.

As the British navy contains ships of all nations, the investigation proposed would go far to exhibit a comparison generally of ships. It might be desirable also to obtain an analysis of some of the latest French and American ships, both merchant and martial.

In October, 1821, I submitted the above plan to the Honourable Navy Board, and they did me the honour to approve of it, by consenting to the execution of it by myself only, on account of economy. As the work, however, is sufficient for the physical exertions of six mathematicians for four years, with the requisite assistance of labour from the dock yards, such an approbation was abortive. It was announced to me in Oct. 1822, "that it was not considered necessary to prosecute the work any further at present." The object I had principally in view was to derive a theory of vessels from facts; in addition to this, it would afford correct official data for computation, and a navy which costs 15,000,000*l.* sterling in every ten years would have its construction founded on accurate estimates. I have ardently pursued the study of the subject since, and in consequence do not hesitate to state, that the government would save by it more than the value which the execution of the plan would cost, besides raising the dock yard service of the navy in scientific competition with that of foreign powers.

Col. Beaufoy and Mr. G. Harvey, of Plymouth, a few months ago, in the *Annals of Philosophy,* recommended a course of experiments on resistance as the only means of extending our knowledge of the scientific construction of ships. So strongly did the latter assert the necessity of it, that he said "all was darkness and uncertainty without it." It is my opinion, however, from the little advantage hitherto derived from such courses, and the difficulties of applying what knowledge could be obtained from them to ships, that it is by no means a promising track of pursuit for a theory of vessels. The maximum of the power of carrying sail must be united with the minimum of resistance, and both with the weight of hull, pitching, and rolling qualities, &c. When we consider the paucity of knowledge applicable to ship-building, arising from the efforts of the splendid constellation of genius that pursued the subject of the resistance of fluids in the French Academy for twenty years (from 1770 to 1790); the results of the ardent application of the Society for the Encouragement of Naval Architecture, in making 10,000 experiments for the same branch of knowledge; together with the failures of several other distinguished bodies and individuals:—our expectation from the institution of another course of experiments on resistance ought not to be very sanguine. To obtain the theory of resistance seems to be more in the department of a national learned body, as the resolution of a fine physical problem in mathematics, rather than as a work to be depended on for improvement in ship-building.

If we can ascertain the force or moving power of the sails acting at the *point velique* or resultant of the resistance, we may note 100 formal experiments on resistance in every ship that goes to sea; and this I believe it possible to obtain to a very near degree of approximation, probably as nearly as in any regular experiment on a model.

Again, if we have the resistance at a given velocity of a ship, which may be obtained by swinging a ship in a stream, and measuring the pull, we have the power of the sail acting at its centre of effort when this ship sails on the ocean with such a celerity as the given motion.

Ships sail on different lines of bearing; therefore the best form for resistance in one direction is not likely to be that in the other. The maximum of the power of carrying sail is also to be united with the minimum of resistance. The smallness of the ship for expense and saving of timber, the working by pitching and rolling, and the weatherly qualities, are all to be blended and properly considered in a ship. And this it appears to me can be only developed by the analysis of facts, and critical methods of comparison. In this manner a generalisation of principle would soon, on a little study, occur to a reflecting mind, and facts would check the speculating fancies which have hitherto been the principal ground for the different forms of ships.

The most important information we have respecting ships is that by increasing the principal dimensions of the various classes of ships, maintaining a similarly constructed body, we have faster sailing vessels. Conversely, if we similarly reduce the forms of ships, we have slower going vessels. This is derived from the observation of facts. And although the principle leads to greater expences, yet the superior quality of sailing, renders the adoption of increased vessels desirable. By this means three ships may expedite what four others do: they would also have the advantage of overtaking all weaker enemies, and avoiding fleets and more powerful ones. The importance of such ships was never so much shown as in the late American war, where six large frigates eluded an English navy of six line-of-battle ships and 30 frigates. For the last 200 years the principle has been increasingly acted on; the French have always preceded us in it, and still continue to do so.

The above feature in vessels is not the only one to be considered: there are others necessary to make a good ship. A ship of the line may be built of better qualities than our 74 gun ships, and cost 6000*l*. less. This the Swedes have effected through the efforts of Chapman, their great theoretical constructor of ships. The Swedish 74 is 350 tons less in weight of hull, which would make the saving just asserted, being 1250 tons, while ours are 1600 tons in weight. They are sufficiently strong to stand the storms of the Baltic for 20 or 30 years without considerable repairs, and carry one-fifth more weight of metal. The plan of floatation is larger, and the midship section considerably less: they carry more sail, so that most probably they sail faster by two knots an hour; they also carry more ballast. From three different authorities of unquestionable verity, I have it in my power to confirm these assertions by presenting the analysis of each.

Chapman will be of immortal memory in ship-building. Perhaps, next to Bouguer, who calculated the metacentre, and first established the true method of stability, he has rendered most service to naval architecture. He had not the advantage of early initiation into mathematics, but in mature life he made considerable progress in them, and exercised his knowledge with great effect. He appears to have applied himself with much energy to the study of the formation of ships by observing the effects of their different forms and equipments, after a similar plan to that laid down in this article, though not with such great advantages as improved calculations since afford, nor on so ample a field for observation as an analysis of the British navy. Neither did Sweden in the time of Chapman produce a *corps du génie maratime* of thirty students of naval architecture, of good mathematical attainments, and who have been devoted to the study of all the problems of the theory, as well as being acquainted with the practice of ship-building.

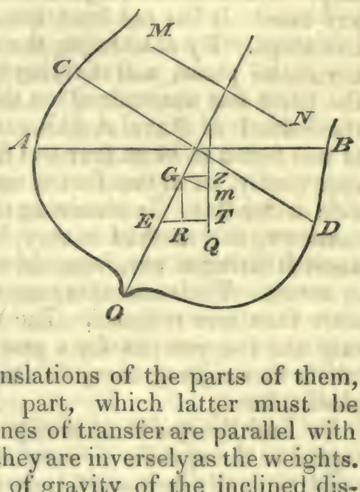
The plan is equally applicable to steam vessels. The French have already done this, by sending a mathematician of the name of Marastier over to America, in 1823, who has given the analyses of above 100 steam vessels, with a theory derivable from them.

The knowledge of the place of the centre of gravity of the ship and its contents, is of the greatest consequence. Most mathematicians have agreed that it is the centre of rotation in a ship. Without knowing it, the stability cannot be measured in any case. It has not been found in this country on more than two ships. By calculating the moments of the weights from a horizontal plane, and dividing by the whole weight of the ship, the point was ascertained on the Bulwark and on the Ajax, at the School of Naval Architecture, under Dr. Inman, in 1817. It was found to be at four feet five inches from the ports in each case nearly, or at one foot seven inches above the channel service water-line. In obtaining the point in this way, the objections are, the method is very long, and the specific gravity of wood differing at sea, from absorption and exhalation, it is liable to errors. The vertical moments are, however, highly useful for more than one purpose. The time of its calculation for each ship was two persons for a year each, besides the assistance of labour in weighing many of the component articles, as stores, blocks, &c.

To find the point at any period of a ship's service without regard to the specific circumstances of each component weight, must evidently be a most important acquisition. This was first proposed to be done by an experiment on the ship itself by Chapman, the eminent Swedish naval architect, in 1793. It has not been undertaken in this country for any ship. Chapman's mode of ascertaining the point has two objections belonging to it. He uses the metacentre as a measure of stability at an angle of 8° or 10°, which is decidedly erroneous. This is, however, easily corrected by substituting Atwood's equation of stability for it. The second objection is, that he has overlooked, apparently, the change of place of the centre of gravity of the ship by moving his guns on one side. This latter obscurity caused Mr. Charles Bonnycastle, late of the School of Naval Architecture, but now Professor of Natural Philosophy at Charlotteville, near Washington, Virginia, United States, who was the best mathematician belonging to our institution, to reject the proposition as illegitimate in its conclusions; and he bestowed considerable time in endeavouring to find it experimentally by other means. His attempts were, however, unsuccessful. The difficulty is here obviated by finding the new centre of gravity of the ship, and by investigating its line of transfer, we are enabled to ascertain the point in the upright position of the masts.

As Chapman's mode is performed by moving the guns and component weights of the ship, some naval architects have regretted the inconvenience of the method. This induced me to study another mode of effecting it by inclining the ship by a horizontal force applied to the masts, by which the weights of the ship are not disturbed, augmented, or diminished: it is here appended.

For the resolution of the problem for finding the centre of gravity of the ship, by moving weights horizontally, let C A O D B represent the bottom of the ship, A B its load water-line in the inclined position, C D that in the upright one. Suppose E to be the centre of gravity of the displacement, G that of the ship: let M be the place of the guns, which are transferred to N, in a direction at right angles to the masts.



Now the new centres of gravity of the displacement and ship may be found from the translations of the parts of them, the guns and newly immersed part, which latter must be equal to the emerged part. The lines of transfer are parallel with those of the parts, and in distance they are inversely as the weights. Suppose Q to be the new centre of gravity of the inclined displacement, and m to be that of the ship. Join Q m, and produce it to the plane of the masts. Now since the ship is in a state of quiescence, Q m is perpendicular to A B.

Draw G Z, E T, parallel to A B, and G R perpendicular to it. Then put V for the whole volume displaced of the ship in cubic feet of sea water; A for that of the immersed part by inclination, in the same measure; x for E G, the unknown distance of G from E; W for the weight of guns in cubic feet of sea water; d for M N, Δ for the angle of inclination; and b for the transfer of immersed part. We then have $G\,m = \frac{W\,d}{V}$ and G Z $= \frac{W\,d\,.\,\cos.\,\Delta}{V}$. G Z is also equal to E T − E R $= \frac{b\,A}{V} - x \sin.\Delta$. Hence,

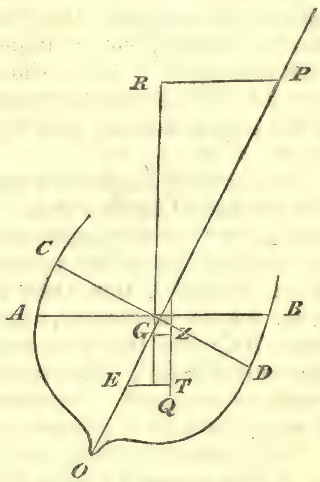
$$\frac{W\,d\,.\,\cos.\,\Delta}{V} = \frac{b\,A}{V} - x\,.\,\sin.\,\Delta.$$

$$x\,.\,\sin.\,\Delta = \frac{b\,A}{V} - \frac{W\,d\,.\,\cos.\,\Delta}{V}$$

$$x = \frac{b\,A - W\,d\,.\,\cos.\,\Delta}{V\,\sin.\,\Delta}.$$

To obtain the value of b, A and V, see Atwood's Stability.*

The other mode is for finding the centre of gravity of the ship from knowing the force of the sails, or any given power, with its place of action on the plane of the masts. It may be also used conversely. Thus, if we know the centre of gravity of the ship, we can tell the inclining power of the sails at a certain inclination.



Let a power P, measured in cubic feet of sea water, incline the ship a known height from the centre of gravity of the displacement, which represent by a. Let Δ be the angle of inclination of the vessel, G the centre of gravity of the ship, E that of the displacement, Q the new centre of gravity of the displacement. Then using the same notation as in the last proposition, G P $= a - x$, R T or G Z $= \frac{b\,A}{V} - x\,s$. Draw G R perpendicular to A B, and P R parallel to it. For this expression of stability, see Atwood's disquisition on the subject.

Now since the power which inclines the ship is equal to the buoyancy of stability, the vessel being at rest, P . $\overline{a - x}$. sin. Δ is equal to V . G Z. Or,

$$V\,.\,G\,Z = P\,.\,G\,R$$

$$V\,.\,\overline{\frac{b\,A}{V} - x \sin.\,\Delta} = P\,.\,\overline{a - x}\,.\,\sin.\,\Delta$$

$$b\,A - x \sin.\,\Delta\,V = P\,a \sin\Delta - P\,x \sin.\,\Delta$$

$$P\,x \sin.\,\Delta - x \sin.\,\Delta\,V = P\,a \sin.\,\Delta - b\,A$$

$$x\,.\,\overline{P\,.\,\sin.\,\Delta - V\,.\,\sin.\,\Delta} = P\,a \sin.\,\Delta - b\,A.$$

$$x = \frac{P\,a \sin.\,\Delta - b\,A}{P \sin.\,\Delta - V \sin.\,\Delta}.$$

* The theory of Stability, which consists in finding the distance of the vertical central line of buoyancy from the centre of gravity of the ship, is applied to all forms of ships by Atwood, in a disquisition on the subject in Phil. Trans. 1798, Part II. The investigation applies exactly to finding R T, which is equal to G Z above.

The foregoing sketch of an analysis of the ships of the navy, with a view to derive from it a body of experience to guide the designs of his Majesty's ships, includes all the principal elements of a ship's composition. There is no new calculation introduced, except Dr. Inman's for ascertaining the necessary form between wind and water to produce transverse motion in rolling, and the experiment for finding the centre of gravity of the ship and contents. A regard has been had to making the comparisons on a general and comprehensive scale, rather than on a minute reference to particulars, which do not materially affect the ship's qualities, and would render the calculations extremely diffuse. At a more advanced period of the science of naval architecture in this country, an analysis more refined in its parts may be used for comparing cases of particular interest, when the principal limits have become familiarly known.

The manner in which the inductive mode of philosophy is here applied to ascertain the principles of ship-building, from its extreme brevity, is more imperfect than it is thought the project itself is capable of being shown to be. In a future article some account of experiments on ships, to ascertain the relative velocity of the ship and wind, and the centre of mean resistance, will be given.

Our navy of England consists of 500 ships of war, of which 120 are line-of-battle ships. Of these, about two-thirds may be said to be "good conditioned ships for sea." The extent of the calculations, therefore, appears very great. It must be remembered, however, that there are only six different rates, which have, for the most part, the same masts, rigging, guns, provisions, &c.; and that in some cases 30 or 40 ships are built from the same draught. The variations are, therefore, not so great as might be imagined. Interpolations may also be used that will give results with a sufficient nicety.

The liberality which the Admiralty have extended to the institution to which I have the honour to belong, renders obligatory every exertion on our part to promote the object of their Lordships in the improvement of the navy; and I shall be extremely happy if the foregoing disquisition should effect it in a humble degree.

Article II.

Astronomical Observations, 1825. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Sidereal Time.

1825. Stars.	Transits.		
Sept. 20.—π Sagitt.	18h	59′	25·63″
20.—Moon's First or West Limb	19	36	52·17
21.—*f* Sagitt.	19	46	13·27
21.—57 Sagitt.	19	42	05·92
21.—*g* Sagitt.	19	48	05·48
21.—Moon's First or West Limb	20	00	40 36
21.—σ Capric.	20	09	21·60
21.—β Capric.	20	11	14·73
21.—π Capric.	20	17	22·44
21.—ρ Capric.	20	18	60·07
21.—τ Capric.	20	29	33·16
22.—β Capric.	20	11	14·81
22.—τ Capric.	20	29	33·55
22.—ε Aquarii	20	38	16·18
22.—325 Capric.	20	41	07·04
22.—μ Aquarii	20	43	17·22
22.—8 Aquarii	20	50	21·79
22.—Moon's First or West Limb	20	51	27·15
22.—γ Aquarii	21	00	07·88
22.—14 Aquarii	21	06	58·38
22.—18 Aquarii	21	14	41·78
22.—*e*¹ Capric.	21	35	44·89
27.—*d* Piscium	0	11	39·90
27.—110 Piscium	0	39	17·23
27.—Moon's Second or East Limb	0	44	44·47
27.—75 Piscium	0	57	26·07
27.—8 Piscium	1	02	23·94
28.—75 Piscium	0	57	26·91
28.—311 Piscium	1	01	00·48
28.—η Piscium	1	22	12·89
28.—101 Piscium	1	26	31·06
28.—Moon's Second or East Limb	1	31	38·88
28.—4 Arietis	1	38	47·28
29.—15 Arietis	2	01	01·71
29.—θ¹ Arietis	2	08	29·57
29.—Moon's Second or East Limb	2	20	14·54
29.—36 Arietis	2	34	39·03
29.—40 Arietis	2	38	49·24
29.—ρ Arietis	2	46	04·27
29.—δ Arietis	3	01	43·17

Sept. 17. Immersion of *g* Ophiuchi 18h 39′ 35″ Sidereal Time. Observation uncertain to three seconds.

Article III.

On some Observations by Dr. Brewster in the fifth Number of the Journal of Science, concerning the Crystalline Forms of Sulphate of Potash. By H. J. Brooke, FRS. &c.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Oct.* 15, 1825.

It is only within the last week that I have seen an article relating to myself in the fifth Number of Dr. Brewster's Journal of Science, p. 147, containing insinuations and assertions which are wholly unsupported by fact.

The article in question is one of those which Dr. Brewster occasionally inserts under the title of "*Decisions on disputed Inventions and Discoveries.*"

On the general question of *original discovery,* we may borrow from Dr. Brewster's own case an illustration of what ought or ought not to be regarded as such.

Dr. B. either did or did not pilfer the kaleidoscope from Bradley. If he did not; if, during the industrious and extensive researches to which, as the editor of an Encyclopædia, we may conceive him to have been led, he did not happen to meet with Bradley's volume before he discovered the principle of the instrument himself; and if on this ground he claims the merit of being an *original discoverer* of a principle which was *already known;* it would in him be no more than an exercise of common candour to concede to *other second discoverers* an equal claim to originality, except indeed in those instances in which there is strong moral presumption, if not direct evidence of plagiarism.

The article I have alluded to is the following:—

"Our mineralogical readers are no doubt aware of the bypyramidal form in which sulphate of potash often crystallises. Count Bournon considered this the primitive form of the salt. In a paper in the *Annals of Philosophy,* Mr. Brooke has described this form of the salt, and shows that it is a composite form, consisting of rhomboidal prisms *combined in the manner which he has represented in a diagram.*

"This composite form had been discovered long before by the agency of polarised light, and *the combination distinctly described* in the first paper of No. I. of the Edinburgh Philosophical Journal.

"*As Mr. Brooke has made no reference whatever to that paper, it might have been presumed that he had not read it.* But we find that he has actually read it and quoted it in his lucubrations on the structure of apophyllite, with which he has favoured the public; and which have already shared the same fate as his speculations on the primitive form of the sulphato-tri-carbonate of lead."

Now the insinuation which this article is intended to convey, standing as it does among the notices of "*disputed discoveries,*" is, that I have assumed the credit of discovering something which was already known, and had been previously discovered by Dr. Brewster.

But this insinuation is unfounded. It is not true that Dr. Brewster had, as he asserts "*distinctly described*" the combination in question in the paper he refers to. Nor, unless Dr. Brewster has learned something more on the subject since he wrote that paper, does he even now understand how the bypyramidal crystals are formed.

The following is the short description I gave of this salt in the *Annals of Philosophy* for January, 1824:—

Sulphate of Potash.

The primary form of this salt was, I believe, first determined by Mr. Levy to be a right rhombic prism, and described in No. 30 of the Royal Institution Journal; but probably from not possessing sufficiently explanatory crystals, Mr. L. has not pointed out *the relation of its primary form to the bi-pyramidal figure under which it generally occurs.*

I have been enabled to do this in a very satisfactory manner by means of a compound crystal which I have obtained from the solution of a portion of this salt in distilled water.

Fig. 1.

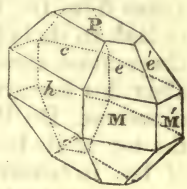


Fig. 1 is a single modified crystal.

M on M′	120° 30′
M on *h*	120 45
M on *e*	146 22
h on *c*	146 10
c on *c″*	112 20
e on *e′*	131 12

Fig. 2.

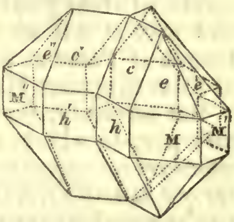


Fig. 2 is the compound crystal, which consists of three single crystals, so united that their upper edges meet at angles of 120°, and consequently their planes of junction incline to each other at the same angle. Hence

M on M″	119° 30′
e on *e″*	130 24

There is not in this brief notice any attempt to set myself up as the discoverer of the composite character of the bypyramidal crystals. On the contrary, I suppose that fact already known, and the evident object of the notice was merely to point out the *precise relation* of the *simple* to the *compound crystal* which had not to my knowledge been previously ascertained.

The reason why I did not refer to Dr. Brewster's paper on the same subject was, that I knew it to be incorrect both in measurement and description, and felt at that time no particular motive to expose these inaccuracies, nor should I have noticed them now if the task had not been forced upon me by Dr. Brewster. The prismatic planes which commonly appear on the simple crystals of this salt are those marked *c* and *c″* in fig. 1, the mutual inclination of which is 112° 20′ very nearly, but according to Dr. Brewster it is 114°—an error of not much more than a *degree and a half*, and which may, perhaps, not be an uncharitable measure of Dr. Brewster's ordinary precision.

Dr. Brewster also supposes these planes *c* to remain upon the compound crystal, whereas it is evident from fig. 2, that they entirely disappear from that form. The measurement of the planes *e* over M, in the bypyramidal crystal, is 112° 44′ very nearly, instead of 114° as quoted by Dr. Brewster.

Dr. Brewster also alludes in his paper on this salt to crystals with *one axis* and crystals with *two axes* of double refraction,—a fact which seems to place another stumbling block in the way of Dr. Brewster's optical system, and which affords additional evidence that the optical characters of minerals are liable to modifications from causes not yet understood.

I would have here closed my observations upon the article quoted above from Dr. Brewster's review, had not the Doctor chosen to tack to its tail what I suppose he intended for its sting—an observation about *Apophyllite,* or rather, as I suppose he means, about his favourite *Tesselite.* On this mineral, as I have shown elsewhere, Dr. Brewster has allowed his imagination to revel to the top of its bent; and whatever may be the fate of any other of Dr. Brewster's novel speculations, his extraordinary discovery in crystal building* relative to this imaginary species, is not likely to have any other claimant, or ever to find its way among "Decisions on *disputed* Inventions." It will remain a memorial of the great extent of the Doctor's knowledge of nature, and may at last be fortunate enough to occupy a niche in the temple of Fame as a companion to the celebrated *optical system* of Miss Margaret Macavoy.

But seriously, for really these sprightly effusions of the Doctor's pen, in which, as regards myself, he has indulged himself wherever an opportunity has been afforded him, scarcely merit a serious attention, if the claim of *Tesselite* to be ranked as a separate species had any real foundation, it might have been expected to be so distinguished in the latest work on mineralogy which has been published in this country, I mean that by Dr. Brewster's coadjutor, Mr. Haidinger.

But the observations of this gentleman on *Tesselite,* although they are, perhaps, calculated to soothe the froward philosopher, do not compromise his own judgment as a mineralogist.

* The following extract from a paper by Dr. Brewster in the Edinburgh Philosophical Transactions for 1823, announces the discovery here alluded to:

"*The Tesselite could not,*" Dr. Brewster says, "*have been formed by the ordinary process of crystallization, but that a foundation appears to be first laid by means of uniform homogeneous plates, the primitive form of which is pyramidal; a central pillar whose section is a rectangular lozenge, then rises perpendicularly from the base, and consists of similar particles. Round this pillar are placed new materials, in the form of four trapezoidal solids, the primitive form of whose particles is prismatic, and in those solids the lines of similar properties are at right angles to each other. The crystal is then made quadrangular by the application of four triangular prisms of unusual acuteness. These nine solids arranged in this symmetrical manner, and joined by transparent lines performing the functions of a cement, are then surrounded by a wall composed of numerous films, deposited in succession, and the whole of this singular assemblage is finally roofed in by a plate exactly similar to that which formed its foundation.*"

He thus disposes of the mineral in question. "Dr. Brewster has observed that in certain varieties (of apophyllite) to which he has given the name of *Tesselite,* the phenomena of double refraction cannot be explained upon the supposition of a single axis, and even the properties of the mineral are not uniform in this respect throughout the whole mass, but that it appears to be composed of various parts acting differently upon light. It will depend upon a *future* accurate examination of the *crystalline forms* and *other properties* of this substance in comparison with *these observations,* whether *they* will *concur* in fixing the limits of the species, or whether *this* will *depend solely* upon the *optical structure of the mineral.*"

Thus the *fate* of my *lucubrations* on the structure of apophyllite, which, it will be recollected, went to show that the optical characters of minerals were not yet sufficiently understood to be relied upon for the discrimination of species, is their distinct recognition by one of the best mineralogists of the present day.

H. J. B.

Article IV.

Notice on the Temperature of the Surface Water of the Atlantic, observed during a Voyage to and from Jamaica. By H. T. De la Beche, Esq. FRS. &c.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

My principal object in presenting the annexed notice of temperatures for insertion in the *Annals* is to induce some of the many persons who so often traverse the Atlantic to and from our West Indian Colonies, to make similar observations (than which nothing can be more easy) respecting the temperature of the surface water during their voyages, as we can only arrive at any thing like a satisfactory theory on this subject from accumulated observations, made at different seasons of the year, and in various parts of the Ocean.

The following observations were made at noon each day, the temperature of the surface water being found by plunging a thermometer into a bucket of water just taken from the sea, and that of the air being ascertained on deck, and in the shade.

Currents must of course have considerable influence on the temperature of the surface water, for instance, it seems probable that the continuation of the Gulf Stream raises the temperature to the southward of the Great Bank of Newfoundland.

As it is not, however, my present object to enter more fully on this subject,* I shall content myself by adding the following list of temperatures, observed during my voyages, trusting that they may be found useful by adding a few facts to our information respecting the surface temperature of the Atlantic.

Observations made during a Voyage from England to Jamaica in the Ship Kingston.

1823.	Latitude.	Longitude.	Temperature. S. Water.	Temperature. Air.	Wind.	
Nov. 10	50° 40′ N	5° 30′ W	52°	51°	SE	Fine breeze; clear weather.
11	48 23	10 4	56	53	SSE	Fresh breeze.
12	46 00	12 43	56	56	SSE	Moderate.
13	43 34	15 3	60	59	SE	
14	41 12	17 30	61	62	E	Fine breeze.
15	38 20	19 0	65	61	E	Squalls.
16	36 40	19 29	66	63	Calm	
17	36 28	20 16	65	67	SSW	Light wind.
18	35 22	21 59	67	64	NE	Fine breeze.
19	33 28	22 26	68	66	W	Light wind.
20	33 12	23 10	68½	70	SSW	Ditto.
21	33 10	23 30	68	67	Calm	
22	32 54	24 0	68	71	Var.	Light winds.
23	31 31	24 42	69	71	NE	Moderate.
24	29 45	26 18	71	75	E	Ditto.
25	28 00	27 51	72	76	SE	Fine breeze.
26	26 40	29 7	74	77	S	Moderate.
27	25 39	31 2	73½	74	S	Fresh wind; gale at night.
28	25 00	32 49	75	69	S	Gale.
29	24 50	34 4	75	76	SE	Strong breeze; gale at night.
30	24 11	36 38	75	75	—	Gale.
Dec. 1	23 13	38 43	75½	77	SE	Strong breeze; gale at night.
2	22 40	40 56	77	77	—	Gale.
3	21 39	42 56	77½	78½	SE	Fine breeze; thunder storms at night.
4	21 10	44 16	77	77	SE	Light winds and calms.
5	21 22	45 41	77½	77	NW	Light airs and calms.
6	21 6	46 9	77½	78½	S by E	Light breeze.
7	21 23	47 7	78	75½	NW	Light winds and calms.
8	20 34	47 18	79	78	Calm	
9	20 6	48 22	79	77	NE	Moderate.
10	19 0	50 20	79	77½	NE	Fine breeze.
11	18 1	52 47	79½	77½	NE	Ditto.
12	17 9	54 57	79½	79	NE	Ditto.
13	16 34	58 14	80	80	ENE	Ditto.
14	Off the N side of Guadaloupe.		80½	79	ENE	Ditto.
15	16 42 N	64 4 W	80	80	E	Ditto.
16	16 52	66 32	80½	80½	E	Ditto.
17	16 56	69 9	81	81	E	Ditto.
18	17 18	72 30	81	80	E	Strong breeze.
19	17 38	75 3	80½	80	E	Fine breeze.
20	Off Fort Royal, Jamaica.		82	83	SE	

* For the same reason I refrain from comparing the temperatures that I obtained with those that have already been noticed by others.

Observations made during a Voyage from Jamaica to England, in his Majesty's Packet Francis Freeling.

Date.	Latitude.	Longitude.	Temperature. S. Water.	Temperature. Air.	Wind.	
1824.						
Dec. 30	18° 54′ N	74° 58′ W	81°	83°	Var.	
31	19 30	74 43	81½	80½	Ditto.	Light breezes.
1825.						
Jan. 1	19 42	74 48	83½	81	Calm	Find W wind in the evening.
2	19 56	73 31	81	79	N	Gale.
3	20 9	73 45	78	77	NE	Strong ENE in the evening.
4	22 8	74 24	78	76	E	Fine breeze.
5	23 32	74 12	78	77	E	Light wind.
6	24 27	73 19	77	74	SW	Fine breeze.
7	25 1	71 19	75	73	N	Fresh breeze.
8	25 24	70 40	75	70	NNE & E	Squally.
9	26 53	69 50	75	69	Var.	Fresh.
10	28 18	67 34	71	73	SW	Light breezes.
11	28 21	66 18	72	71	ENE	Fresh breeze.
12	29 52	66 00	72	70	Var.	Fine breeze.
13	31 20	63 20	69½	69	SSE	Ditto.
14	32 54	60 24	68	66	S	Strong breeze.
15	34 32	58 39	67	64½	SE	Ditto.
16	35 35	55 40	66	67	SW	Moderate.
17	36 33	53 8	66½	69	SW	Ditto.
18	37 45	51 5	66½	66½	SW	Ditto.
19	39 17	47 54	67	65	Var.	Strong wind.
20	40 22	44 30	66	56½	N	Moderate.
21	40 37	43 9	65	63	NE to SSE	Squally.
22	41 49	40 33	62	64	SSW	Moderate.
23	43 18	37 29	62	61	W	Ditto.
24	43 18	34 53	61	63	SW	Moderate; fog.
25	44 30	30 50	60	62	SW	Gale.
26	45 47	26 4	57½	60	WSW	Strong wind.
27	46 24	21 17	57	54	Var.	Ditto.
28	47 00	19 0	55	55	Ditto.	Moderate.
29	47 45	15 4	53½	52½	S	Ditto.
30	48 41	11 8	52½	52	SW	Ditto.
31	49 21	8 5	51	52	SW	Fresh breeze.

Article V.

On the Value of Leases.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

The following question being one of very frequent occurrence, and having met with no solution of it, you will oblige me by the insertion of that which I now send.

A leasehold estate, after deducting ground rent, produces annually a clear improved rental of (a). It is renewable every

(n) years on the payment of a fine of (b). What is the lease worth, considering it as a perpetuity subject to the ground rent and renewal fines, and allowing interest at r per cent.?

Solution.

Let R = the amount of 1*l.* at the end of one year at r per cent.; then will $\frac{a \times \overline{1-\frac{1}{R^m}}}{R-1}$ be the present value of an annuity (a) to continue for (m) years. But if (m) be infinite, then will $\frac{1}{R^m}$ be $= 0$, and the present value of a perpetual annuity a will be $\frac{a}{R-1}$.

At the end of m years, the amount of the fines and the interest thereon will be $b \times R^m + b \times R^{m-n} + b \times R^{m-2n}$, &c. $= b \times (R^m + R^{m-n} + R^{m-2n}$, &c.$) = F$. Let p represent the number of times n is contained in m without remainder, and put $R^m + R^{m-n} + R^{m-2n} \ldots\ldots R^{m-pn} = S$. Then dividing this equation by R^n we have $R^{m-n} + R^{m-2n} + R^{m-3n} \ldots\ldots R^{m-\overline{p+1}.n} = \frac{S}{R^n}$; and by subtracting the latter equation from the former, $R^m - R^{m-\overline{p+1}.n} = S - \frac{S}{R^n}$, and $S = \frac{R^m - R^{m-\overline{p+1}.n}}{1-\frac{1}{R^n}}$. F, the amount of the fines and interest thereon will therefore be, $= b \times \frac{R^m - R^{m-\overline{p+1}.n}}{1-\frac{1}{R^n}}$, and since 1*l.* present money is equal to R^m to be paid at the end of m years, $R^m : 1 :: \frac{R^m - R^{m-\overline{p+1}.n}}{1-\frac{1}{R^n}} : \frac{R^m - R^{m-\overline{p+1}.n}}{\overline{1-\frac{1}{R^n}} - R^m}$, the present value of the fines $= \frac{1}{1-\frac{1}{R^n}} - \frac{R^{m-\overline{p+1}.n}}{\overline{1-\frac{1}{R^n}} \times R^m} = \frac{1}{1-\frac{1}{R^n}} - \frac{1}{\overline{1-\frac{1}{R^n}} \times R^{\overline{p+1}.n}}$. But if m be infinite, p will be so too, and the quantity $\frac{1}{\overline{1-\frac{1}{R^n}} \times R^{\overline{p+1}.n}}$ being infinitely small will disappear from the equation, and $\frac{b}{1-\frac{1}{R^n}}$ will be the present value of the fines on a perpetuity. The present value of the lease will, therefore, be $\frac{a}{R-1} - \frac{b}{1-\frac{1}{R^n}}$. W. B.

Article VI.

An Outline of an Attempt at the Disposition of Mammalia into Tribes and Families, with a List of the Genera apparently appertaining to each Tribe. By J. E. Gray, Esq. FGS. &c.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *British Museum.*

Although popular curiosity is almost exclusively confined to the study of the manners of this class of animals, an eminent zoologist has observed, that notwithstanding the anatomy of the Mammalia has had infinitely more attention paid to it than that of all the rest of the organized creation put together, it is not too much to say that their natural arrangement is as little or even less known than that of any other part of zoology.

Indeed Illiger and Cuvier are the only zoologists, since the time of Linnæus, who have paid attention to the classification of Mammalia. The arrangement of the former is professedly artificial, and of that of the latter, the above quoted zoologist has observed, that no where at least do we find inconsistencies so conspicuous as in the following order (quoting that of Cuvier), which is that nevertheless of the most learned comparative anatomist in existence.

I have found the orders of Linnæus, which are merely a paraphrase of those proposed by Ray, to be exceedingly natural, and several of my families have been established as orders and genera by Cuvier and others. In the following sketch, the disposition is more novel than the families themselves, except in the order *Glires,* where I have attempted (but not very successfully I am afraid), to re-model them entirely, and to divide them according to their general habits. In so doing I placed the genera together, in what I considered natural tribes, and then threw them into what appeared to be natural groups, and have attempted to find out some character common to the tribes by which these groups might be distinguished; but much more is wanting to be known respecting the genera of this order.

I have added to each of the tribes a list of the published genera which have come to my knowledge, with the name of the original describer.*

§ 1. *Teeth of the three distinct sorts, and forming a continuous series.*

Order I.—Primates, *Lin.*

The anterior, and the hinder extremity, with a distinct and

* The Mammalia at present in the Museum amounting to about 200 species, are disposed, as far as is consistent with their being well seen in the present confined space, according to the following arrangement.

opposite thumb; claws flat, small; grinders uniform, tubercular; condyle of the jaws round; orbital and temporal fossæ distinct; penis free, pendulous; teats pectoral.

**Anthropomorphous.*

Fam. 1. HOMINIDÆ.

Cutting teeth four above and below; grinders 5-5 above and below; nostrils separated by a narrow septum.

†Tail none. 1. *Hominina*, Homo. 2. *Simiina*. Troglodytes, *Geoff*. Simia, *Lin*. Hylobates, *Illiger*. ††Tail long or short. 3. *Presbytina*. Presbytes, *Eschy*. 4. *Cercopithecina*. Lasiopyga, *Illig*. Cercopithecus, *Lin*. Cercocebus, *Geoff*. Macacus. 5. *Cynocephalina*. Cynocephalus, *Brisson*. Papio, *Brisson*.

Fam. 2. SARIGUIDÆ.

Grinders 5-5 in each jaw, acutely tubercular, or 6-6 bluntly tubercular; nostrils separated by a broad space; tail long. *South America*.

†Tail end naked. 1. *Mycetina*. Mycetes, *Illig*. 2. *Atelina*. Ateles, *Geoff*. Brachyteles, *Spix*. Gastromargus, *Spix*. Lagothrix, *Geoff*. ††Tail end hairy. 3. *Callithricina*. Cebus, *Erxl*. 4. *Saguinina*. Saguinus, *Lacep*. Nyctipithecus, *Spix*. Pithecia, *Geoff*. Brachypus, *Spix*. 5. *Harpalina*. Jacchus, *Geoff*. Midas, *Geoff*.

***Quadrupedoid.*

Fam. 3. LEMURIDÆ.

Grinders 6-6 above, 5-5 below; nostrils terminal; extremities free; first finger of the hind feet armed with recurved claws.

†Head long; grinders blunt. 1. *Lemurina*. Lemur, *Lin*. 2. *Lichanotina*. Indris, *Lacep*. Lichanotis, *Illig*. ††Head round. 3. *Loridina*. Loris, *Geoff*. Nyaticebus, *Geoff*. 4. *Galagonina*; Otolienus, *Illig*. Galago, *Adams*. Cheirogallus, *Geoff*. 5. *Tarsina*, Tarsius. 6. *Cheiromina*. Cheiromys, *Cuv*.

Fam. 4. GALEOPITHECIDÆ.

Grinders 6-6 above, 5-5 below, acutely tubercular; extremities and tail enveloped in a hairy skin; finger short.

Galeopithecus, *Pallas*.

Fam. 5. VESPERTILIONIDÆ.

Grinders various, true 3-3 in each jaw; tail, limbs, and fingers, inclosed in thin, naked membranes, fingers very long, stretching the membrane.

†Nose leaved. 1. *Rhinolophina*. Megaderma, *Geoff*. Rhinolophus, *Geoff*. Nycteris, *Geoff*. Mormoops, *Leach*. Nyctophilus, *Leach*. 2. *Phyllostomina*. Phyllostomus, *Geoff*. Vampyrus, *Geoff*. Arctibeus, Medateus, and Monophyllus, *Leach*. Diphylla, *Spix*. Rhinopoma and Glossophaga, *Geoff*. ††Nose leafless. 3. *Pteropina*. Pteropus, *Geoff*. Cynopterus and

Macroglossum, *F. Cuv.* Cephalotis, *Geoff.* Harpyia, *Illig.* 4. *Noctilionina.* Noctilio, *Lin.* Stenoderma and Nyctinomus, *Geoff.* Dysopes and Molossus, *F. Cuv.* 5. *Vespertilionina.* Vespertilio and Plecotus, *Geoff.* Barbastellus, *Gray.* Proboscidea and Thyroptera, *Spix.* Cælano, *Leach.*

Order II.—FERÆ, *Lin.*

Thumb of the fore extremities not opposite; toes clawed; teats ventral; penis sheathed.

**Cutting teeth six above and below; grinders of three sorts.*

Fam. 1. FELIDÆ.

Toes only applied to the ground in walking. Nose scarcely mobile, rounded.

†No tubercular grinders in the lower jaws. 1. *Hyenina.* Hyena, *Brisson.* Proteles, *Geoff.* 2. *Felina.* Felis, *Lin.* Lyncus, *Gray.* Prionodon, *Horsf.* ††Tubercular grinders in both jaws. 3. *Mustelina.* Putorius, Zorilla, and Mephitis, *Cuv.* Mustela, *Lin.* Lutra, *Ray.* 4. *Viverrina.* Viverra, *Lin.* Genetta, *Cuv.* Herpestes, *Illig.* Crossarchus, *F. Cuv.* Suricata, *Desm.* Paradoxurus, *F. Cuv.* Ictides, *Valenc.* 5. *Canina.* Canis, *Lin.* Fennecus, *Desm.* Lycaon, *Brookes.*

Fam. 2. URSIDÆ.

The soles of the feet bald, cartilaginous, applied to the ground in walking; toes 5-5 often armed with long claws; nose mobile, often used in digging.

†Tubercular grinders, 2-2 above, and 2-2 or 1-1 below. 1. *Ursina.* Ursus, *Lin.* Danis, *Gray.* Prochilus, *Illig.* Helarctos, *Hors.* Thalassarctos, *Gray.* 2. *Procyonina.* Procyon and Nasua, *Storr.* ?Potos, *Geoff.* ††Tubercular grinders 1-1 above and below. 3. *Gulonina.* Gulo, *Retz.* Galera, *Brown.* Grisonia, *Gray.* Mellivora, *Storr.* 4. *Myadina.* Myadus, *F. Cuv.* 5. *Taxina.* Meles, *Brisson.*

***Cutting teeth various (rarely six above and below); grinders of two sorts, false and tubercular.*

Fam. 3. TALPIDÆ.

Cutting teeth distinct; grinders acutely tubercular; legs short for walking or digging; no nursing pouch nor marsupial bones. Allied to *Vespertilionidæ.*

*Fore feet fit for digging. 1. *Talpina.* Talpa, *Lin.* 2. *Chrysochlorina.* Condylura, *Illiger.* Chrysochloris and Scalops, *Cuv.* ††Fore feet for walking. 3. *Soricina.* Sorex, *Lin.* Mygale, *Cuv.* 4. *Erinacina.* Erinaceus, *Lin.* 5. *Tenrecina.* Tenrecus, *Lacep.* 5.? *Tupaina.* Tupaia, *Raffles.*

Fam. 4. Didelphidæ.

Cutting teeth distinct; canine sometimes wanting; grinders acutely tubercular; thumb of hind feet mostly distinct, clawless; nursing pouch and marsupial bones distinct.

†Cutting teeth six above, two below. 1. *Macropina.* Macropus, *Shaw.* Halmaturus, *Illiger.* Potorous, *Desm.* 2. *Phalangistina*, Acrobata, *Desm.* Petaurus, *F. Cuv.* Phalangista and Balantia, *Illiger.*? Phascolaretus, *Blainv.* ††Cutting teeth not six above, and two below. 3. *Phascolomina.* Phascolomys, *Illiger.* 4. *Didelphina.* Didelphis, *Lin.* Cheironectes, *Illiger.* 5. *Dasyurina.* Peracyon, *Gray.* Dasyurus, *Illiger.* Phascogale, *Tem.* 6. *Peramelina.* Perameles and Isodon, *Geoff.*

Fam. 5. Phocidæ.

Cutting teeth six or four above, four or two below; canine teeth distinct; grinders tubercular, or truncated; limbs short, fin-shaped, hinder ones horizontal; nostrils operculated.

†Grinders many rooted; ears none; nose simple. 1. *Stenorhyncina.* Pelagios, *F. Cuv.* Stenorhyncus, *F. Cuv.* 2. *Phocina.* Phoca. ††(Grinders roots simple, or divided, and with ears distinct.) 3. *Enhydrina.* Enhydra, *Flem.* 4. *Otariina.* Otaria, *Peron.* Platyrhynchus, *F. Cuv.* 5. *Stemmotopina.* Stemmotopus and Macrorhinus, *F. Cuv.*

§ 2. *Teeth not of three sorts, or not forming a continuous series.*

Order III.—Cetæ, *Lin.*

Teeth none, or all similar, conical; body, fish-shaped, nearly bald; limbs fin-shaped, hinder sometimes forming a horizontal tail.

* *Skin smooth, without any hair or whiskers.*

Fam. 1. Balænidæ.

Head very large, one-third the length of the body. 1. *Balænina.* Balæna, *Willoughby.* Balænoptera, *Lacep.* 2. *Physeterina.* Physalus, *Lacep.* Physeter, *Lin.* Catodon, *Lin.*

Fam. 2. Delphinidæ.

Head small or moderate; body long; blowers united. 1. *Delphinina.* Delphinus, *Lin.* Delphinorhyncus, *Blainv.* 2. *Phocænina.* Phocæna, *Cuv.* Delphinapterus, *Lacep.* Heterodon, *Blainv.* Monodon, *Lin.*

** *Skin rather hairy, whiskers distinct; grinders flat-topped.*

Fam. 3. Trichechidæ.

Body oblong; hind feet, rather prominent, clawed; tail short, separate; canine upper, very long exserted.

Trichecus, *Lin.*

Fam. 4. Manatidæ.

Manatus, *Cuv.*

Fam. 5. HALICORIDÆ.
Halicora, *Illiger?* Stellerus, *Cuv.*

Order IV.—GLIRES, *Lin.*

Teeth, cutting, two in each jaw, large, strong, separated from the grinders by a space; canine teeth none; condyles of the jaws longitudinal; orbital and temporal fossæ united; toes distinct, with small conical claws; thumb sometimes rudimentary. [Exceedingly difficult to arrange: the following is only an attempt according to their habits.]

**Fur with scattered larger hairs or spines; tail spiny or scaly.*
Fam. 1. MURIDÆ.
Cutting teeth two in each jaw, lower, awl-shaped; grinders simple or compound, upper shelving backward, lower forwards; limbs proportionate; tail scaly; fur with scattered longer hairs, or flat spines; clavicles distinct.
†Grinders rooted, simple. 1. *Murina*, Mus, *Lin.* Otomys, *F. Cuv.* Capromys, *Desm.* 2. *Hydromina.* Hydromys, *Geoff.* ††Grinders rootless, compound. 3. *Ondatrina.* Ondatra. 4. *Castorina.* Castor, *Lin.* Osteopera, *Harlan.* 5. *Echymina.* Echymys, *Geoff.* Heteromys, *Desm.* Saccomys, *F. Cuv.*

Fam. 2. HISTRICIDÆ.
Cutting teeth two in each jaw, lower, truncated; grinders 4-4 in each jaw, rooted, compound; tongue and body covered with spines; clavicles none.
†Tail short. 1. Histrix, *Lin.* 2. Acanthia. ††Tail elongated. 3. Erythizon. 4. Spygurus. 5. Simthurus, *F. Cuv.*

***Fur nearly equally soft; tail none, or hairy.*
Fam. 3. LEPORIDÆ.
Cutting teeth two in each jaw, or four in the upper one, lower one subsubulate; grinders numerous, rootless; ears generally large; tongue often hairy; eyes large; clavicles none; fore feet short; hinder ones long; tail none, or very short, hairy; fur soft.
†Cutting teeth four above. 1. *Leporina.* Lepus.? *Lagomina.* Lagomys. ††Cutting teeth two above. 3. *Caviina*, Cavia, *Lin.* Kerodon, *F. Cuv.* 4. *Hydrocharina.* Hydrocharus, *Brisson.* 5. *Dasyporcina.* Cælogenys, *Illig.* Dasyporca, *Illig.* Dolichotis, *Desm.*

Fam. 4. IERBOIDÆ.
Cutting teeth two in each jaw; grinders simple, or compound, rooted; ears moderate; eyes large, prominent; clavicles distinct; fore feet short (used as hands); hind feet very long; tail long, hairy, used in leaping or walking; fur soft.

†Grinders compound or rootless. 1. *Pedestina.* Pedestes, *Illig.* 2. *Dipina.* Dipus, *Schreb.* Meriones, *F. Cuv.* not *Illiger.* ††Grinders simple, roots divided; legs nearly equal. 3. *Gerbillina.* Gerbillus, *Desm.* 4. *Myoxina.* Myoxus, *Gmelin.* 5. *Sciurina.* Sciuropterus, *F. Cuv.* Pteromys, *Cuv.* Macroxus, *F. Cuv.* Sciurus, *Lin.* Tamia, *Illiger.* The latter genus is very closely allied to *Arctomina.*

Fam. 5. Aspalacidæ.

Cutting teeth two in each jaw, lower chisel, or awl-shaped, often very much exposed; grinders compound or simple, rarely rootless; ears and eyes often very small, sometimes hid; clavicles strong; limbs proportionate; tail none, or hairy, cylindrical; fur very soft.

†1. *Aspalacina.* Orycterus, *F. Cuv.* Bathyergus, *Illiger.* Aspalax, *Oliv.* 2. *Lemnina.* Arvicola, *Lacep.* Sigmodon, *Say.* Neotoma, *Say.* Lemnus, *Lin.* ††3. *Cricetina.* Cricetus, *Lacep.* 4. *Pseudotomina.* Pseudotoma, *Say.* Diplostoma and Geomys, *Raff.* 5. *Arctomina.* Arctomys, *Gmel.* Spermophilus, *F. Cuv.*

Order V.—Ungulata, *Ray.* Bruta, Pecora, and Belluæ, *Lin.*

Teeth irregular; cutting and canine teeth often wanting in one or both jaws; grinders all similar, sometimes wanting; toes large, covered with hoofs or large conical claws.

***Two middle toes large, equal; bones of the metacarpus and metatarsus united.**

Fam. 1. Bovidæ.

Two middle toes separate; cutting teeth eight below; upper jaw callous; grinders 6-6 in each jaw; frontal bones with horns; gullet with two large pouches just before the stomach, used for holding and soaking the food before it is chewed; using their head and horns in defence.

†Horns persistent. 1. *Bovina.* Bos, *Lin.* Ovis, *Lin.* Capra, *Lin.* Antilocapra, *Ord.* Antilope, *Brisson.* Catoblepas, *Gray, Med. Rep.* The nostrils of this genus are very peculiar, being very large, and exactly covered with a moveable lid. 2. *Camelopardina.* Camelopardalis, *Lin.*

††Horns none, or deciduous. 3. *Camelina.* Camelus, *Lin.* Auchenia, *Illiger.* 4. *Moschina.* Moschus, *Lin.* Memina, *Gray, M. R.* 5. *Cervina.* Muntjaccus, *Gray.* Coassus and Capreolus, *Gesner.* Axis, *Blainv.* Cervus, *Lin.* Dama, *Gesner.* Tarandus, *Pliny.* Alus, *Pliny.*

Fam. 2. Equidæ.

Two middle toes soldered in one; cutting teeth six in each jaw; canine teeth one in each jaw; gullet and stomach simple: using the hind feet in defence.

Equus, *Lin.* Asinus, *Gray.*

***Toes 3, 4, or 5, to each foot, nearly equal; teeth nearly in one series.*

Fam. 3. ELEPHANTIDÆ.

Grinders rooted, transversely ridged; toes 3-3, 3-4, or 5-5; last joint covered with a hoof; skin thick, nearly naked; hairs large, ridged; gullet simple.

†Nose extended into a trunk. 1. *Elephantina.* Elephas, *Lin.* Mastodon, *Cuv.* 2. *Tapirina.* Tapirus, *Briss.* Lophiodon and Paleotherium, *Cuv.* ††Nose not produced into a trunk. 3. *Rhinocerina.* Rhinoceros, *Lin.* Hyrax, *Herman.* (allied to *Caviina.*) Lipura and Elasmotherium, *Fischer.*? Anoplotherium, Xyphodon, Dolichotuna, Adapis, Anthacotherium and Chæropotamus, *Cuv.* (all very much allied to *Suina*). 4. *Suina.* Sus. *Lin.* Babiroussa, Phascochærus, *F. Cuv.* Dicotyles, *Cuv.* 5. *Hippopotamina.* Hippopotamus, *Lin.* (allied to *Halicoridæ?*)

Fam. 4. DASYPIDÆ.

Grinders rootless, crown flat, sometimes entirely wanting; face long, acute; mouth mostly very small; body armed with scales or ridged hairs.

†Body covered with scales and armour, revolute. 1. *Manina.* Manis, *Lin.* 2. *Dasypina.* Tylopeutes, *Illiger.* Priodon, *F. Cuv.* not *Horsf.* Dasypus, *Lin.* Chlamyphorus, *Harlan.* ††Body hairy or spinous, not convolute. 3. *Orycteropina.* Orycteropus, *Geoff.* 4. *Myrmecophagina.* Myrmecophagus, *Lin.* Tamandua, *Gray, M. R.* Cyclothurus, *Gray.* 5. *Ornithoryncina.* Echidna, *Cuv.* Ornithorhyncus, *Blum.*

Fam. 5. BRADYPIDÆ.

Grinders rootless, cylindrical; crown, when young, conical; tail round; neck short; limbs very long; teats pectoral; hair, dry, crisp; stomach two or three celled (allied to *Loridæ* in habits). Bradypus, *Lin.* Cholæpus, *Illiger.* Megatherium, *Cuv.* Megalonix, *Jefferson.*

I have placed *Glires* between *Cetæ* and *Ungulata* that the orders of mammalia and birds should be parallel in analogy; and also because both orders have apparently a nearly equal affinity to the Primates by the genera *Bradypus* in one, and *Cheiromys* in the other; but the affinity of *Hippopotamus* to some of the *Cetæ* is much more apparent than any affinity that I am able to discover between any of the *Glires* and the latter. The *Glires* and the *Ungulata* are allied by means of the genera *Hydrocharus* and *Hyrax.*

The following series will exhibit the manner in which the orders appear to be connected together:—

Order I.—PRIMATES.

Typical Groups.	Annectant Groups.
Fam. 1. Hominidæ.	3. Lemuridæ.
2. Sariguidæ.	4. Galeopithecidæ.
	5. Vespertilionidæ.

Order II.—FERÆ.

Fam. 1. Felidæ.	3. Talpidæ.
2. Ursidæ.	4. Didelphidæ.
	5. Phocidæ.

Order III.—CETÆ.

Typical Groups.	Annectant Groups.
Fam. 1. Balænidæ.	5. Trichechidæ.
2. Delphinidæ.	4.? Manatidæ.
	5.? Halicoridæ.

Order IV.—GLIRES.

Histricidæ.	Leporidæ.
Muridæ.	Ierboidæ.
	Aspalacidæ.

I am uncertain which are the typical families of this order.

Order V.—UNGULATA.

Fam. 1. Bovidæ.	3. Elephantidæ.
2. Equidæ.	4. Dasypidæ.
	5. Bradypidæ.

ARTICLE VII.

On the Influence of Solar Light on the Process of Combustion. By Thomas M'Keever, MD.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

THERE is an opinion prevalent in this, and I have reason to believe in other countries, that the sun's rays, or even the ordinary light of day, when admitted freely into an apartment in which a common fire is burning, have the power either of dulling it considerably, or should the combustion be going on languidly, of altogether effecting its extinction. Hence it is a common practice to place screens of different kinds before the fire-place, or to close the shutters of the apartment in order to prevent as much as possible the access of light to the burning materials. I was for a long time impressed with the belief that this was merely a piece of popular prejudice, for which there existed no rational foundation whatever, or that at furthest, the appearances might be owing to the retina having become less sensible to the comparatively feeble rays emitted by a body in a low state of combustion while already under the influence of a stronger light.* But as opinions so generally entertained usually rest more or less on observation and experience, the best sources of evidence in all such cases; and as I was unable to procure any information whatever on the subject from the several works on chemistry which I have had an opportunity of consulting, I was induced, during the late summer, when we had such an unusual succession of steady sunshine, to make the following experiments.

Exper. 1.—Two portions of green wax taper, each weighing ten grains, were both ignited at the same moment; one of them I placed in a darkened room, the other I exposed to broad sunshine in the open air: thermometer in sun 78° Fahr.; in room 67°; loss as follows: †

In five minutes that placed in sunshine lost. ...... $8\frac{1}{2}$ grs.
darkened room lost.. $9\frac{1}{4}$

Exper. 2.—Two portions of taper, each weighing 23 grains, were placed under similar circumstances, as in the former experiment.

In seven minutes that placed in sunshine lost ...... 10 grs.
darkened room lost 11

We here see, notwithstanding the higher temperature to which the taper in sunshine was exposed, which must of course have favoured the liquefaction of the wax, and consequently its ascent in the wick, that during the short period of seven minutes, there was a difference of loss amounting to not less than one grain.

Exper. 3.—A common mould candle, fourteen inches in length and three in circumference, was accurately divided into inches, half-inches, and eighths, and exposed in the first instance to strong sunshine: thermometer 80° Fahr.; atmosphere remarkably calm.

* Hence it is that the strongest light appears to produce the deepest shadow. A total eclipse of the sun occasions a more sensible darkness than midnight, being more immediately contrasted with the strong light of noon-day.

† I should mention that in all these experiments the snuff was carefully removed with a sharp scissars, whenever a quarter of an inch of taper was consumed. This was obviously necessary as the length of the snuff is known to influence materially the rate of combustion.

To consume one inch it took	59′	0″
In darkened room (temp. 68° F.)	56	0
In ordinary light of day (temp. 68° F.) . .	57	10

Exper. 4.—A piece of taper, seven inches in length, and six-eighths of an inch in circumference, was carefully divided into inches, and, as in former experiment, submitted to bright sunshine: thermometer 79°.

To consume one inch it took	5′	0″
Transferred to darkened room (temp. 67°)	4½	0
In ordinary light of day (temp. 67°)	4	52

Exper. 5.—In order to vary the experiment, and to guard as much as possible against the agitation of the surrounding atmosphere, I procured two lanterns; one of them I coated with black paint; the other I left naked. In these I placed two portions of taper, of precisely equal weights, and exposed them both to a strong glare of sunshine.

In 10 minutes that placed in painted lantern lost . . 16½ grs.
that placed in uncoated lantern lost. 15*

Exper. 6.—With the view of ascertaining whether similar results were to be obtained by exposure to the light of the moon, I prepared the lanterns as in the last experiment, and took an opportunity lately when this luminary shone forth with peculiar splendour, of trying its effects; but although I employed an exceedingly delicate balance for the purpose, I could detect no difference whatever in the loss sustained by the two portions of taper.

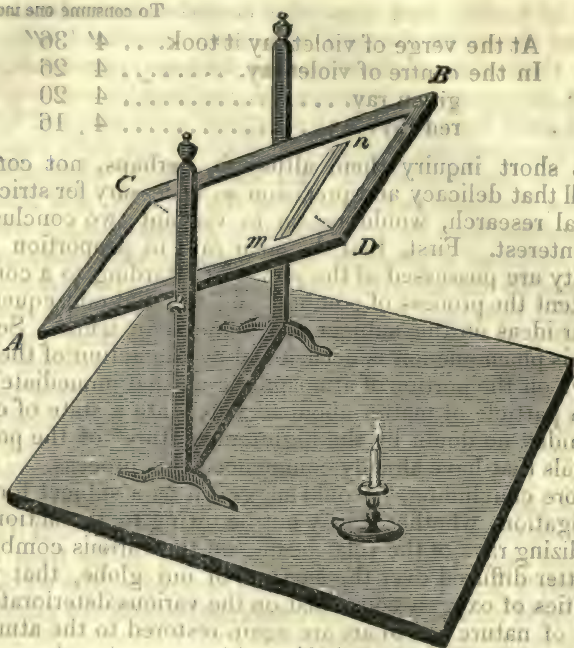
After I had made these experiments, I naturally turned my attention to an explanation of the principles on which results of so singular a nature could depend; and it occurred to me that they probably were owing to the well-known decomposing power possessed by the solar rays, in consequence of which the shell of air that immediately encircles a particle of matter about to enter into combustion, is deprived, to a certain extent, of its oxygenous principle, and is thus rendered less fitted for the maintenance of this important process.† Thus in order to narrow and simplify the matter, let us suppose that one atom of carbon is about to enter into combination with two atoms of oxygen, we can readily conceive that the chemical rays may possess the power of withdrawing one of those atoms from the

* The diminished rate of consumption in this experiment was probably owing to the want of a free current of air through the interior of the lantern.

† That an affinity or attraction is exerted between light and the particles of bodies may be justly inferred from the great refractive power of inflammable bodies, which, all other things being equal, must be supposed to attract light more powerfully than other substances.—(See Ellis on Atmospheric Air, p. 167.)

sphere of action, and thus offer considerable resistance to the chemical union of the two elements. Further it may be supposed, that when combustion has become very brisk, as it is termed, the attraction between the combustible material and the oxygen shall have become so energetic as to suspend or altogether to overcome the deoxidizing power of the sun's rays. Nay, that those very rays which, at a less elevated temperature, had but a few moments before retarded the process, may now contribute materially to its acceleration. In this respect we merely assign to them a property equivalent to that unquestionably possessed by the calorific rays under particular circumstances. In several instances these last mentioned rays have the power, when of a certain degree of intensity, of causing the union of several bases with oxygen, while at a more elevated range of temperature, they will have the effect of occasioning their total disseveration. Perhaps the red oxide of mercury affords us one of the most remarkable and satisfactory instances of this circumstance. At the temperature of 600°, mercury will combine with about 8 per cent. of oxygen, forming an acrid caustic substance consisting of brilliant, sparkling, deep red scales; but if the heat be raised even a few degrees beyond this, so far from combining with a still further proportion of oxygen, the whole of this principle is released from its combination, and the metal returns to its original state of fluidity.

As the solar rays are now ascertained to consist of three distinct species of radiant matter; namely, those that impart heat, those that impart light, and the chemically acting rays, it appeared to me that the best mode of putting the conjecture I have ventured to advance to the test of experiment would be to try whether any difference could be detected in the loss sustained by a lighted taper when exposed to the several portions of the solar spectrum. Accordingly I constructed an apparatus similar to that described by Dr. Herschell in his interesting researches on "*the Power of the Prismatic Colours to heat and illuminate Objects,*" consisting of a frame AB moveable on two centres, into which I inserted a piece of pasteboard C D, having an opening in it *m n* of sufficient size to allow the whole extent of *one* of the prismatic colours to pass through.



I then placed a prism moveable on its axis before an opening in the window shutter, so that the sun's rays should fall on it at right angles, and having got the spectrum stationary on the pasteboard, I allowed the rays of one colour only to pass at a time. A piece of green taper, accurately marked after the usual manner, was now ignited, and submitted to different portions of the spectrum with the following results:

	To consume two inches of taper.	
In the red ray it took	8′	0″
green ray.	8	20
violet ray.	8	39
verge of violet ray	8	57

Commencing with the violet ray, the loss was as follows:*

* The prismatic spectrum having been received on a page of print, the lines placed in the respective colours were legible at the following distances:

In the violet ray at	14 inches.
indigo	16
blue	$16\frac{1}{2}$
green	18
deep yellow....................	$18\frac{1}{12}$
orange	$17\frac{1}{3}$
red	$16\frac{1}{4}$

The circumstance of the luminous rays existing in greatest abundance in the green

	To consume one inch.	
At the verge of violet ray it took. . .	4′	36″
In the centre of violet ray.	4	26
green ray.	4	20
red ray.	4	16

This short inquiry then, although, perhaps, not conducted with all that delicacy and precision so necessary for strict philosophical research, would appear to warrant two conclusions of some interest. First, that the solar rays in proportion to their intensity are possessed of the power of retarding to a considerable extent the process of combustion; and that consequently the popular ideas on this subject are founded in truth. Secondly, that this phænomenon is occasioned by the action of the chemical rays on the portion of atmospheric air that immediately envelopes a particle of matter about to enter into a state of combustion, aided no doubt by the high temperature* of the portion of materials that have already commenced this process.

Before concluding, I would suggest it as a subject for further investigation, whether it may not be owing to the action of the deoxidizing rays of the solar beam, on the various combinations of matter diffused over the surface of our globe, that the vast quantities of oxygen consumed on the various deteriorating processes of nature and of art are again restored to the atmosphere so as to preserve at all periods, and in every situation, a state of constant uniformity of composition.

Plants, it is satisfactorily ascertained, give out a large quantity of oxygen gas when exposed to sunshine, an effect that may very fairly be attributed to the action of the chemical rays on the carbonic acid generated in the minute vascular system of the leaf.† Even the ordinary light of day has, in some instances, been found capable of accomplishing the same purpose. Thus marsh plants, as the *polygonum persicaria* and the *lythrum salicaria*, yielded oxygen gas by a weak diffused light when confined in an atmosphere of nitrogen; and different species of *epilobium* vegetated a long time, and grew as well in pure nitrogen gas as

portion of the spectrum would appear to suggest the propriety of imparting this colour to objects that we wish to render visible at great distances, such, for instance, as lights situated along the sea coast. I am not aware of the experiment having been tried in light-houses, and I should anticipate but one objection to its success; namely, that the blue colour of the atmosphere combining with the green would impart a yellowish tinge. In the colouring of our apartments, however, and a variety of other occasions, this principle might be advantageously attended to. The circumstance of our fields, as well as the greater portion of the vegetable world, having been clothed with this delicate and refreshing colour, may enable those animals that feed by night more readily to discover their wonted repast. This, at all events, may be one end accomplished by it. The same simple agent in the hands of an all-wise Providence is frequently employed to accomplish the greatest variety of purposes.

* The temperature of a common fire, according to Irvine, is 790°.

† Saussure found that by the decomposition of carbonic acid not only was oxygen discharged from the plant, but that the proportion of its carbon was materially increased.

in common air, though exposed only to a weak light, or protected from the action of the sun. The nitrogenous atmosphere at the end of two months was increased in bulk, and contained $\frac{8}{100}$ths of oxygen gas; whereas when similar plants were confined in pure nitrogen gas, and kept in perfect darkness, though they were renewed every twelve hours, lest their vegetation might languish; yet they produced no oxygen gas, but augmented their atmosphere by a quantity of carbonic acid.*

The production of the *green* colour in plants (a process I may observe intimately connected with the discharge of oxygen)† has been clearly demonstrated to be influenced by the portion of the spectrum to which the plant may have been subjected; and these changes, it has been further proved, were occasioned not by the heating or illuminating rays, but by the peculiar properties of the chemical rays associated with them. Senebier having sowed different quantities of lettuce seeds in several small cups, subjected them to the influence of light transmitted through fluids tinged of various colours. The leaves exposed to yellow light were at first of a faint green, but afterwards became yellow: those exposed to violet light were of a bright green, and their colour augmented with their age; while those raised in obscurity possessed no verdure whatever.‡

As animal substances (according to the experiments of Abernethy, Cruickshank, and Jurin) deteriorate the air after the manner of vegetables; namely, by the absorption of oxygen and the emission of an equivalent quantity of carbonic acid gas, the supposition seems not improbable, from their similarity in structure and other circumstances, that they also when exposed to sunshine would give out oxygen gas. But for this, we have as yet no data. I believe the experiment has not been tried, and I merely mention it as affording a field for curious and interesting inquiry both to the chemist and physiologist. With regard to inorganic matter, we find that the most compact and solid materials with which we are acquainted are incessantly subjected to those slow and silent changes to which we commonly apply the term decay. By this in truth we understand, that the elements of which they are composed are constantly entering into new and varied states of combination. Influenced by the physical agents that surround them, the results of this mutual interchange of

* We can readily conceive that during the ordinary light of day, the chemical rays, from their dilute and feeble state, may not possess sufficient power to interfere (unless in a few instances such as those to which I have alluded) with the usual functions of plants; but that during sunshine, their intensity may become so much increased as to occasion a directly opposite round of changes, the air that had hitherto been contaminated by vegetation being now restored to its original state of purity.

† "The emission of oxygen and the production of the green colour in plants appear both to depend on the same cause—the decomposition of carbonic acid; so that we cannot so properly affirm that the green parts afford oxygen as that they become green when that gas is expelled."—(See Ellis's Further Enquiries.)

‡ See Mr. Ellis's Further Enquiries, &c. p. 78; Saussure's Recherches, p. 54.

affinities are again destined to pass away, and having thrown off the forms they had assumed, they serve as the basis of other, perhaps far different compounds. It has been usual to attribute the changes here spoken of almost exclusively to the combined influence of air and of moisture aided by inequalities in atmospheric temperature.* These no doubt are powerful, and to a certain extent, effective agents; yet when we come to consider apparently the most simple phænomena, how many serious and unbending difficulties have we not to encounter? The co-operation of an active energetic principle such as that I have suggested, would, it appears to me, afford a ready explanation of many miscellaneous occurrences at present involved in much obscurity. Above all, its well-known power of causing the disengagement of oxygen from a variety of compound bodies would appear to offer a fair and satisfactory explanation of one of the most abstruse and difficult problems that has as yet engaged the attention of scientific men. "By whatever process," observes Mr. Ellis, "the purification of the atmosphere may be accomplished, of this general fact we may rest satisfied, that as oxygen is withdrawn from it in order to enter into new combinations, so it can again be restored only by such decompositions as shall set it free, and these decompositions must be as numerous, and to an extent as great, as the combinations to which they succeed."

I am fully aware that this is a subject on which even a conjecture should be hazarded with extreme caution, but I trust its importance, as well as the acknowledged want of any thing like a satisfactory explanation of the principles on which such interesting and important changes depend, will furnish an apology for the few imperfect ideas I have here ventured to throw out.

Whether chemically acting rays exist in the moonbeams is a point I believe not yet fully determined. It is likely, however, that they do, although in an extremely dilute and attenuated condition. Indeed if the colorization of the leaf is once admitted to be owing to the action of these rays (and of this the experiments of Senebier appear to leave no doubt), we can hardly hesitate to admit their existence.† The Abbe Lessier and

* Matter is commonly divided by chemists and natural philosophers into *living* and *dead*, but the distinction is probably not founded on rational or accurate views: what we usually term dead matter is by no means inert or indifferent with regard to itself; there is not a particle of dust on which we tread but has its peculiar relations and affinities, by which it is enabled to form a portion of that perpetual round of modifications that form so remarkable a feature in the material world. Attraction in fact may be said to constitute the *life* of the inorganic masses of our globe.

† Although no difference could be detected in the process of combustion when carried on by moonlight and in a darkened room, yet this might be owing to the imperfection of the means employed to ascertain the weights of the respective portions of taper. The light of this luminary does not amount to 100,000th part of that of the sun, and the very feeble heat which this excites has never yet been detected by the most delicate contrivance of art. Such may be the case with the chemical rays: our best devised experiments may not be able to detect their presence, although from a variety of natural phænomena their existence is hardly to be questioned.

others have both fully established the fact that the light of the moon has the power of imparting the green colour to plants, although of a tinge less deep than that occasioned by the light of day. Fruits also that have been kept excluded from the sun are found to ripen with considerable rapidity when exposed to the lunar rays.

Finally, it may be stated, the several species of rays that compose the solar beam would appear to exert the following specific but varied actions.

First, the calorific rays: they impart heat to the different combinations of matter, organic as well as inorganic, and thus assist in the various chemical changes to which they are incessantly subjected.

Secondly, the calorific rays by which the objects that surround us are illuminated, and but for which it is obvious all nature would be as a dreary blank.

Lastly, the deoxydizing or chemical rays, by means of which (it is suggested) the oxygen consumed during combustion, respiration, and a variety of other processes, is again restored to the atmosphere, thus preserving this medium in a state constantly fitted for administering to the support of organized beings.

Article VIII.

Answer to Mr. Rainy's Paper on the Specific Gravity of Hydrogen Gas. By Thomas Thomson, MD. FRS.

In the *Annals of Philosophy* for August last (vol. x. p. 135, New Series), my friend Mr. Rainy has endeavoured to show that I have underrated the quantity of vapour in hydrogen gas, by making its specific gravity too low; and that when the errors in my calculations are corrected, the specific gravity of hydrogen gas is to that of oxygen gas not as I have supposed, as 1 to 16, but as 1 to 16·54, or as 0·967 to 16. He draws as a conclusion that my experiments disprove the hypothesis that the specific gravities of all the gases are multiples by integer numbers of the specific gravity of hydrogen gas.

I was anxious, before answering this paper of Mr. Rainy, to make a few additional experiments on the subject; and as the manipulations were rather delicate, I thought it requisite in the first place to get my balance put into the best possible order, and my weights adjusted so as to render the unavoidable errors in weighing as trivial as possible. My friend Mr. Crichton, to whose uncommon zeal, abilities, and accuracy, I have been already so frequently indebted, was kind enough to put my balance into excellent order; but it was almost the middle of September before I was able to execute the projected experiments.

Mr. Rainy's paper is written with all that perspicuity, accuracy, and modesty, which I looked for from him; and had he taken into his consideration all the circumstances of the case, his conclusions would have been undoubted. But I flatter myself that I shall be able to satisfy him that if I have committed any error in my calculations, it lies exactly on the opposite side from what he supposes; and that instead of underrating, I have in reality overrated the weight of the moisture which the hydrogen gas ought to have contained, on the supposition that the theory of vapour at present adopted is correct.

In my recent work, to which Mr. Rainy alludes, I did not consider it proper to enter into any details. I have not even noticed all the data which were employed by me in calculating the quantity of vapour in the hydrogen gas. It will be requisite, therefore, to state some particulars of the experiments a little in detail that the grounds of the calculations may be fully understood.

The weight of the small flask in which I dissolved the zinc was 786·9 grains. Its capacity was 8·8 cubic inches.*

The dilute sulphuric acid employed for dissolving the zinc was a mixture of very nearly

Water	1700 grs.
Sulphuric acid..................	400
	2100

so that the usual weight of the glass filled with dilute sulphuric acid was 2886·9 grains. This weight was not rigidly the same in different experiments; because the acid and water were measured and not weighed. But the difference scarcely exceeded eight grains, except in a few cases when the quantities were purposely varied.

The boiling point of my dilute acid was always 224°, except in two or three trials in which the acid was made stronger on purpose; but as none of these trials were employed in my determination of the specific gravity of hydrogen gas, I need not bring them under review.

The length of the glass tube filled with chloride of calcium was 15 inches, and its weight when so filled varied from 815 grains to 820 grains in different experiments.

About two cubic inches of the flask were left empty. The zinc was introduced while the flask was kept in a sloping position, and this position was maintained during the whole time that the zinc was dissolving. This was to prevent any small drops that might be elevated by the escape of the hydrogen gas from making their way out of the flask. It was kept under

* There is a typographical error in vol. i. p. 67, line 9 from bottom, of my late work. Instead of "about 18 cubic inches," it should have been "about 8 cubic inches."

water of the temperature 49° during the whole time that the zinc was dissolving, in order to prevent any elevation of temperature.

When the zinc was completely dissolved, the flask was taken out of the water trough, and wiped dry on the outside. It was then laid upon a table with its mouth open, and gently moved for about ten minutes to allow the two cubic inches of hydrogen gas which it contained to make their escape, and common air to take its place. It was then weighed, taking care to allow it to remain on the scales till the weight became stationary, if it was not so at first, which, however, was generally the case.

When the tube containing the chloride of calcium was detached from the flask, I put that extremity of it which had been furthest from the flask into my mouth, and, drawing a long breath, displaced the whole of the hydrogen gas which it contained, substituting in its place the common air of the room. The tube was then wiped dry and weighed.

After these details, which will enable the reader to appreciate the degree of confidence which may be put in the experiments, I shall follow Mr. Rainy through his calculations.

Mr. Rainy says that I have employed an erroneous formula in calculating the specific gravity of vapour. In fact, however, the formula which I used is precisely the same as his; excepting that he has introduced an additional term $\frac{V}{V'}$. I did not consider it as worth while to introduce this additional term; because some uncertainty still hangs over the value of *p*, and must continue to do so till the law of the expansion of vapour be accurately determined. But I have no objection, since Mr. Rainy chooses it, to introduce the term $\frac{V}{V'}$. It will in fact produce only a very trifling alteration in the result.

I must begin by reminding Mr. Rainy that the boiling point of the liquid from which the hydrogen gas was evolved was not 212°, but 224°, or 12° above the boiling point of water. It was a liquid that required to be raised 12° higher than water before it gave out the same quantity of vapour that water placed in the same circumstances would do. It was necessary on this account to reduce the temperature of 49° (at which the experiment was made) by 12°; for the vapour given out was what would have been given out by water of the temperature 37°.

The volume of gas extricated at 49° was 137·08 cubic inches. To find its volume at 60°, we have this analogy, 497 : 508 :: 137·08 : 140·396 cubic inches = volume of gas at 60°.

The pressure of vapour at 37° (according to Dalton's table) is 0·237 inch of mercury = *p*. Let us calculate the specific gravity of this vapour by Mr Rainy's formula, which is $\frac{V}{V'} \times \frac{p}{30} \times 0{\cdot}625$ = specific gravity.

$$\text{Here } V = 140{\cdot}396$$
$$V' = 137{\cdot}08$$
$$p = 0{\cdot}237$$

Log. 140·396	2·1473561
Log. 137·08	2·1369741
	0·0103820
Log. 0·237	− 1·3747487
Log. 30·	1·4771212
	− 8·8976275
Log. 0·625	− 1·7958800
Log. $\frac{V}{V'}$	0·0103820
Log. $\frac{p}{30}$	− 3·8976275
Log. 0·625	− 1·7958800
	− 3·7038891

The number to which this logarithm corresponds is 0·00505695 = specific gravity of vapour at 37° (or extricated from weak sulphuric acid of 49°). Thus the specific gravity calculated by Mr. Rainy's own formula, instead of being higher than I made it, turns out in fact rather lower.

The difference between this and my former calculation is owing to some little alterations in the data. But these alterations, for reasons to be immediately stated, I consider it as needless to discuss here.

Now the absolute weight of this vapour calculated by the obvious formula 0·00505695 × 137·08 × 0·305 is 0·21143 grs.

Log. 0·00505695	− 3·7038891
Log. 137·08	+ 2·1369741
Log. 0·305	− 1·4843000
	− 1·3251632

The number corresponding to this logarithm is 0·21143. This is about $\frac{1}{100}$th of a grain less than I estimated it.

These details have been both tedious and minute; but they were necessary to satisfy Mr. Rainy that my conclusions are not liable to the objections which he supposes.

If we subtract the 0·163 grain of moisture retained by the chloride of calcium from 0·21143 gr. The remainder 0·04843 will be the moisture still retained by the gas. This is 0·011 grain less than my former estimate.

If we take 0·048 from 3, the remainder 2·952 will be the weight of 138·7551 cubic inches of hydrogen gas.

138·7551 : 100 :: 2·951 : 2·1274 = weight of 100 cubic inches of hydrogen gas; and admitting that 100 cubic inches of oxygen gas weigh 33·915 grains, the specific gravity of oxygen gas is to that of hydrogen gas as 16 to 1·0036. This deviates less than $\frac{1}{300}$th from the ratio of 1 to 16.

But a careful examination of my former experiments, which I was induced to make by the perusal of Mr. Rainy's paper, led me to entertain some doubts about the accuracy of this mode of proceeding. Of ten experiments which I formerly made, nine gave a greater augmentation of weight in the chloride of calcium than the weight of all the vapour that could have been contained in the hydrogen gas at the temperature in which the experiments were made. Now it appeared somewhat unreasonable to lay aside nine-tenths of all the experiments, and to draw my conclusions from the odd tenth. I was desirous, therefore, to try whether by increasing the length of tube filled with chloride of calcium, I could not render the gas perfectly dry, and thus get rid of the necessity of introducing the specific gravity of vapour into the calculation.

The experiments which I am going to give an account of were four in number. They were made when the thermometer stood at 60°, and the barometer at very nearly 30 inches, so that no correction whatever was required for pressure or temperature. I filled three glass tubes with chloride of calcium, the two extremities of each of which were stuffed in the usual manner with amianthus. These tubes were united by slips of caoutchouc, which were cemented into tubes by means of a solution of caoutchouc in naphtha. The length of these tubes was as follows:

First tube	15 inches.
Second tube	22
Third tube	27
Total length	64

The zinc dissolved was 130·21 grains.

Weight of flask with dilute acid	2780·7 grains
Weight of first tube	815·95
Weight of second tube	1033·05
Weight of third tube	1051·79

The experiments were conducted precisely in the way already described, and exactly the same precautions were employed in weighing the tubes and flask. The weather happening to be rainy, I was afraid that some moisture might have insinuated itself into the open end of the tube containing chloride of calcium. To prevent this, that tube was made to pass through a perforated cork fixed in the beak of a tubulated retort containing some sulphuric acid, while a sewing thread doubled was inserted between the tubulure and the glass stopper to allow the hydrogen gas to escape as it was evolved.

The third tube containing chloride of calcium, which was furthest from the flask, had undergone no alteration whatever in weight. I concluded from this that three feet one inch of tube filled with chloride of calcium was sufficient to render the hydrogen gas as dry as it could be made by this method. In consequence of this in a subsequent experiment, I omitted the third tube altogether.

The increase of weight of the second tube was	0·1 gr.
first tube	0·84
Total	0·94

The loss of flask was	4·8 grains.
Gain of tubes	0·94
Weight of hydrogen gas	3·86

Now 130·21 : 100 :: 3·86 : 2·964 = weight of gas evolved during the solution of 100 grains of zinc; and 130·21 : 100 :: 0·94 : 0·7219 = moisture deposited in the tube during the solution of 100 grains of zinc in dilute sulphuric acid. The volume of this gas being 138·7551 cubic inches, it is easy to see that when the barometer stands at 30 inches, and the thermometer at 60°, the weight of 100 cubic inches of hydrogen gas is 2·136 grains.

According to this determination the specific gravity of oxygen gas is to that of hydrogen gas as 16 to 1·0077. This deviates $\frac{1}{138}$th part from the ratio 1 to 16; and this ratio will be exact, if it be admitted that an error amounting to 0·02 grain was committed in the weighing.*

If we calculate the weight of vapour which the hydrogen gas ought to have carried off from the dilute acid, we shall find it to amount to 0·313128 grain; but the moisture imbibed by the chloride of calcium amounted to 0·94 grain, or three times the calculated quantity. We must, therefore, either admit that our notions respecting vapour are still imperfect; or that the hydro-

* It is reasonable to expect that the specific gravity of hydrogen gas as determined experimentally will be a little higher than the theoretical weight, because it is almost impossible to prepare it absolutely pure, and every impurity must necessarily increase its weight. The zinc which I employed, though it had been distilled in an earthenware retort, was by no means absolutely pure; for I could still detect in it minute quantities of foreign matter. Now it is surely not at all unlikely that so great a volume of hydrogen as 137 cubic inches might contain 0·02 grain of foreign matter. This is all the impurity which it is requisite to admit on the supposition that no error whatever was committed in the weighing.

gen gas carries along with it and deposits in the chloride of calcium small quantities of sulphuric acid, or of sulphate of zinc, or of both together.*

Whether Mr. Rainy will consider an experiment which comes within less than one per cent. of the ratio of 1 to 16, as establishing the truth of that ratio, I cannot pretend to conjecture. But I am sure that I am not able to come nearer the truth by means of the balance which I employed. I am not at all certain that the real weight of the hydrogen was not 0·02 grain less than I reckon it; for notwithstanding the goodness of the beam, and the scrupulous attention with which every thing was weighed, it is scarcely possible to guarantee a deviation from accuracy to so small an amount as 0·02 grain when the whole weight amounts to 4759·91 grains, or not much short of a troy pound. This was weighing to $\frac{1}{237995}$th part of the whole, a degree of precision to which I believe it to be very difficult to attain.

But it was not by means of these experiments that I satisfied myself of the truth of the ratio between the specific gravities of hydrogen and oxygen gases. The evidence already brought forward was conclusive. My object was merely to produce an approximation by means so simple as would be likely to satisfy those who had not the requisite knowledge to draw their conclusions from more complicated sources. It may be worth while to mention a few of the facts upon which my opinion was originally founded.

1. I determined by actual experiment that the specific gravity of hydrogen gas is 0·0694. The subsequent determination of Berzelius and Dulong will be found to approach so near to this, that I have often been surprised that these ingenious gentlemen did not perceive that 0·0694 approaches more nearly to the mean of their experiments than the number which they themselves pitched upon.

2. I consider the evidence which I have adduced in my late work as conclusive that air is a mixture or compound of one volume of oxygen gas and four volumes of azotic gas, or of one atom oxygen and two atoms azote. From this it follows that the specific gravity of oxygen gas must be $1·11\dot{1}$, if atmospheric air be reckoned unity.

3. The specific gravity of ammoniacal gas, deduced from a mean of the determinations of Sir H. Davy and my own, is 0·590237. It has been proved I consider to the satisfaction of every person that it is a compound of one volume of azotic

* The chloride of calcium would not merely imbibe the moisture contained in the hydrogen gas passing through it; but it would be constantly absorbing the atmosphere of vapour in the empty part of the small flask. Now as the experiment always lasted 24 hours, it is not unreasonable to suppose that more vapour might be absorbed than what was capable of existing at the given temperature in the hydrogen gas which passed through the chloride.

gas and three volumes of hydrogen gas condensed into two volumes.

2 volumes ammoniacal gas weigh..	1·180474
Subtract 1 volume azotic gas	0·972222
Remain for 3 volumes hydrogen gas	0·208252

The third of which, or 0·069417, must represent the specific gravity of hydrogen gas.

Now $0{\cdot}069417 : 1{\cdot}111\dot{1} :: 1 : 16{\cdot}006$.

Thus we see that the specific gravity of hydrogen gas deduced from that of ammoniacal gas is within $\frac{1}{300}$ th part of $\frac{1}{16}$th of that of oxygen gas.

4. Water has been shown to be a compound of one volume of hydrogen gas and half a volume of oxygen gas united together, and condensed into a liquid.

Weight of a volume of hydrogen gas	0·069417
Weight of half a volume of oxygen gas	$0{\cdot}55555\dot{5}$

Now $0{\cdot}069417 : 0{\cdot}55555\dot{5} :: 1 : 8{\cdot}003$

Here we have the same ratio as before.

The compositions of water and ammonia have been determined with fully as much care as any thing within the whole range of chemical science; and they concur in establishing the ratio between the specific gravities of hydrogen and oxygen gases to be 1 : 16. Indeed I am aware of very few numerical ratios in any department of science that have been determined with so much accuracy.

5. Even the specific gravity of vapour, upon which Mr. Rainy lays so much stress, and which he considers as so completely established, leads to the same conclusion, or rather indeed is founded on the assumption of the truth of this ratio. This specific gravity has been settled at 0·625.

Now vapour is a compound of one volume of hydrogen gas and half a volume of oxygen gas united together, and condensed into one volume. If we subtract $0{\cdot}55\dot{5}$ from 0·625, the remainder $0{\cdot}069\dot{4}$ must represent the specific gravity of hydrogen gas; but $0{\cdot}069\dot{4} : 1{\cdot}111\dot{1} :: 1 : 16$.

In reality, therefore, all the calculations and objections of Mr. Rainy were founded on the admission of the very ratio which he endeavoured in his paper to overturn.

I might easily bring forward a great number of other proofs that the specific gravity of hydrogen gas is exactly $\frac{1}{16}$th of that of oxygen gas. But I have already extended this paper much further than I originally intended; and I believe that in Great Britain at least, the specific gravities of hydrogen and oxygen gases, as I have here stated them, are universally admitted to be true. I have myself considered the subject frequently, and with all the attention of which I am capable, and I am satisfied that a better established fact is not to be found within the limits of chemical science.

Glasgow, Oct. 1, 1825.

Article IX.

On the Discovery of the Anoplotherium Commune in the Isle of Wight. By the Rev. W. Buckland, Professor of Geology in the University of Oxford.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Oxford, Oct.* 4, 1824.

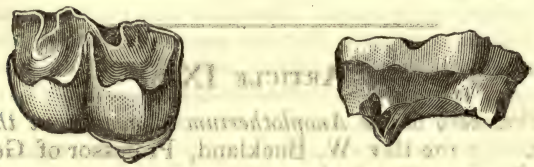
Since the publication of Mr. Webster's excellent Memoirs on the Geology of the Isle of Wight, and the coasts adjacent to it, no doubt has existed as to the identity of the freshwater formations that occur so extensively in that island with those described by Cuvier and Brongniart in the vicinity of Paris; and this conclusion has rested on the similarity of the remains of freshwater molluscæ and vegetables which these formations respectively contain, and on a correspondence in their substance, and their relative position to other strata of marine origin, quite sufficient to establish the contemporaneous deposition of these remarkable strata at the bottom of ancient fresh water lakes in the districts which are geologically distinguished by the appellation of the basin of Hampshire and the basin of Paris.

There was still, however, a further point on which evidence appeared desirable, inasmuch as the remains of the genus Anoplotherium and other large lacustrine quadrupeds which occur in the basin of Paris, had not been ascertained to exist in England. This desideratum I have long felt anxious to supply, and in a rapid excursion to the west of the Isle of Wight two years ago, I sought for the bones of these animals in the cliffs of Headon Hill and Totland Bay, and some adjacent quarries of the interior, without finding any thing more than a small fragment too indistinct to be considered decisive of a point to which no other evidence had yet been adduced. But in the month of November last, whilst occupied in looking over the cabinets of Mr. Thomas Allan, of Edinburgh, I discovered a tooth, which he informed me he had himself collected several years ago in the Isle of Wight in the quarries of Binstead, near Ride, and which immediately struck me as belonging to one of the animals I had been so long in search of; and on my subsequently showing it to Mr. Pentland (who is accurately versed in all the details of the fossil quadrupeds of the Paris basin) he at once pronounced it to be a molar tooth of the lower jaw of the Anoplotherium commune.

The annexed drawing of the tooth in question being of the

Crown of the tooth much worn.

Base of the tooth with broken portions of the roots.



M M del.

natural size will give a more correct idea of it than can be conveyed by any description; and as the evidence of its having been found in the quarries of freshwater limestone at Binstead (I believe the lower freshwater) rests on such accurate authority as that of Mr. Allan, we may consider this important and almost only deficient link in the chain of evidence that unites the English freshwater formations with those of France to be now supplied, and hope that this discovery will stimulate others whose local position affords them opportunity, to persevere in the attempt to collect further traces of the remains of this remarkable class of extinct quadrupeds in the freshwater strata of the Isle of Wight.

Article X.

Descriptions of Two New Minerals. By Mr. A. Levy, MA. of the University of Paris.

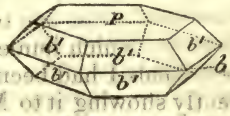
(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Oct.* 14, 1825.

Herschelite.

The substance for which I propose this name, in honour of the Secretary of the Royal Society, was brought by him from Aci Reale, in Sicily, and has not yet been noticed, I believe, as a distinct species.

Fig. 1.



It occurs in white, translucent, and opaque crystals of the form represented by fig. 1, sometimes isolated on the matrix, but most generally very closely aggregated in a manner analogous to that in which the crystals of prehnite are so frequently met with. The matrix, in the cavities of which it is found, greatly resembles lava, but upon a close examination, I found it entirely composed of small grains and crystals of olivine, several of which I have detached, and measured by means of the reflective goniometer. Dr. Wollaston, with his usual kindness, has examined chemically a small quantity of Herschelite, and has found it contains silex, alumina, and potash. These are also the constituent parts of felspar and amphigene, but the new substance most certainly differs from both by its crystallographical and other characters. The form of the crystals indicates that they are derived either from a rhomboid or a six-sided prism, but the exact dimensions of the primitive form I cannot give, on account of the difficulty of obtaining accurate measurements. The face p is always dull and curved, the faces b', though sometimes sufficiently brilliant for measurement, are generally composed of a number of planes slightly raised one above the other. The mean between several measurements gives

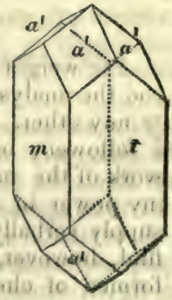
$$p, b' = 132^\circ \qquad b', b' = 124' \; 45'$$

If, therefore, we suppose the primitive form to be a six-sided prism, and the faces b' to be the result of a decrement by one row on the terminal edges, the ratio between one side of the base and the height of the prism will be nearly that of equality. I could obtain no cleavage either parallel to the base of the prism, or in any other direction. The mean of two experiments to determine the specific gravity gives 2·11. The fracture is conchoidal, and this substance is easily scratched by the knife.

Phillipsite.

Herschelite is accompanied by another substance, which, I also believe, belongs to a distinct species, for which I propose the name of Phillipsite, in honour of Mr. W. Phillips, whose contributions to mineralogy are so numerous and so valuable.

Fig. 2.



This substance occurs in minute white, translucent, and opaque crystals of the form represented by fig. 2. In the specimens from Aci Reale, these crystals are lengthened, adhere closely together radiating from a common centre, and forming globular concretions. It is also found in separate crystals disseminated on the matrix with comptonite and other substances, in specimens from Vesuvius. The form of these crystals is the same as that of harmotome, Haüy has called *dodecaièdre,* and the incidences of the faces are nearly the same. In consequence of these analogies, this substance has been considered by some mineralogists as identical with harmotome.

The incidences of the faces marked *a′* in the figure I could not obtain with great accuracy, but yet they appeared to differ constantly from those of harmotome, the most obtuse being nearly 123° 30′, and the less obtuse 117° 30′. The substance appears to cleave parallel to the planes *m* and *t*, but not in the direction of the diagonal planes as harmotome, and finally the hardness is much less. These differences induced me to request Dr. Wollaston to ascertain whether this substance could be chemically considered as harmotome. The result of his examination was, that it contained silex, alumina, potash, and lime, but not the slightest trace of barytes.

The absence of this earth, which is an essential constituent of harmotome, decides at once the propriety of separating the new substance from that mineral, and to make a distinct species of it. It is easy to verify the chemical difference between the two substances in the following manner: if a fragment of harmotome is pounded and digested for a minute or two in boiling nitric or muriatic acid, and then the liquid filtered, a drop of sulphuric acid put into it will give a precipitate, whilst there will not be the least appearance of one, if Phillipsite be treated in the same manner. I do not give the dimensions of the primitive form, because the measurements are not sufficiently accurate, but it is obvious that a right rectangular prism, or a right rhombic prism, may be assumed as the primitive.

Article XI.

On the Method of analyzing Sulphate of Zinc.
By Thomas Thomson, MD. FRS.

In my late work entitled "An Attempt to establish the First Principles of Chemistry by Experiment," I have made the analysis of sulphate of zinc the foundation on which I have endeavoured to rear the whole subsequent doctrine of the atomic weight of bodies. I was obliged to begin somewhere, and the analysis of this salt appeared as simple and as decisive as any other. I abstained from describing the processes which I followed, because I thought them rather too tedious for a work of the nature that I had projected, and because it was in my power in a book intended chiefly for my own students to supply verbally whatever was wanting in the practical part. I find, however, that I was mistaken in the opinion which I had formed of chemists, when I supposed that they would have given me credit for being acquainted with the usual methods of separating the oxide of zinc from acids. For I lately received a letter from a gentleman, of whose practical skill I entertain a high opinion, informing me that my experiments and calculations are of very little value, as *I decomposed sulphate of zinc by simply pouring carbonate of soda into a cold solution of it.*

This assertion excited in me a good deal of surprise. I do not say in my work how I performed the analysis, but merely that the results were so and so. Now if a practical chemist, who must know the usual mode of throwing down oxide of zinc by alkaline carbonates, was induced from my silence to conclude that I had experimented with so little regard to precision, as to be satisfied with a mode which would have left more than one-fifth of the oxide of zinc still in solution, I have reason to be apprehensive that those gentlemen who are only commencing the study of practical chemistry may be still more injuriously misled, that they may attempt the analysis of the sulphate of zinc without being aware of the requisite precautions, and that the quantities which they will thus procure will be so different from those which I have stated, that they will be either inclined to consider my statements as erroneous, or, what would be still more unfortunate, be discouraged from prosecuting their researches till they have satisfied themselves respecting the truth of the atomic weights which I have given. I conceive, therefore, that it will be highly proper to state with some minuteness the different modes of analysis which I found to answer best. And the safest method of proceeding seems to be to give the steps of a few of my actual analyses.

1. The sulphate of zinc of commerce is usually a compound of one atom acid and one atom oxide of zinc. But I have never met with it absolutely free from iron, and seldom without some traces of cadmium. When the salt is made by dissolving the zinc of commerce in sulphuric acid, and crystallizing the solution, you often obtain a salt containing about one-third too much acid, and this excess is not all got rid of even when the salt is dissolved and crystallized several times successively. The best way is to put a plate of zinc into the acidulous salt, and to leave it in contact with it till all excess of acid is neutralized, a process which, even when heat is applied, takes a considerable time; the best way to free the salt from iron is to dry the crystals, expose them to a red heat, redissolve in distilled water, filter and crystallize. It is only when the sulphate of zinc is pure that its constituents are as I have stated them.

2. The water of crystallization of sulphate of zinc cannot be obtained directly by experiment. I usually reduced the crystals to powder in a porcelain mortar, wrapt up the powder in several folds of blotting paper, and kept it for some time under a pretty strong pressure. 181·25 grains of the salt thus treated were put into a balanced platinum crucible, and exposed on the sand-bath in a temperature as nearly as possible of 320°, till they ceased to give out moisture. The loss of weight in several

successive trials varied from 68·5 grains to 69·2 grains, and nothing was given out but water. This I ascertained by making the experiment in a retort, and collecting the water in a receiver.

After the salt had ceased to give out water on the sand-bath, the crucible was transferred to a spirit lamp, and the heat gradually raised to redness. Water was given off at first nearly pure; but it was soon mixed with sulphuric acid fumes, and the quantity of these fumes was greatest when the salt became red-hot. The total loss of weight which 181·25 grains of crystallized sulphate of zinc sustained when thus treated, varied in different trials from 81·6 to 81·9 grains. The salt thus treated did not dissolve completely in distilled water, and the undissolved portion was always greatest when the loss of weight was greatest.

From these experiments, repeated about a dozen of times, I concluded that 181·25 grains of sulphate of zinc do not contain so much water as 81·6 grains, nor so little as 69·2 grains.

The conclusions to be drawn from these experiments will be better understood if we divide the quantities experimented on by 10. If we do so, we find that 18·125 grains of crystallized sulphate of zinc contain more water than 6·92 grains, but less than 8·16 grains. I had previously satisfied myself that the atomic weight of water is 1·125. Now six atoms of water amount to 6·75, and seven atoms to 7·875. 8·16 exceed this last number by 0·285, which is only a small fraction of an atom. The conclusion which I drew from these experiments is, that crystallized sulphate of zinc contains seven atoms of water, that when placed on the sand-bath it gives out six of these seven, and that the remaining atom cannot be separated without taking along with it a portion of sulphuric acid.

These conclusions, indeed, required to be verified by the subsequent steps of the analysis. Meanwhile it was probable that 18·125 grains of sulphate of zinc contain 7·875 grains, or seven atoms of water.

3. To obtain the oxide of zinc without loss from the sulphate, I had recourse to a variety of methods, some of which were more, others less successful. A few of these may be stated.

(1.) The quantity of acetate of barytes necessary to decompose a given weight of sulphate of zinc was found to be (supposing both salts pure and in crystals)

Acetate of barytes	193·75
Sulphate of zinc	181·25

The two salts being dissolved in these proportions, and mixed, the sulphate of barytes was collected on the filter, and the acetate of zinc was evaporated to dryness and exposed to a red heat in a balanced platinum crucible. The sulphate of barytes obtained by this process, gave the quantity of sulphuric acid in the sulphate of zinc with sufficient exactness. But there were two sources of inaccuracy which prevented me from obtaining exactly the oxide of zinc. A portion of the zinc was usually sublimed during the exposure of the acetate to a red heat. When the oxide of zinc obtained was dissolved in an acid, a little charcoal from the decomposed acetic acid usually remained behind.

(2.) The sulphate of zinc was dissolved in water, and the solution mixed with a sufficient quantity of oxalate of ammonia to throw down all the zinc. The oxalate of zinc was collected on a double filter; and the liquid which passed through, together with that employed in washing the oxalate of zinc, was evaporated to dryness, and the residue redissolved in water, to obtain any oxalate of zinc which might not have been deposited at first. The oxalate of zinc thus obtained was dried on the filter and weighed. A portion of it was exposed to a red heat, and the loss of weight determined. From this, the whole oxide of zinc contained in the salt was deduced.

This mode of analysis came much nearer the truth than the preceding. Indeed, the oxide of zinc calculated from the oxalate would be exact; but this method could not be employed, because it required the previous knowledge of the very thing which was under investigation. The oxide of zinc obtained by exposing the oxalate to a red heat, always was below the truth. The deficiency, when 181·25 grains of sulphate of zinc had been employed, varied in different trials from 1 grain to $2\frac{1}{2}$ grains, and it was never less than one grain. I ascribe this to a little zinc sublimed during the application of the heat. It was not owing to any of the zinc being in the metallic state; for I digested the residue in nitric acid without any alteration in the total weight.

4. Foiled in both of these modes of experimenting, I had recourse to the decomposition of sulphate of zinc by carbonate of soda. This method came sufficiently near the truth to satisfy myself completely respecting the true weight of oxide of zinc contained in a given weight of sulphate. Perhaps the most instructive analysis I can select will be the preliminary one, by which I determined the circumstance necessary to be attended to in order to obtain the whole of the oxide of zinc from a given weight of sulphate.

(1.) I commonly took 90·625 grains (five atoms) of crystallized sulphate of zinc; the smallest weight which I employ is 0·01 grain. I weighed out commonly 90·63 grains of sulphate of zinc, and afterwards checked this analysis by another, in which the weight of the salt was 90·62 grains. These two analyses added together gave me the quantity of oxide of zinc in 181·25 grains (10 atoms) of sulphate of zinc. It will be sufficient if I state here one of these two analyses.

(2.) The salt was weighed in a small flask; distilled water was poured into it, and it was placed on the sand bath till the salt was dissolved. 90 grains (five atoms) of crystallized carbonate of soda, previously reduced to coarse powder, were then put into the flask, and the flask was gently agitated till the whole of this last salt had dissolved. This process was usually over in about 10 minutes, and the temperature of the liquid was about 70°. As the carbonate of soda dissolved, carbonate of zinc was deposited in white flocks; this carbonate was immediately collected by pouring the whole contents of the flask upon a double filter. The carbonate of zinc was washed with distilled water till the liquid that passed through the filter ceased to produce any effect on muriate of barytes. The object of this part of the experiment was to ascertain how much carbonate of zinc would be precipitated from a cold solution of five atoms of sulphate of zinc by five atoms of carbonate of soda.

The carbonate of zinc thus obtained was dried upon the filter in a temperature, which never much exceeded 212°. It was then weighed by placing the balanced filters in the two opposite scales of the balance; the weight in different experiments varied from 29·3 to 31·03 grains; as much of this carbonate as possible was detached from the filter, and after being weighed in a balanced platinum crucible of a very small size, was heated to redness in the flame of a spirit lamp. From the loss of weight sustained by the portion thus treated, it was easy to infer how much the whole carbonate would have lost; there remained 20·37 grains of oxide of zinc. There was scarcely any difference in the amount of this weight in different experiments, provided sufficient care had been taken not to vary the previous steps of the process.

(3.) The liquid which had passed through the filter together with all the washings (properly concentrated) was put back into the flask. It had the property of rendering cudbear paper purple; but after being boiled for about half an hour it was capable of reddening vegetable blues. It was obvious from this, that the whole of the soda had not united with the sulphuric acid of the sulphate of zinc; but that this union was effected by half an hour's boiling. During the boiling, an additional precipitate fell, not in loose white flocks as the first precipitate, but in a powder which was less white and much heavier; for it fell much more rapidly to the bottom. The liquid thus treated was thrown upon a double filter, and the powder (6) remaining on the filter, was washed with distilled water till the liquid ceased to affect muriate of barytes.

(4.) All this liquid that had passed through the filter (properly concentrated) was again poured into the original flask, and a solution of carbonate of soda was added to it till the liquid contained a decided excess of alkali. This new alkaline liquid occasioned the appearance of a new precipitate which was in white light flocks, like the carbonate of zinc which had been thrown down cold. After this precipitate had subsided, the flask was heated by a spirit lamp and kept boiling for about 20 minutes; the whole was then poured upon the same double filter upon which the second precipitate (6) had been collected; and the filter was washed with distilled water till it ceased to produce any alteration on muriate of barytes.

The precipitate collected on this filter was now dried on the filter and weighed. Its weight was 6·07 grains. After exposure to a red heat it was reduced to 4·54 grains.

(5.) The liquid thus freed from all the oxide of zinc that could be thrown down by boiling was put into a porcelain dish, and slowly evaporated to dryness on the sand bath. The dry residual salt being redissolved in water, a few flocks of oxide of zinc separated. These collected and dried on the filter weighed 0·44 grain, and when heated to redness were reduced to 0·431 grain.

(6.) The liquid containing the residual sulphate and carbonate of soda was again evaporated to dryness in a platinum vessel, and the dry residue exposed for an hour to a strong red heat. The salt thus treated when dissolved in water deposited a few black flocks. These being collected and exposed to a red heat in a platinum spoon became grey, and weighed 0·3 grain. Being digested in nitromuriatic acid, the bulk diminished, and a portion was dissolved. The solution was colourless, and was precipitated in white gelatinous flocks by prussiate of potash, showing that it was oxide of zinc. The undissolved portion was not acted on by any acid, but it fused before the blowpipe with carbonate of soda into a white globule, and the solution was accompanied with effervescence. Hence I considered it as silica; consequently the precipitate was silicate of zinc, and it contained 0·22 grain of oxide of zinc.

I do not know the cause of the black colour which this powder had at first. It seems to have been owing to the presence of some combustible matter, as it was dissipated by heat. The platinum crucible was covered with a lid all the time that it was in the fire, so that no charcoal could have reached it from the fuel.

The carbonate of soda used was pure, consisting of crystals picked with great care from Mr. Tennant's evaporating pans. Hence I am disposed to ascribe the origin of the silica to the glass flask in which the mixture had been so long boiled.

(7.) The solution from which the silicate of zinc had been deposited was neutralized with muriatic acid, and then mixed with a few drops of hydrosulphuret of ammonia, and the mixture left to digest for 24 hours in a very moderate heat. On examining the flask containing this mixture next day, I observed a deposition of a few dark coloured flocks; the supernatant liquid was drawn off with a syphon, and the flask again filled with distilled water. Next day the liquid was again drawn off and fresh distilled water poured in; this process was repeated till the water drawn off had become pure; the precipitate was now dried and exposed to a red heat in a glass capsule; in this state it weighed 0·9 grain: it had a yellow colour, was tasteless and fixed in the fire. Being digested in nitromuriatic acid it slowly dissolved, leaving a portion of sulphur. The solution was precipitated white by muriate of barytes, and in white gelatinous flocks by prussiate of potash. Hence it consisted of sulphuric acid and oxide of zinc. From this it is obvious, that the yellow matter was sulphuret of zinc; and it must have contained the equivalent of 0·65 grains of oxide of zinc.

(8.) If we collect all the oxide of zinc obtained in these different processes, we shall find them as follows:

	Grains.
From the carbonate	20·37
Thrown down by boiling	4·54
Obtained by evaporating to dryness.	0·431
From the silicate of zinc	0·22
From the sulphuret of zinc	0·65
Total	26·211

Now 26·211 divided by 5, gives 5·245 for the quantity of oxide of zinc contained in 18·125 grains of sulphate of zinc.

According to this determination, the atomic weight of oxide of zinc is 5·245; this, I am persuaded, is about $\frac{1}{1000}$th part below the truth; I believe that in the preceding analysis I lost 0·039 grain of oxide of zinc, which constituted about $\frac{1}{673}$ part of the whole. The loss I conceive to be owing to the want of a substance capable of precipitating the whole of the oxide of zinc from its solution in sulphuric acid. Carbonate of soda does not throw it down completely; and I can affirm from experiments made with care, that hydrosulphuret of ammonia likewise acts imperfectly.

My experiments, though numerous, never gave me more oxide of zinc from 18·125 of sulphate than 5·245 grains of oxide of zinc. But this quantity I can always get when I take the requisite pains.

5. I have detailed rather minutely my mode of determining the water and oxide in sulphate of zinc; but it will not be necessary to describe with equal minuteness the method fol-

lowed to determine the quantity of sulphuric acid. The process has been already given in the *Annals of Philosophy*, vol. i. p. 246, New Series. It consisted in dissolving 18·125 grains of sulphate of zinc, and 13·25 of chloride of barium in as little water as possible, mixing the solutions; and after the sulphate of barytes has subsided, testing the clear supernatant liquid with muriate of barytes and sulphate of soda, and finding that the liquid is affected by neither. From this experiment I concluded, that 18·125 grains of sulphate of zinc contain exactly five grains of sulphuric acid.

I made an experiment on purpose to ascertain how small a portion of sulphuric acid could be detected in water by this method. One grain of glauber salt being dissolved in 12000 grains of water, I found that the solution was distinctly precipitated by muriate of barytes. Now, one grain of glauber salt contains rather less than $\frac{1}{4}$th grain of sulphuric acid. The liquid in which I had dissolved the 18·125 grains of sulphate of zinc and 13.25 grains of chloride of barium did not amount to so much as 500 grains; hence had so much as $\frac{1}{96}$th of a grain of sulphuric acid remained in solution, it would have been rendered visible by the muriate of barytes dropt into the liquid. I was sure, therefore, from this experiment, that the sulphuric acid in 18·125 grains of sulphate of zinc is not less than five grains, and not so much as 5·01 grains. There could be no hesitation in concluding that the exact quantity was five, especially as this is the atomic weight of sulphuric acid.

Knowing that 18·125 grains of sulphate of zinc contain five grains of sulphuric acid, and that the oxide of zinc is not less than 5·245 grains, and the water not so much as 8·16 grains; and knowing farther, that the water in crystallized salts constitutes a certain number of atoms, and that the atom of water weighs 1·125; there was no longer any difficulty in determining the true atomic weight of oxide of zinc, and the exact quantity of it contained in 18·125 grains of sulphate of zinc.

Let the atom of oxide of zinc = x, and let 18·125 sulphate of zinc be a compound of

$$\begin{aligned} &1 \text{ atom sulphuric acid} = 5 \\ &1 \text{ atom oxide of zinc} \;\; = x \\ &7 \text{ atoms water} \ldots\ldots = 1{\cdot}125\,y \end{aligned}$$

We have $5 + x + 1{\cdot}125\,y = 18{\cdot}125$

We know that x is not less than 5·245, and that y is more than 6, and not more than 7. If we make $y = 7$, then we have

$$5 + 7{\cdot}875 + x = 18{\cdot}125, \text{ and consequently}$$
$$x = 18{\cdot}125 - 12{\cdot}875 = 5{\cdot}25.$$

Nor can any numbers be substituted for y and x consistently with the preceding experiments, except 7 and 5·25.

These were my reasons for pitching upon 5·25 as the atomic weight of an atom of zinc. I consider the analysis by means of chloride of barium as the experimentum crucis from which, knowing by approximation the quantity of water and oxide of zinc, we can determine the amount of each with mathematical accuracy. The uncertainty respecting the quantity of sulphuric acid may be reduced almost without limit by concentrating the liquid before applying the test of muriate of barytes; and I made myself certain that the error which I could have committed did not amount to $\frac{1}{1000}$th of a grain by that method.

6. Having determined the composition of sulphate of zinc in the way just stated, I calculated the composition of carbonate of zinc, as given in page 60 of my late work, in this way. The carbonate of zinc obtained by precipitation from 90·63 grains of sulphate of zinc, weighed 31·03 grains, and was composed of

Oxide of zinc	20·37	or 5·25
Carbonic acid	10·66	2·747
	31·03	

Now 2·747 approached so near 2·75, which I knew to be the atomic weight of carbonic acid, that I considered myself entitled to consider the carbonate of zinc when anhydrous as a compound of

1 atom oxide of zinc	5·25
1 atom carbonic acid	2·75
	8·00

I thought it better merely to state the general results in my preliminary chapters than to enter into tedious details. Whoever will take the trouble to repeat the analyses which I have given with the requisite care will obtain results not deviating more than $\frac{1}{1000}$th part from those which I have given. I am afraid that in actual analyses we can seldom come nearer the truth, except indeed by peculiar contrivances, some of which I employed in determining the fundamental points of the atomic weights of bodies.

Article XII.

On the Mathematical Principles of Chemical Philosophy.

By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Great Ouseburn, Sept. 7, 1825.*

Since the discovery of galvanism, the powers of electricity as a chemical agent, have been clearly made out, yet this part of science is not fully developed; every fact, therefore, which throws any light upon the subject is of importance, since from phenomena we discover the laws of nature.

Those bodies which can enter into chemical union, stand in different electrical relations to each other; when a compound is exposed to the action of the galvanic series, its elements are separated from each other; oxygen and acid matter generally tend to the positive pole, whilst hydrogen and inflammable matter appear at the negative. Hence philosophers generally explain chemical combination and decomposition upon the principles of electrical attraction and repulsion: and the theory most generally received is that the ultimate particles of matter possess certain electrical energies, which are immutable; oxygen, upon this hypothesis, is highly negative, and all acids possess the negative energy in different degrees of intensity; whilst inflammable bodies and bases have various degrees of positive energy; hence, since there is an attraction between two bodies, when one is in a positive and the other in a negative state, oxygen can combine with inflammable matter, and acids with the different bases; also since oxygen is negative, it will tend to the positive pole, and inflammable bodies will arrange themselves at the negative. Chemical philosophers are not agreed respecting the existence of electric energies, and in fact, in the present state of science, it is impossible to set the question at rest; reasons, almost of equal weight may be advanced in support of, and against their existence.

That the elementary particles of matter may have permanent energies, appears from many electrical facts: lay a black silk ribbon upon a white one, and excite them, by drawing them through any soft exciting substance; no signs, or at most, very feeble ones, of excitation will appear; separate them, and they will possess the opposite states of electricity; replace them, and the electricity disappears; and on separating and replacing them alternately, the signs of electricity may be exhibited or suppressed many times with one excitation. These phenomena may be exhibited by all non-conducting substances; but if conductors be insulated, and electrified with the opposite powers, on bringing them together, an equilibrium is produced, and on separating them, they show either no signs of electricity, or are electrified with the same power. The cause of this difference is by no means apparent; the power of induction is sometimes conjured up to account for such phenomena as these; however, this is only to hide a difficulty under a name of very doubtful meaning and uncertain application. However, if such energies exist, and be the cause of chemical changes, they must remain permanently and unchanged, after combination, as they continue in the case of the ribbons; for then the attraction is permanent, and no electricity appears; for if they be not unchangeable, as the particles of matter themselves, then, as in the case of conductors, contact produces an equilibrium, and there is no attraction. If then energies do exist, the particles of all bodies, even of metals, must act upon each other as non-conductors; this may possibly be the case, for the conducting or the electric power of bodies is not inherent or permanent; glass, when heated to redness is a conductor; ice cooled considerably below zero is a non-conductor: this property of bodies, therefore, depends upon something foreign; yet it is difficult to imagine how the particles of metals can act upon each other as non-conductors; and if energies exist, this must be the case. Also if electric energies be the cause of chemical combination, it follows that no solid can be a simple substance, except we suppose one force to produce cohesion, and another chemical attraction; such a supposition is inadmissible, because we have no evidence of the existence of such forces; and likewise electrical repulsion (a doubtful power) is generally supposed to be as powerful as attraction, intensities being equal; the force of chemical attraction is usually more powerful than that of cohesion; therefore the action of electric energies must prevent any simple substance from assuming a solid form. Other philosophers suppose the existence of such a relation of chemical attraction to electricity, that the intensity of the chemical attraction of the particles of matter determines their place in the galvanic series; if this be the case, the electrical relations of bodies may be taken as the measures of their chemical action. This supposition is more free from hypothetical views than the other, and is, therefore, entitled to preference until our knowledge shall be more precise.

Every relation or analogy that can be clearly and definitely made out between the force of gravity, chemical attraction, caloric, and electricity, is of the utmost importance to science, because only by such facts can the laws of chemical action be developed. Hitherto chemistry and the mathematical sciences have been entirely insulated; but such is the precision of its phænomena, that the laws by which chemical changes are produced must be as definite and as susceptible of geometrical investigation as those which are displayed in the solar system.

To suppose every chemical change susceptible of mathematical demonstration would be extravagant; but that the laws of action may be made out may be fairly supposed; it receives the sanction of the immortal Newton, than whom a more competent judge never existed, although chemistry had scarcely begun to assume the form of a science in his days: his opinion (and the opinion of such a man ought to have great weight), may be collected from the following part of the preface to his Principia: —" Utinam cætera naturæ phænomena ex principiis mechanicis eodem argumentandi generi liceret. Nam multa me movent, ut nonnihil suspicer ea omnia ex veribus quibusdam pendere posse, quibus corporum particulæ per causas nondum cognitas, vel in se mutuo impelluntur, et secundum figuras regulares cohærent, vel ab invicem fugantur et recedunt; quibus viribus ignotis, philosophi hactenus naturam frustra tentarunt. Spero autem quod, vel huic philosophandi modo, vel veriori alicui principia hic posita lucem aliquam præbebunt."

Analogy 1.—The most inflammable bodies have the least force of attraction to the earth.

In a former communication I demonstrated that the diameter of a particle of any simple substance is proportional to $\sqrt[3]{\frac{\text{atomic weight}}{\text{specific gravity}}}$; the body being in its most dense solid state: also, that if the force of attraction of an ultimate particle of matter belong to the entire mass of the atom, the intensity of its force of gravity is proportional to its specific gravity, if the particles of solids be always similarly situated; but that if it belong to the surface only, to which opinion I incline, for many cogent reasons, the same force is proportional to specific gravity × diameter of a particle, or to its equal $\sqrt[3]{\left\{\text{atomic weight} \times \{\text{specific gravity}\}^2\right\}}$. By this attraction I do not understand the entire gravitation of a particle, which is the product of the intensity of the force, and the square of the diameter, or cube of the diameter, according as the force belongs to the surface or the solid content of an atom; but the intensity only of the force, or, as it may be called, the density of a particle. The following table gives the force of some of the chief undecompounded substances; the atomic numbers are from Brande's Manual. The gaseous bodies ought to be included, but at present the data are insufficient.

	Force.
Gold	78·3773
Silver	52·6365
Copper	39·5736
Iron	34·6054
Lead	55·0248
Tin	33·8793

Zinc	27·8530
Phosphorus	7·4641
Sulphur	9·2355
Carbon	1·5560? to 5·5450?

In the present state of science, these numbers, as well as others that I have made public, are but approximations; for in deducing them, the particles of all solids must be supposed to be similarly situated, which is not the case; and until the primary laws are fully developed, their relative positions in different solids cannot be determined. By inspecting the table, which might have been much enlarged, it appears that the most inflammable bodies have the least force of attraction of gravitation; whilst gold, silver, and lead, have great forces, and are scarcely inflammable: iron, tin, and zinc, are very inflammable, and have smaller forces; and phosphorus, sulphur, and carbon, have very small forces, and are very highly inflammable. If the order of specific gravity be compared with that of inflammability, the same analogy nearly is observed; and could we discover how far the specific gravity is affected in every particular case by differences in the arrangement of the particles of different substances, the coincidence would in all probability be much closer.

Analogy 2.—Those substances which differ most from oxygen in electrical relation have the smallest force of gravity. In all galvanic decompositions, oxygen appears at the positive, and inflammable matter at the negative pole; those bodies which are most remote from oxygen in the galvanic series have the most powerful attraction for it: by the last analogy these are the substances which have the least intense attraction of gravitation. From the experiments of Sir H. Davy and others (Phil. Trans. 1807), when sulphur is separated from contact with a metallic plate, it is in a positive, and the metal in a negative state of excitation. Also phosphorus separates most, and charcoal many metals from their acid solutions; and from their general galvanic relations, they undoubtedly stand above zinc in order of galvanism.

Since then those substances have the least intensity of attraction to the earth which tend most powerfully to the negative pole, or, which is the same thing, which have the greatest attraction for oxygen, either there is a remarkable accidental coincidence, or the tendency of bodies to the poles of the galvanic series, is determined by, or determines their tendency to the earth. To suppose it accidental, since it extends to all bodies of which we have sufficient data, would be extravagant; for in all branches of science we are led by phænomena to a knowledge of the laws of action. To deny the validity of evidence of this sort would be to take away the foundation of every branch of natural philosophy. Supposing the particles of matter to possess immutable electrical energies, one of these conclusions must follow; either, first, the earth's attraction is made up of the sum of the energies of all its component atoms; then on the principles of electrical attraction, those bodies which differ most in energy from oxygen differ least from the mean energy of the earth. Or, secondly, the force which determines the place of a body in the galvanic series, and which seems to be the same with chemical attraction, bears such a relation to gravity, that those bodies which differ most from oxygen in electrical relation have the least tendency to the earth. The second conclusion seems to follow, if electrical energies do not exist; it appears to be the most probable, and may be safely assumed, because it involves no hypothetical views, being but a bare statement of observed phænomena.

Analogy 3.—The most inflammable bodies, i. e. those which have the least force of attraction of gravity, have the least atomic capacities for heat. The atomic capacity = capacity of a given weight × atomic weight. The following table exhibits the atomic capacities of some undecompounded substances:

	Atomic capacity.
Gold	0·64
Silver	1·07
Copper	0·88
Iron	0·89
Lead	0·52
Tin	0·52
Zinc	0·44
Sulphur	0·38
Carbon	0·20
Mercury	1·00
Nickel	0·37
Antimony	0·36
Bismuth	0·35

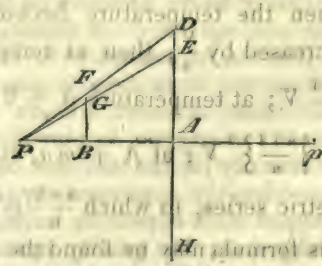
The numbers in this table do not follow the precise order; for the atomic weights are not only uncertain in some degree, but the capacities for heat of the metals are by no means determined to any thing like precision. Capacities determined by different chemists are exceedingly variable; however the analogy is evident in non-metallic bodies. In this and all other tables, I have used the experiments of others in every case. In the calculations, the extreme numbers have been employed; and the minor differences are amply sufficient to interrupt the order very considerably. If the atomic capacities of all the solids supposed to be simple be compared, the analogy will be found to apply to all, although there are small differences in the case of some of the metals, which most probably arise from imperfect data.

Hence those bodies whose electrical relation is the most remote from oxygen, or which tend most powerfully to the negative pole of the galvanic series, or which have the least tendency to the earth, have also the smallest atomic capacities ; also the electrical relation of oxygen is opposite to that of caloric, or, in other words, the bodies which approach most nearly to oxygen in electrical relation, have the greatest attraction for caloric ; and, therefore, the atomic capacities of oxides are greater than those of the metals.

If the specific gravities be compared with the table, the same order nearly is observed ; but since we do not know the relative arrangements of the particles of different substances, we are obliged at present to consider them always similarly situated ; this may cause a difference of about one-third the specific gravity.

Analogy 4.—Those atoms which evolve most heat during their combination, are generally the lightest. By inspecting the table of forces, carbon, sulphur, and phosphorus, which have the smallest forces, evolve more heat than zinc, tin, or iron, whose force is greater ; and these, more than copper, silver and gold. Lead forms an exception, and in fact the only exception, in every case. Until more shall be known of the laws by which combination is effected, we need not be surprised if disagreements appear, for oxygen in combination does not always possess the same specific gravity ; in the glass of antimony its specific gravity is 2·21 ; in phosphoric acid 5·1 ; in oxide of arsenic 1·4 ; in red lead 3·2 ; oxide of manganese 3·1 or 2·7 ; red copper ore 1·47 ; iron mica 1·36. Therefore more caloric remains in combination with the oxygen in some compounds than in others. The results deduced from the best data which are before the public, are not so coincident as might be desired ; for not only are the capacities for heat and the atomic weights variously ascertained, but, as I remarked in a former communication, the specific gravities of porous bodies are very incorrect, owing to the manner in which they are ascertained ; and lest I should seem to arrogate the ability of deciding those numbers on which the most eminent chemists are not agreed, or of deducing conclusions from my own experiments, calculated to favour the principles I advance, I have not argued from them in any instance, but have taken those of the most eminent modern chemists. The great apparent density of oxygen in phosphoric acid appears to arise from a peculiarity in the texture of phosphorus: its density may be considerably increased by compression ; and therefore the oxygen must appear to have more density in the acid than it really has ; its gravity was computed by the following formulæ ; specific gravity of oxygen in an oxide $= \frac{n \cdot a \cdot c}{(m+n) \times a - m \cdot c}$;

in which m is the weight of the metal; n, that of oxygen; a the specific gravity of the metal, and c the specific gravity of the compound. From this the reason is evident, why the oxygen in some oxides, as that of mercury, gold, silver, the peroxide of lead, is highly energetic, whilst in others, as protoxides of iron, manganese, potassium, &c. its power as a supporter of combustion is scarcely sensible. The conclusions here drawn depend not upon any hypothetical views; they are merely deductions from observed phenomena: I have no desire to institute any researches into the nature of electricity or gravity; because in all probability we shall never know more of them than we do at present; and if known, perhaps science would receive but little benefit from the discovery; I have endeavoured simply to ascertain, from phenomena, their relative effects as chemical agents. Another law of chemical action merits attention: in general, a simple substance will not combine with a compound. No simple substance is soluble in water; the small quantity of oxygen, hydrogen, or azote that is frequently found, cannot be regarded as chemically united. Chlorine is soluble: but its elementary nature is very doubtful; it certainly has never been proved; its high atomic number is against the idea; for, the metals excepted, together with iodine, which is in the same predicament as chlorine, elementary, or rather undecompounded substances, have very low numbers. The solutions of sulphur and phosphorus in oils or ether, or the sulphurets of alkalis are not exceptions, because decomposition accompanies the solution. There are certainly triple compounds, particularly in animal and vegetable substances, which seem to be exceptions; but this is a class of phenomena of a different nature; three atoms of different substances may be combined, as in the prussic acid; but we find that generally a simple or undecompounded substance cannot be united directly to one that is compound. It is of importance to determine experimentally, the law of repulsion in gases; two gaseous particles repel each other with a force which is very nearly as the distance inversely (Newton's Principia, lib. 2, prop. 23.) This is the aggregate effect of the force between the particles, but it is not the variation in the elastic force of the calorific atmosphere. Let P and p be two particles of a gas; bisect P p in A; and through A pass a plane D A H, perpendicular to P p, which divides the calorific atmospheres, and therefore in which the repulsive force is exerted. Take E indefinitely near to D: join P D, P E; and with the centre A and radii



A D, A E, describe two circles: the space between the circles will be an annulus, whose breadth is D E. Let the repulsive force at any point D in the annulus be D P^n. The force of the annulus unresolved will be D P^n × area of the annulus. Through any other point B in P *p*, pass a plane similar and similarly situated to the former; when the distance P*p* becomes 2 P B, the plane E B F will divide the calorific atmospheres, because the angle D P A, and therefore 2 D P A is constant; therefore B F : A D :: P B : P A. With radius B F and centre B describe a circle, also with radius B G. The force of this annulus is as F P^n × area of the annulus. Since the forces are similarly resolved, the resolution may be neglected;

and force at F : force at D :: P D^n : P F^n
and annulus F G : annulus D E :: P F^2 : P D^2; therefore, force of annulus F G : that of annulus D E :: P D^{n-2} : P F^{n-2} :: $\frac{1}{PF^{n-2}} : \frac{1}{PD^{n-2}}$.

And since the two planes may be divided into the same number of similar and similarly situated annuli, of which the forces have all the same ratio to each other, the entire forces have the same ratio. But at the same temperature, the whole force between two adjacent particles of a gas is very nearly proportional to the distance between them inversely; therefore $\frac{1}{PF} : \frac{1}{PD} :: \frac{1}{PF^{n-2}} : \frac{1}{PD^{n-2}}$; thence $n = 3$; or the elastic force of the calorific atmosphere is very nearly proportional to the cube of the distance inversely; which is the ratio nearly of the difference between the attractive force and the repulsive power of caloric. The rate of expansion of gases by heat has been obscurely expressed by most chemists: they lay down as the datum, that the change of temperature of one degree produces a change of the $\frac{1}{480}$th part of the volume of a gas; and yet the differences of temperature being equal, the volumes given in their tables are in arithmetical progression. Let V be the volume of a gas, at any temperature A; let the increments of temperature be *a*, 2 *a*, 3 *a*, &c. Then at temperature A, volume = V; when the temperature becomes A + *a*, let the volume be increased by $\frac{V}{n}$; then at temperature A + *a*, the volume is $\frac{n+1}{n}$ V; at temperature A + 2 *a*, it is $\left\{\frac{n+1}{n}\right\}^2$ V; at A + 3 *a*, it is $\left\{\frac{n+1}{n}\right\}^3$ V; at A + *m a*, it is $\left\{\frac{n+1}{n}\right\}^m$ V; which is a geometric series, in which $\frac{n+1}{n}$ is the common ratio. By help of this formula may be found the elastic force of a confined portion of gas at different temperatures. Let the temperatures and volumes be the same as before: when the temperature

$$\begin{aligned}
&\text{is A, let the volume} = V\,; \\
&\text{at } A + a, \text{ let it be } = \frac{n+1}{n} V\,; \\
&\text{at } A + 2\,a, \ldots\ldots = \left\{\frac{n+1}{n}\right\}^2 V; \\
&\text{at } A + m\,a \ldots\ldots = \left\{\frac{n+1}{n}\right\}^m V.
\end{aligned}$$

Since the elastic force is balanced by the pressure of the atmosphere, which is supposed to be constant, the elasticity of the expanded gas is constant. Whilst the temperatures are A, $A + a$, $A + 2\,a$, &c. suppose the volumes $\frac{n+1}{n} V$, $\left\{\frac{n+1}{n}\right\}^2 V$, &c. be compressed till they occupy the same space V; the elastic force is nearly proportional to the density, i. e. inversely as the volume; therefore, at $A + a$, the elastic force before compression : that after it :: $\frac{1}{\frac{n+1}{n} V} : \frac{1}{V} :: \frac{n}{n+1} : 1 :: n : n + 1$ at $A + 2\,a$, elasticity before compression : that after it :: $\frac{1}{\left\{\frac{n+1}{n}\right\}^2} : 1 :: n^2 : \overline{n+1}^2$: which also is a geometrical series. Therefore, if a confined portion of a gas be heated, the temperature is proportional to the logarithm of the elastic force; or if it be under a constant pressure, the temperature is the logarithm of the volume. These conclusions depend upon the hypothesis that the elastic force of a gas is proportional to its density, which is not quite correct; but when the change of temperature is not very great, it is an approximation sufficiently exact for all practical purposes.

The ratios of the calorific atmospheres of different bodies may be found as follows: Take D proportional to the distance between two adjacent particles in a solid, i. e. to the atomic diameter: heat the substance until it fuses: let this distance be $D + n$; heat it until the distance is increased by any known quantity $p\,n$; the densities are $\frac{1}{D^3}$, $\frac{1}{(D+n)^3}$, $\frac{1}{(D+pn)^3}$. Since the force of attraction is proportional to $\frac{1}{D^2}$, $\frac{1}{(D+n)^2}$, $\frac{1}{(D+pn)^2}$, it is easily found; and it is equal to the force of repulsion. In another substance, find the corresponding quantities d, $d + m$, $d + \pi\,m$; $\frac{1}{d^3}$, $\frac{1}{(d+m)^3}$, $\frac{1}{(d+\pi\,m)^3}$, &c. Then by help of the problems 80, 81, 90, 91, and 92, of lib. 1, of Newton's Principia, the law of variation of the force may be found from the aggregate.

J. B. Emmett.

Article XIII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Reciprocal Action of Hydrosulphuric (Sulphuretted Hydrogen) and Carbonic Acid, on the Carbonates and Hydrosulphates.* By M. Henry, Jun.

Although M. Chevreul had shown that carbonic acid is capable of decomposing the hydrosulphates, (*hydrosulphurets,*) yet when M. Henry advanced the opinion that the sulphuretted hydrogen disengaged from the mineral waters of Enghien, is owing to the action of free carbonic on the hydrosulphates contained in those waters, it met with considerable opposition, in consequence of which he resumed the subject, and undertook a series of experiments with a view to elucidate it; from which he has deduced the following conclusions:

1. Carbonic acid, in contact with the alcaline, or magnesian hydrosulphates, is capable of decomposing them completely, if the action be continued for a sufficient length of time.

2. The decomposition is effected either by boiling a hydrosulphate in water impregnated with carbonic acid, or by placing the mixture, without heat, in the vacuum of an air-pump; or by passing a current of carbonic acid gas, through a diluted solution of the hydrosulphate.

3. The hydrosulphates obtained by converting sulphates into sulphurets, by carbonaceous matter, are less readily acted on.

4. The result of the decomposition of all these salts is the production of carbonates, or rather of bicarbonates, and the quantity of sulphuretted hydrogen disengaged is proportionate to that of the carbonate formed.—(Bulletin des Sciences.)

2. *Carbono-Phosphate of Soda.*

There is a prussian blue manufactory in the neighbourhood of Glasgow belonging to Mr. Macintosh, in which, likewise, prussiate of potash is made in very considerable quantities. The acid is obtained by the combustion of the hoofs of black cattle, imported chiefly from Ireland; and the hoofs of a thousand cattle are required for every day's consumption in the manufactory. The slaughtering of cattle in Ireland having considerably diminished at the end of the last war, hoofs became scarce. This induced Mr. Macintosh to substitute the animal substance called cracknales, procured chiefly, I believe, from the candle makers. Soon after this substitution, considerable quantities of a white salt in fine needles began to make their appearance in the prussiate of potash leys, and incommoded the process considerably. These crystals exhibiting appearances different from any of the common salts, Mr. Macintosh sent me a quantity of them to ascertain their nature.

The crystals were pretty regular six-sided prisms, which were obtained of a pretty large size by a second crystallization. The taste was cooling, and alkaline, and they rendered cudbear paper violet, indicating the presence of an alkali. The salt was pretty soluble in water, and the crystals were not altered by exposure to the air. They effervesced slightly, but distinctly in nitric acid.

I neutralized a portion of these crystals by means of nitric acid, and then mixed the solution with a sufficient quantity of nitrate of barytes—a white precipitate fell, which, when washed, and dried on the filter, was a beautiful white soft powder, which dissolved without effervescence in nitric acid, was again precipitated by ammonia, and exhibited all the properties of phosphate of barytes. I therefore decomposed a portion of it by means of sulphuric acid. The acid which I obtained possessed the following properties:

It threw down nitrate of barytes, and nitrate of lead white, and both precipitates were dissolved by nitric acid. Persulphate of iron was thrown down white, and the precipitate became red when digested in potash ley. Nitrate of silver was thrown down yellow, and the precipitate was dissolved in nitric acid. Muriate of lime, muriate of magnesia, nitrate of strontian, sulphate of copper, sulphate of zinc, nitrate of mercury, were not precipitated. From these properties, there could be no doubt that the acid in the salt was chiefly the phosphoric.—I now dissolved a quantity of the salt in water, and neutralized it exactly with sulphuric acid; the solution was then concentrated, and set aside. It shot out into crystals of sulphate of soda, and phosphate of soda, easily recognized by their shape.

To ascertain the proportions of the constituents, I dissolved 200 grains of the crystals in water, neutralized the solution with nitric acid, and precipitated by nitrate of lead. The precipitate weighed 141·3 grains, equivalent to 28·26 grains of phosphoric acid. The residual liquid contained no lead, but was entirely nitrate of soda, weighing 109·5 grains, equivalent to 40·74 grains of soda. Now, 28·26 grains of phosphoric acid require for saturation 32·297 grains of soda. There remain 7·443 grains of soda, which require for saturation 5·117 grains of carbonic acid.

Thus, the constituents of the salt are

Phosphoric acid	28·260	or	14
Carbonic acid	5·117	..	2·535
Soda	40·740	..	20·182
Water	125·883	..	62·36
	200·000		

Now,

4 atoms phosphoric acid	=	14
1 atom carbonic acid	=	2·75
5 atoms soda	=	20
55 atoms water	=	61·875

Hence, I am disposed to consider the salt as a compound of

4 atoms phosphate of soda	30
1 atom carbonate of soda	6·75
55 atoms water	61·875
	98·625

If this be really a compound of phosphate and carbonate of soda, the union is very slight; for I found that by repeated solutions and crystallizations, I could separate from it phosphate of soda in the usual rhomboidal form. What leads to the notion that it is a compound salt, is the form of the crystals—a six-sided prism apparently regular, which could not be derived from the primary form of phosphate of soda. Nor is the water of crystallization what it would be if the salt were a mere mixture of 4 atoms of phosphate of soda, and 1 atom of carbonate of soda; for phosphate of soda containing 12 atoms, and carbonate of soda 10 atoms, the water of crystallization should have amounted to 58 atoms, instead of 55—the quantity found, unless there was an error in the analysis. I do not well see, however, how any supposed error could serve to diminish the apparent quantity of water in the salt; but it is possible that the salt may have sustained a loss of water before I began to examine it.—(Dr. Thomson's Attempt to establish the First Principles of Chemistry.)

Mineralogy.

3. *Beryl in Cornwall.*

We have been favoured by Mr. A. R. Barclay, with a notice of his discovery of the existence of the Beryl, in Cornwall; he states that the crystals are nearly colourless, though some have a very faint greenish hue; they are six-sided, the summits of a few have the edges of the plane taken off, but most of them are flat; they are very small, and some thickly grouped; with the blowpipe they become of an opaque white. They are accompanied with apatite and mica, crystallized upon a dark grey quartzy wall of a vein which so commonly traverses the granite and schist of St. Michael's Mount, and in which most of the minerals to be met with at that interesting spot occur. They were not discovered in situ, but found in one large mass or slab, lying detached close to the little cliff of debris, on the south side of the Mount, at that part only which would be washed by very high or stormy tides.

4. *Sodalite of Vesuvius.*

The crystals are distributed in drusy cavities of limestone, and associated with grey feldspar, pale green mica, calcareous spar and augite; the latter of various tints of colour, and often perfectly white and transparent. Form, that of dodecahedrons in combination with the hexahedron, elongated in the direction of one of the rhombohedral axes. Cleavage pretty distinctly parallel to the faces of the dodecahedron, but much interrupted by conchoidal fracture. The surface of some of the crystals is rather smooth, but does not possess much lustre, which is vitreous; more generally, the edges are rounded off, and the faces curved so as to obliterate the regular form. Colour varies from pale greenish-white, to a sky-blue. Hardness 5·5 to 6 of Mohs' scale, between apatite and feldspar, nearest the latter. Specific gravity = 2·349.

The comparison of these crystals, of which there are several varieties, is connected with the question whether sodalite, spinellane, haüyne, and crystallized lapis lazuli are distinct species. The new varieties of the *Vesuvian sodalite,* examined by Mr. Haidinger, are in the Royal Museum at Edinburgh, and were arranged by Professor Jamieson with haüyne, which substance appears to belong to the same species; and M. M. Bergemann, Nöggerath and Von Gerolt are of opinion that the preceding substances, especially the haüyne and spinellane, form varieties of one and the same species.—(Extracted from Mr. Haidinger's paper, Edin. Phil. Journal.)

5. *Boracic Acid in Lava?*

According to Moricand, in the Æolian Isles, boracic acid assists in rendering the lavas more easily fusible. Do the lavas and obsidian of Lipari really contain boracic acid? The greenstone of Salisbury Craigs in this neighbourhood, contains Humboldtite, a mineral rich in boracic acid. Does the mass of the greenstone rock contain any of this curious substance?—(Edin. Phil. Journal.)

Zoology.

6. *On the Circulatory System of Saurii.* By Prof. Harlan.

I was desirous of comparing the structure of the heart with that organ in the crocodile, which is very unlike the heart of the *Turtle,* to which Cuvier, in his Comparative Anatomy, has compared it. As no correct description of the anatomical structure of the Saurii has ever been given to the public, I shall offer a brief outline of these organs in the crocodilus lucius, which will serve as the type of all the lacerta.

1. I forced air into the vena cava ascendens, which inflated the right auricle and ventricle, and passed into the lungs through the pulmonary artery into the splanchic aorta; also into the systemic aorta through the valvular opening at its base; the blood into the superior cavæ regurgitated.

2. I forced air into one of the pulmonary veins which inflated the left auricle and ventricle, passed into the systemic aorta, and the subclavian trunks which leave the super cardiac sacs, (each of the large arteries are dilated immediately on leaving the heart, and are so united as to appear externally as a single sac.)

The circulation in the animals is briefly as follow: 1st, The blood passes from the right auricle into the ventricle of the same side, and in this cavity there are four openings, 1. One leading from the auricle; 2. One into the pulmonary artery; 3. One into the splanchic aorta, carrying black blood to the viscera; and 4. One into the systemic aorta, by the valvular communication of the circulation when that through the lungs is impeded by expiration. During expiration there is still some pulmonic circulation, a small quantity of blood passing from the lungs through the left auricle to the ventricle of the same side, from whence it has a direct passage into the systemic aorta; the valve at its base will not even permit air to pass into the right side of the heart, nor will the semilunar valves of the aorta permit regurgitation, so that the only mixture of black and red blood takes place in the systemic aorta during expiration or collapse of the lungs. The systemic and splanchic aorta do not unite until after the viscera have been supplied with blood by the latter. In the land lizards there is no necessity for that complicated structure which exist in the crocodiles, and the ventricles communicate freely with each other.—(Harlan. Jour. Acad. N. S. Phil.)

7. *On the Float of Janthina.* By Reynell Coates, M.D.

Some have asserted that the animal of *Janthina* is capable of absorbing the air of the vesicles and refilling them at will, in order to sink or rise in the water, but Cuvier could not discover any communication between the animal and the float, or any cavity within the animal which could contain the air when absorbed; he, therefore, was inclined to consider it as a rudimentary operculum.

During a voyage to the East Indies, I had several opportunities of observing the mode in which this organ is constructed by the animal.

Individuals being placed in a tumbler of brine, and a portio of the float being removed by scissors, the animal very soon commenced supplying the deficiency in the following manner; the foot was advanced upon the remaining vesicles, until about two-thirds of the number rose above the surface of the water. It was then expanded to the uttermost, and thrown back upon the water, like the foot of a lymneous when commencing to swim; in the next place it was contracted at the edge, and formed into the shape of a hood, enclosing a globule of air, which was slowly applied to the extremity of the float. A vibratory movement could now be perceived throughout the foot, and when it was thrown back again to renew the process, the globule was found inclosed in its newly constructed envelope.

It does not appear that the janthinæ ever sink below the surface, while they remain attached to the vesicle; but when they are entirely separated, they immediately fall to the bottom of the tumbler, and are unable afterwards to rise from their position, and though they continue to be vigorous for some time, they generally die in a few days. As their respiratory organs are calculated for the water, this circumstance is probably accidental.

Along the surface of the float passes a little line of pearly fibres, and upon this line are attached the egg of the animal. In *I. fragilis* the float is convex, slightly scaled above, and concave beneath, strait and composed of large vesicles. In the I. globosa it is composed of smaller, it is flat above and beneath, and by the re-union of one of the edges, it is formed into a spiral and nearly circular disk.

The float appears to be constructed for the purpose of supporting its shell and its young upon the surface of the water, and is secreted by the foot, and has no attachment to the animal except by the close cohesion resulting from the nice adaptation of their proximate surfaces.

8. *On the Vertebræ of Reptiles and Amphibia*, By R. Harlan.

Cuvier, in his Ossemens les Fossil, remarks, "the dorsal vertebra of the Maestricht animal have their transverse apophyses short, and terminated by an articulating surface enlarged vertically, which carries the ribs, which is consequently attached by a single head; this characterizes the monitors, and most of the Saurians, excepting only the crocodile, in which particular this structure is absent, with the exception of the three last ribs."

To the Crocodiles, as an exception, Cuvier should have added the Ichthyosaurus, Iguana, and Camelion, amongst the Saurians, together with the Crotalus and Coluber amongst the Ophidia, in all which the ribs are articulated with the bodies of the vertebræ by two tubercles, but do not unite with the transverse process as in the Crocodile.

Conceiving it highly important to the science of Osteology, to ascertain correctly the manner in which the ribs of the different genera of the Saurian Family are articulated; as far as my examinations have extended (with the exception of those genera above noticed in which the ribs are articulated to the body) the transverse processes (or a tubercle which supplies their place) receives the head of the ribs as in the following genera: viz. the Plesiosaurus, Maestricht Animal, Calotes, Monitor, Ameiva, Scincus Geckos, Agama, Anolis, also the Sirena, the Triton, and the Salamandra, amongst the Batrachia.—(Harlan Jour. Acad. N. S. Phil. iv. 235.)

9. *On the enormous Sumatra Ape.*

An account has been lately published of the caption of a specimen of monkey at Taruman, in Sumatra, which is said by some to be six, and others seven feet high. It is, perhaps, the full-grown ouran-outang.—(Edin. Jour. Scien. 1825.)

10. *On some newly described British Shells.*

Mr. W. Macgillwray has described in the Edinburgh Journal of Science what he considers a new species of *Pecten* found on the island of Seappay and the east coast of Harris. He describes it as follows:

P. niveus, snowy pecten. Shell orbicular, thin, diaphanous. Snow white, ribs 46, rather compressed, rounded, and scaled with short thin spines. (Edin. Phil. Jour.) t. 3, f. 1. Length 3 inches, breadth $2\frac{1}{12}$ length of the auricular margin 1 inch. Mr. Macgillwray only compares it with *P. varius*, perhaps not aware that *Pecten Islandicus, Lam.* of which this shell appears to be only a variety, has long been known as a British species.

Mr. Lowe, in the Zoological Journal, has described two shells which appear to be new to this country.

Turbo carneus, t. 5, f. 12 β. Shell subconical, umbilicated girth with regular rather distant elevated striæ; the spire short, with the apex elevated acute.

This is allied to *Helix margarita* of Montague, which Mr. Lowe also makes a *Turbo*; it is the *Margarita striata* of Dr. Leach, in the Appendix to Capt. Ross's Voyage.

Chiton Aselloides, t. 5, f. 5. Shell carinated, the valves slightly beaked, minutely, but regularly granulated over the whole surface not at all in a beaded manner; margin coarsely granulated, the granulations raised, black, dark. Chocolate brown or black; the ridges, edges, and interstices of the valves yellowish white. Fringe very short and indistinct; length rather less than one-half, breadth one-fourth of an inch; inhabits Oban, Appin.

Mr. Lowe also describes a species of *Terebratula* under the name of *T. costata*, as new both to science and Great Britain; but it appears to be the *T. aurita*, figured and described by Dr. Fleming in his Philosophy of Zoology, as found in Scotland.

Mr. Bell, in the same journal, has described what he considers a new species of Emarginula, which he discovered in Poole Harbour, under the name of

Emarginula rosea, t. 4, f. 1. Shell ovate, cancellate; inside rose coloured, vertex acute, much recurved or nearly involute.

This shell is very common on the English coast, and appears to be only a variety of the *E. conica.* It has been figured by *Martini*, i. t. 11, f. 109, 110, and by several other conchologists.—J. E. G.

11. *On the East Indian Unicorn.*

Whether the animal called by the *Bhoteas* was, as they asserted, the unicorn or not, the horns which they produced proved that they spoke of no imaginary creature, and warranted every exertion to discover the animal to which they belonged. Interest was, therefore, made with the local authorities to assist in the search, and inducements held out to travellers to procure the animal. Accordingly a few days since the skin of the *Chirsu* was sent to the resident, with the *horns* attached, proving the animal to be no unicorn, but a noble antelope, of a species apparently new. There was no possibility of procuring it alive, as it frequents the most inaccessible parts of the snowy mountains, and is exceedingly vigilant, and easily alarmed. It is found in the haunts of the Musk Deer, and sometimes associates with them.

It is added that though the animal produced is *bicornate*, yet that some of the species are unicorns—a rather odd assertion, which, however, is stoutly maintained. Every one, therefore, will, from the production of the present animal, augur every thing or nothing for the existence of the unicorn, according to his particular fancy. This only seems necessary, that the name *Chirsu*, and the horns (abundance of which has been furnished), should for the present be given to the bicornate animal, and the ultimate right of participating in either, due to the unicorn, left to the decision of time. It is much to be regretted that the skin was sent folded up in such a manner, and suffered to stiffen in that state, that the figure of the animal to which it belonged can hardly be conjectured; nay, the probable size even will be obtained, if at all, by painful admeasurement of such parts as are not shrivelled, and a comparison by analogy must fill up the due dimensions of those parts that are so. The animal is an antelope, not a deer. It is a male; his colour slaty or bluish grey, inclining to red, especially on the back; his hair, which is about an inch long, and exceedingly thick, has a good deal of that quill-like hollow appearance and feel that characterized the Musk Deer's hair, but it is softer and shorter than that animal's. It resembles as nearly as possible the hair of the *Nowahs*, or wild sheep of Bhote in colour, texture, and feel, and like the Nowahs conceals a spare fleece of very soft wool lying close to the animal's skin. The forehead is nearly black, and so are the legs; the belly white, and snout nearly so; the snout in size and shape deer like; the horns are placed very near each other entirely on the back of the head, and with that side uppermost on which the annular marks are largest.

The most remarkable feature in the animal's figure is the excessive length of the neck, which is almost half of the whole body.

The dimensions, so far as they can be taken from so shrivelled a skin, are as follows:

The skin itself will probably be sent down by and bye, from which a more accurate description can be made; but to guard against contingencies the present one may suffice.

Total length five feet eight inches; length of body four feet two inches; circuit of body, very faulty, shrivelled, two feet three inches; length of body between the legs and beneath one foot eleven inches; above, from hip to shoulder blades, two feet $3\frac{1}{4}$ inches; the neck, from back of head to shoulder bone, one foot nine inches; height of fore legs, the body being shrivelled, only one foot eight inches; of hind leg only, faulty, one foot eight inches; length of head ten inches; circuit of head one foot eight and a half inches; length of horns two feet one and a half inch; length of ears four inches and a half; length of tail eight inches.

Such are the dimensions according to careful measurement: the principal deficiency is in the bulk of the body, its depth, and circumference, neither of which can be obtained in the present skin. Admitting the Chirsu, however, to be an antelope, the general notion we have of that animal's figure, taken in connexion with the proportions above given, will enable an adept in the comparative anatomy of animals to deduce probably the entire size of the Chirsu with tolerable accuracy.

This is the rather to be attempted, because it is very unlikely that we shall soon obtain a living subject, and as long as the skins only are brought, there seems little chance of one more perfect than the present ever reaching *Atmandra*.—(Calcutta Orient. Mag.)

12. *On the Chinese Manner of forming Artificial Pearls.* By J. E. Gray.

In a former number of this Journal, I gave an account of the manner in which pearls might be formed artificially, of any size or form that is required. In a late visit to the College of Surgeons, I observed some pearls in the same species of shell, (*Barbala Plicata*,) which had the external appearance of being formed artificially, which Mr. Clift, the excellent conservator of this establishment, very kindly allowed me to examine and describe.

These pearls are of a very fine water, and nearly orbicular; their base is supported by a small process which separates at the end into two short diverging processes, which stand off at right angles to the central rib; on more minute examination it appeared that these pearls were produced by there being introduced between the mantle of the animal (while yet alive) and the shell, a small piece of silver wire, bent into a peculiar form, that is to say, so as to form a right angle, with one arm ending in two diverging processes, so as to make the simple end always keep its erect position. These wires must be introduced in the same manner as the semi-orbicular pieces of mother of pearl in the other method of forming artificial pearls, as there is no appearance of any external injury. The pearls are solid and nearly orbicular, with a small pedicell, which is continued so as to entirely cover the wire. They may be perforated and used so as to show their whole surface, which I did not expect could ever be the case with any artificial pearls; but they must doubtless, unlike the artificial pearls formed by the other means, be a considerable time in coming to any useful and valuable size.

Miscellaneous.

13. *Greenwich Observations.*

The following extract of a letter, addressed by Professor Bessel to Professor Schumacher, appeared in the last No. of the Journal of Science.—"When I had the pleasure of being your guest at Altona, you showed me the numbers of the Philosophical Magazine, which contain a very severe censure of the Greenwich Observations for 1821. I saw this censure with some surprise, because I had always considered the collection of observations at Greenwich as singularly valuable, and as a rich source of astronomical truths; nor were you, I believe, of a different opinion, and we were perfectly agreed respecting the unimportance of the inaccuracies that were imputed to this work in the two papers published in the 64th volume of the Philosophical Magazine. For those who are acquainted with the Greenwich observations, and who compare them with the critic's remarks, every further explanation would be superfluous, but since it may be supposed that these remarks will fall into the hands of many persons not well versed in astronomy, I readily comply with the request which you made, that I would commit to writing our common view of the subject. I feel, as well as yourself, the propriety of doing my best on the occasion, in order that too great importance may not be attached to this censure of an establishment, to which astronomy is indebted for a great proportion of its advancement; and that its importance cannot be very great, is sufficiently shown by the facility with which Mr. Olufsen has computed the declinations of the fundamental stars, as published in Nachrichten, No. 73, from the Greenwich observations for 1822.

The greater number of the errors which have been pointed out by the censor, are merely accidental errors of the pen. Errors of this kind are certainly disagreeable, and it would be better if they could be entirely avoided; but since all collections of observations in existence do contain such errors, they clearly appear to be unavoidable. The first class of errors mentioned in the Philosophical Magazine contains the cases in which the mean deduced from the readings of the two microscopes A and B differs from the column in which that mean is assigned. Since there must be some manifest oversight in all these cases, it may sometimes be difficult to determine whether it is in the readings or in the mean assigned, but it will, in general, be easy to distinguish, fiom the preceding or following observations of the same star, where the error lies.

The *second* class contains the differences between different records of the same observation. These must be errors in the copies sent to the press, and not in the readings of the microscopes; and they may generally be corrected by a comparison of the two passages: they sometimes extend to whole degrees, or to the tens of the minutes, and are then of no importance; for example, in the observations of Procyon, the 23d Feb. 1821, and of β Cephei, the 8th Dec. where there are errors of 30° and 5° respectively.

The *sixth* class of errors contains the intervals between the micrometer wires, as they are deduced from different observations of the same star. These are often dependent on errors of the pen, as in the observation of Capella on the 7th February, and in that of Sirius on the 8th, where there are errors of 5″ and of 4″ respectively in the fourth wire; frequently also they arise from inaccuracies of observation. In the former case they are of no consequence whatever, being easily detected at first sight; in the latter they are fundamental imperfections; but such imperfections are inseparable from the nature of observations, and it would be ridiculous to expect from an astronomer that he should perform impossibilities. *All* registers of observations exhibit inaccuracies of this kind, and if any should be produced without them, it might with confidence be asserted to be a forgery. The diligence of the astronomer is proved, not by the perfect agreement in his tenths of seconds, but by the magnitude of his mean or probable error; and it would probably be difficult for the critic to prove that this error is much greater in the Greenwich observations, than the nature of the instruments renders unavoidable.

The errors of the *fifth* class, which comprehends the differences between the polar distances observed with two and with six microscopes, seem to me to have been introduced without the least propriety: they are either insignificant errors of the pen, as in the case γ Draconis, 28th March, or slight accidental errors of observation, mixed with the changes of place of the stars and of the refraction, or, lastly, changes of the place of the pole on the instrument. For this last the observer can by no means be responsible. Had the critic pointed out any new method of fixing the instrument so that it should be subject to no alterations, he would have deserved the thanks of all practical astronomers; but the constant result of past experience shows that the greatest possible care, in procuring a firm foundation for the pillars, affords us only a comparative and not an absolute stability. The fixing of the instruments at Greenwich has been such as to keep them for a long time admirably firm; but at other times it has not been so successful, as may be seen in the table of the place of the pole, printed in the Nachrichten, No. 73; the differences between the latter days of July, and the beginning of August, 1821, depending on a change of this kind, so that they cannot be considered as accidental errors of observation, nor are they of material importance, as they may be readily determined by a series of observations of the pole star, so complete as those which are made at Greenwich. The accidental irregularities of the polar distances, which remain after the correction of the place of the pole, can be as little considered as an imputation on the accuracy of the observer, as those of the intervals of the micrometer wires. The truth of this remark is illustrated in the Nachrichten, No. 73.

The *fourth* class contains the differences between the times of transits observed with the transit telescope, and the mural circle. The latter instrument, however, not being intended for the observation of transits, nor being ever actually so employed, it would have been of no manner of use to seek for greater accuracy in the memorandums which are made merely with a view of determining its place with respect to the meridian. We ought to acknowledge the occasional insertion of these memorandums with gratitude, as they assure us that the instrument never deviates so much from the meridian as to affect the polar distances; but they are not intended for any other purpose. Neither Bradley nor Maskelyne have ever noted the times of the transits by their mural quadrant, although it was more liable to variation than the mural circle. But to correct the place of the axis of this circle continually, so as to bring it perfectly into the plane of the meridian, would certainly be of no advantage to the Greenwich observations.

Other errors which are criticised, for example, those of the names of the stars, of the hour or minute of their transits, and so forth, are of no material importance whatever; and how difficult it is to avoid errors of this kind, may be inferred from the circumstance of my having found about 1400 such errors in Bradley's observations.

The remark that the observations at Greenwich are commonly concluded at midnight, would be of some weight, if it could be proved that any thing essential is omitted by this practice, which does not appear to me to be the case. The observations relate chiefly to the sun, the fundamental stars, the moon, and the oppositions of the planets; and it may easily be discovered that these different series are exhibited with an uncommon degree of perfection. Had the censor in the *Philosophical Magazine* pointed out any other series of observations which could have been combined with these, so as not to interfere with them, no doubt the Astronomer Royal would have been much obliged to him. Every thing cannot be done at once in an observatory; and if as much is affected as can be wished in one respect, something must be omitted in others. But to multiply observations, without any plan or object whatever, would be mere idleness. *Whoever is dissatisfied with the actual riches of the Greenwich observations, would do well to make the attempt to excel them; he would convince himself by such an experiment that the labour and patience required for doing so much, are fully sufficient to exhaust the powers of any one man.*

The *third* class of errors, relating to the meteorological instruments, I have not yet mentioned, because I think myself that greater accuracy is required in this department than it has hitherto been usual to observe. And if I should be allowed to suggest any improvement that could be made in the observations at Greenwich, it would be a more correct account of the meteorological instruments, and of the place in which the exterior thermometer is fixed."

14. *On the Zetland Islands.*

An accurate chart of the Zetland Islands has long been a desideratum in British hydrography. Authorized surveys of them have, it is true, been made; but of these some are almost obsolete; and all are more or less partial or defective: and to errors of this nature, perhaps as much as to any other cause, are to be ascribed many of the disastrous shipwrecks of which that remote country has too often been the melancholy scene.

It is not a little surprising that while the most extended, expensive, and minute surveys have been executed by order of the English government of many distant regions of the globe, the nautical geography of the northern extremity of the British Islands should have been so long suffered to remain in obscurity. Charts are to maritime, what roads are to inland commerce: and we duly appreciate the laudable and fostering care which our statesmen have evinced to facilitate its extension and stability.

The Zetland Islands have too long been the bugbear, the Scylla and Charybdis of northern mariners; hence commerce has been repelled from them; and one grand source of their improvement and prosperity injudiciously obstructed. Besides, they might afford a secure refuge and resting-place, not only to vessels trading in the North Sea, but also to others forced by boisterous weather, and unavoidable accidents, into their latitude. And when, superadded to these circumstances, are considered the barbarous and iron-bound nature of the coast, and the dangerous rapidity and variety of the currents, it cannot but be highly gratifying to learn that this important chasm in our maritime knowledge is in progress of being filled up.

For this purpose the Admiralty, in the month of May, this year, sent to Zetland their surveyor, Mr. Thomas, an officer whose ability, experience, and indefatigable zeal are so conspicuous; and who has more particularly displayed his dexterity and talent in his surveys of the two metropolitan rivers of England and Scotland, and their adjacent coasts; and we trust that no delay or impediment will now occur to a work so very desirable, and which will reflect so much honour on the enlightened liberality and humanity of our Admiralty, and on the skill and activity of its surveyor.

The coast of Zetland is everywhere bold and prominent, and intersected with numerous and excellent harbours, of which the headlands are the sublime and natural beacons; and there are few situations in which the seaman can be placed where the confident guidance of an accurate chart might be of such paramount utility, and few where the want of it might be so perilous and fatal. Such a chart of Zetland would be a permanent one; unlike in this respect to many, regarding other parts of Great Britain, which require to be frequently modified to suit the changes produced by the action of the waves in the formation and dissolution of sand-banks. And where, even the best charts can be too often of little other use, from the scarcity of harbours, than to present more distinctly to the unfortunate mariner the locality of his inevitable and impending shipwreck. Y.

15. *On the Thermometrical State of the Terrestrial Globe.*

M. Arrago, in an article in the "Annales de Physique," discusses the question of the temperature of the globe at its surface, and arrives at this conclusion, that in Europe in general, and in particular in France, the winters, some centuries back, have been as cold as at present. He grounds his opinion upon the fact of the freezing of the rivers and seas at a great number of periods even of very remote date. The author then gives a table of the extreme temperatures observed at Paris, from which there results that, in the second half of the last century, the greatest cold (23·5° cent.) took place in the 25th January, 1795, and the greatest heat (38·4°) on the 8th July, 1793. He then gives the temperatures observed during the expeditions of Captains Parry and Franklin, and the dates of the natural congelation of mercury, together with the tables of the maximum temperatures observed on land, the maximum temperatures of the atmosphere observed on the open sea at a distance from the continents, and of the maximum temperature of the sea at its surface. From these observations together M. Arrago draws the following conclusions: 1st, In no part of the earth on land, and in no season, will a thermometer, raised from 2 to 3 metres above the ground, and protected from all reverberation, attain the 46th centigrade degree; 2dly, In the open sea, the temperature of the air, whatever be the place or season, never attains the 31st centigrade degree; 3dly, The greatest degree of cold which has ever been observed upon our globe, with a thermometer suspended in the air, is 50 centigrade degrees below zero; 4thly, The temperature of the water of the sea, in no latitude, and in no season, rises above 30 centigrade degrees.—(Ann. de Phys. et de Chim.)

16. *Light of Haloes.*

M. Arrago, from observations made on the 11th April, 1825, with the instrument which he has invented for the examination of polarized light, has discovered that the light of haloes (luminous circles which sometimes appear round the sun, and whose apparent diameters are 22½° and 45°), is not a reflected, but a refracted light; a result which gives much probability to the explanation of the phenomenon proposed by Mariotte. This philosopher supposed that the solar ray is refracted in its passage through the drops of water frozen and suspended in the atmosphere. M. Arrago is of opinion, that the observation of haloes might lead to the discovery of the true law of the decrease of temperature in proportion as we rise from the earth's surface, a law which hitherto has had no other foundation than a single aerostatic ascension of Gay Lussac.—(Bullet. Univ. May, 1825.)

17. *On Aerolites.*

Mr. Rose of Berlin has succeeded in separating, from a large specimen of the aerolite of Javenas, well marked crystals of augite, of the figure 109 of Haüy's Mineralogy. The same specimen appeared also to contain crystals of felspar with soda, that is, of albite. He also finds, that the olivine of the Pallas meteoric iron is perfectly crystallized, and that the trachytes of the Andes, like the aerolite of Javenas, is mixed with augite and albite.—(Edin. Phil. Journal.)

18. *On Evaporation.*

Pouillet, from a series of experiments he made, on the evaporation of liquids, infers : 1. That, during the evaporation of perfectly pure water, no electricity is evolved. 2. That when water contains certain alkalies in solution, electricity is evolved, which is vitreous for the apparatus, when the alkali is fixed, and resinous when the alkali is volatile, as ammonia.—(Edin. Phil. Journal.)

19. *Amsterdam Canal.*

It may be said, with justice, that Great Britain has outstripped all the other countries of Europe in what regards the undertaking and execution of public works, in which utility and grandeur of conception go together. We had been accustomed to consider as unique in its kind, both with respect to its extent and its other dimensions, our Caledonian Canal, which can carry a large frigate from the North Sea to the west coast of Scotland; but the new Amsterdam canal, which establishes a direct communication between the ocean and this important place of commerce, surpasses in depth and breadth every thing of the same nature existing in Great Britain. It appears that a frigate of 44 guns has already passed along its whole extent, and it is even capable of receiving vessels of 80 guns. The projected Portsmouth canal, which is intended to receive vessels of the line, would rival that of Amsterdam as to depth and width, and surpass it in length, in proportion of a hundred to fifty miles.—(Edin. Phil. Journal.)

20. *Sea Horse killed in Orkney.*

An extract of a letter from Robert Scarth, Esq. of Kirkwall, is given in the last number of the Edinburgh Philosophical Journal, describing the capture of a walrus of very large size, which after having been first seen in the opening of the Pentland Frith, was again discovered lying on the rocks of the island of Eday by one of the shepherds of the proprietor, who had the good fortune to wound it severely by a shot in the body, and having followed it to sea, with some companions, in a boat, succeeded in ultimately making prize of it, and towing it ashore. In the adventure, one of the party had nearly paid dear for his expedition, for having seized the walrus by its hind leg, the animal pulled him out of the boat, and dragged him to the bottom, and it was with difficulty his life was saved. This is the first instance, Mr. Scarth says, of any of these formidable inhabitants of the polar regions having been met with on our coasts. The hide, though dried and a good deal shrunk up, measured 15 feet in length and 13 in breadth, and was rather more than 1 inch thick. The skin is in the Royal Museum of the University of Edinburgh.—(Edin. Phil. Journ.)

Article XIV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

A new Edition of Dr. Henry's Elements of Chemistry, 2 vols. 8vo. will be ready in a few days.

An Historical View of the Hindoo Astronomy, from the earliest Dawn of that Science in India down to the present Time. By John Bentley, Mem. Asiat. Soc.

Loudon's Encyclopædia of Agriculture.

Researches in Pathology, Part I. containing an Inquiry into the Nature and Treatment of Dropsies. By Dr. Ayre.

A Treatise on Clock and Watch Making, Theoretical and Practical. By Thomas Reid, Author of the Article 'Horology,' in the Edinburgh Encyclopædia. Royal 8vo. illustrated with numerous Plates.

JUST PUBLISHED.

The Practical Miner's Guide; with a Treatise on the Art and Practice of assaying Silver, Copper, Lead, and Tin, &c. By J. Rudge. Royal 8vo. 1*l.* 10*s.*

A Treatise on the Ligaments; intended as an Appendix to Sir A. Cooper's Work on Dislocations, and Fractures of the Joints. By Bransby B. Cooper. Royal 4to. Plates. 1*l.* 1*s.*

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The English Flora. By Sir James E. Smith, Pres. Lin. Soc. &c. Vol. 3. 12*s.*

A Short Inquiry into the Capillary Circulation of the Blood; with a Comparative View of the more intimate Nature of Inflammation. By James Black, MD. 8vo. 6*s.*

Mathematics for Practical Men, being a Common-place Book of Principles, Theorems, Rules, and Tables in various Departments of Pure and Mixed Mathematics, with their most useful Applications, &c. By Olinthus Gregory, LL.D. &c. 8vo. Illustrated with Plates, and 230 Wood-cuts. 14*s.*

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Medico-Chirurgical Transactions, Vol. 13, Part I. 8vo. Plates. 12*s.*

A Manual of the Elements of Natural History. By J. F. Blumenbach, Prof. Univ. of Gottingen. Translated from the Tenth German Edition, by R. T. Gore, MRCS. Lond. 8vo. 14*s.*

Article XV.

NEW PATENTS.

W. Duesbury, Boasal, Derbyshire, colour manufacturer, for a mode of preparing or manufacturing of a white, from the impure native sulphate of barytes.—Sept. 29.

J. Martineau, the younger, City-road, engineer, and H. W. Smith, Lawrence Pountney-place, for improvements in the manufacture of steel.—Oct. 6.

Sir G. Cayley, Brompton, Yorkshire, Bart. for a new locomotive apparatus.—Oct. 6.

J. S. Broadwood, Great Pultney-street, piano-forte maker, for improvements in square piano-fortes.—Oct. 6.

T. Howard, New Broad-street, merchant, for a vapour engine.—Oct. 13.

N. Kimball, New York, merchant, for a process of converting iron into steel.—Oct. 13.

B. Saunders, Bromsgrove, Worcestershire, button manufacturer, for improvements in constructing or making of buttons.—Oct. 13.

T. Dwyer, Lower Ridge-street, Dublin, silk manufacturer, for improvements in the manufacture of buttons.—Oct. 13.

J. C. Daniell, Stoke, Wilts, clothier, for improvements in machinery applicable to the weaving of woollen cloth.—Oct. 13.

J. Easton, Braford, Somersetshire, for improvements in locomotive or steam carriages; and also in the manner of constructing the roads or ways for the same to travel over.—Oct. 13.

W. Hirst, J. Wood, and J. Rogerson, Leeds, for improvements in machinery for raising and dressing of cloth.—Oct. 21.

R. S. Pemberton, and J. Morgan, Lanelly, Carmarthenshire, for a consolidated or combined drawing and forcing pump.—Oct. 21.

G. Gurney, Argyle-street, Middlesex, surgeon, for improvements in the apparatus for raising or generating steam.—Oct. 21.

L. W. Wright, Princes-street, Lambeth, Surrey, engineer, for improvements in the construction of steam-engines.—Oct. 21.

H. C. Jennings, Devonshire-street, Middlesex, practical chemist, for improvements in the process of refining sugar.—Oct. 22.

ARTICLE XVI.

METEOROLOGICAL TABLE.

1825.	Wind.	Barometer. Max.	Min.	Thermometer. Max.	Min.	Evap.	Rain.
9th Mon.							
Sept. 1	N E	30·27	30·18	84	53	—	
2	N E	30·31	30·27	83	52	—	
3	S W	30·31	30·17	78	52	—	
4	N W	30·18	30·17	71	50	—	
5	N W	30·18	30·13	65	43	—	
6	N W	30·13	29·94	71	46	—	
7	N W	29·94	29·80	81	55	—	
8	N W	29·86	29·80	82	50	·87	
9	S W	29·86	29·82	81	55	—	
10	S W	29·82	29·73	81	60	—	19
11	S W	29·88	29·73	76	56	—	18
12	S	29·92	29·88	77	55	—	
13	S E	29·88	29·65	74	53	—	23
14	N E	29·70	29·65	73	60	—	32
15	N W	29·97	29·70	70	50	—	
16	S W	30·00	29·96	74	62	—	
17	S E	29·96	29·95	78	58	·90	36
18	S W	29·95	29·92	72	64	—	03
19	S W	29·92	29·92	72	56	—	
20	S W	29·94	29·75	76	60	—	·06
21	S W	29·81	29·75	69	57	—	13
22	N W	30·18	29·81	66	44	—	
23	N W	30·18	30·16	66	45	—	
24	S W	30·19	30·14	70	63	—	
25	S W	30·19	30·08	73	59	—	64
26	S W	30·28	30·08	71	48	—	
27	N W	30·48	30·28	73	41	·94	
28	N W	30·48	30·41	66	41	—	
29	E	30·41	30·16	60	43	—	
30	E	30·16	30·02	57	52	·30	39
		30·48	29·65	84	41	3·01	2·53

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Ninth Month.—1—9. Fine. 10. Day fine: rainy night. 11, 12. Fine. 13. Cloudy: several vivid flashes of lightning about eleven, a. m. followed by a long peal of thunder, and a very heavy shower of rain. 14. Rainy. 15, 16. Cloudy. 17. Showery. 18—20. Cloudy. 21. Showery. 22, 23. Fine. 24. Cloudy. 25. Cloudy night: rainy. 26. Cloudy. 27. Fine: a *stratus* in the marshes at night. 28—30. Fine.

RESULTS.

Winds: NE, 3; E, 2; SE, 2; S, 1; SW, 12; NW, 10.

Barometer: Mean height

For the month.................................... 30·022 inches.

Thermometer: Mean height

For the month.................................... 62·883°

Evaporation.................................... 3·01 in.

Rain.................................... 2·53

Laboratory, Stratford, Tenth Month, 22, 1825. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

DECEMBER, 1825.

Article I.

On the Means of ascertaining the comparative Tanning Powers of Astringents. By Mr. Edward Bell Stephens, Chemical Assistant to the Royal Dublin Society.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Oct.* 17, 1825.

Of all the manufactures which depend on chemistry for explanation and improvement, that of Leather, though highly favoured by the attention of scientific men, is still, perhaps, *most* in need of their aid.

Notwithstanding Seguin's happy discovery of the chemical affinity between tan and gelatine, which promised to introduce something like *analytic* certainty into his art, the practical tanner is yet unable to estimate the goodness of any bark (previous to its actual use,) otherwise than by its external characters. He depends wholly on the colour, taste, and the *healthy* brittleness which in many cases requires an experienced eye to distinguish it from the brittleness produced by *decay*. By the mere appearance he may indeed discriminate between sound and unsound bark of the *same* species; but when both are fresh and healthy, or of *different* kinds (for instance, valonia and cork tree bark), his eye and tongue no longer assist him in determining the proportional worth of either.

Any method therefore which would enable the tanner to ascertain with speed and certainty the comparative value of astringents (of which the market always affords a striking variety) by the examination of samples, *previous* to purchase, would be a great step towards rendering his business *safe*, consistent, and regularly profitable;* and would, no doubt, be

* A friend assures me that valonia (which is now much in demand amongst tanners at 28*l.* a ton) was offered to them from Italy, 30 years ago, in any quantity, at 4*l.* a ton, in vain: they had no means of ascertaining its value experimentally.

the means of introducing general improvements into every branch of the manufacture.

To arrive at this is the object of the present essay. However, as several chemists of unquestioned talent and extensive knowledge have preceded me in this inquiry, and as a process to effect this particular object has already been proposed by high authority, it may be proper to state the circumstances which rendered a rejection of the mode so recommended, a matter of expediency, indeed of necessity.

In the year 1803, Sir H. Davy published an essay in the Phil. Trans. "On Vegetable Astringents;" and another in the Journals of the Royal Institution, "On the Process of Tanning," which were both of high importance to the practical tanner, as affording him a clear and masterly explanation of the varieties of chemical action that take place in this interesting manufacture. These valuable essays peculiarly exemplify the happy tact by which the talented author can so well illustrate, by *practical* application, the importance of his *scientific* researches.

In this excellent spirit of useful illustration, Sir H. Davy proposes the following process (vide Jour. Roy. Inst. 1803) for the attainment of this wished-for mercantile comparison.

"The solution of gelatine, most proper for the general purpose of experiments, is made by dissolving an ounce of glue or of isinglass in three pints of boiling water.

"The substance to be examined as to its tanning power may be used in the quantity of two ounces. It should be in a state of coarse powder, or of small fragments. A quart of boiling water will be sufficient to dissolve its astringent principles.

"The solution of glue, or gelatine, must be poured into the astringent infusion, till the effect of precipitation is at an end.

"The turbid liquors must then be passed through a piece of blotting paper, which has been before weighed.

"When the precipitate has been collected, and the paper dried, the increase of its weight is determined, and about two-fifths of this increase of weight may be taken as the quantity of tannin in the ounce of the substance examined."

If no well-grounded objections had been discovered to this apparently simple process, it would have ensured greater advantages to the leather manufacture than any previously obtained through the medium of scientific investigation; but Sir H. Davy has, with his usual candour, stated several *niceties* connected with its management (Phil. Trans. 1803), which, to ensure accuracy, require particular attention; and therefore tending (in the hands of any but a practised experimentalist) to render the process *very fallacious.*

From my own experience, I can state that the idea of this nicety of manipulation, requisite by the author's own showing, has been quite sufficient to deter every person in the tanning business in Dublin from entering into such an analysis; but as the object proposed is truly important, and as the scientific world yet appears to be of opinion that the process should, with proper attention, lead to *correct* results, it may be well to recapitulate the sources of error, which, in my opinion, render it totally inadmissible.

1. The *degree of concentration* of the solutions (of tan and gelatine) has a decided influence on the quantity of the precipitate formed; the *strongest* solutions giving most: so that a bad sample of bark which only *partially* saturated the quart of water employed, would, on this account, appear (by its deficient precipitate,) *worse* than it really was. This is a serious cause of inaccuracy, for it is without a remedy. Evaporation, to equalize the strength of the infusions, is here inadmissible, as boiling, or even moderate continued heat with exposure, is found to precipitate both tan and extract in an *insoluble* form.

Additions of the astringent substance under examination, to bring up the specific gravity of the weaker infusion, afford no surer means of equalizing the tanning matter in both. For the mucilage present in vegetable astringents, so far influences the specific gravities of their solutions, that their equality in this respect determines nothing to the purpose.

2. When bad samples (giving weak infusions) are tested, the precipitate is not entirely retained on the filter; but (notwithstanding repeated filtration) is partially carried through with the residual liquor, in which it remains a long time suspended, rendering it turbid and opaque.

3. The solution of gelatine must be *fresh* made preparatory to every new set of experiments; for, if it lie till tainted, its power of precipitating tan will be materially impaired.

4. The solution of gelatine must be in as high a state of saturation as is compatible with its perfect fluidity; and to ensure this latter requisite, heat must be applied to keep it at a standard temperature during the experiment.

5. Great care must be taken to prevent *excess* of gelatine in the mixed liquors, for when this excess exists, a portion of the solid compound formed is redissolved.

So far, it may be said, these are only difficulties in *practice* to the attainment of correct results; but Sir H. Davy mentions one striking fact, which is, in reality, an objection in *principle* to the institution of any comparison (*by this mode*) between astringents *not* of the same species. He says (Phil. Trans. 1803), "the tanning principle, in *different* vegetables, demands for its saturation *different* proportions of gelatine;" so that precipitates from valonia and sumach (by gelatine) of *equal* weight, might contain *unequal* quantities of tan.

Since the publication of the two essays above-mentioned, this hitherto intricate subject has been greatly elucidated by the original researches of Dr. Bostock, who, in the year 1809, was engaged in a series of experiments (the *converse* of Sir H. Davy's) in search of a vegetable astringent which might serve as a certain test to determine the quantity of gelatine in animal fluids; during which examination he found so many new *sources of error*, both in *practice and principle*, from the use of tan *as a test for the quantity of gelatine*, that he was compelled to abandon it. As I conceive that these objections equally apply to the use of gelatine as a *test for the quantity of tan*, I will here enumerate them, and thus bring into one view the mass of evidence which compels us, however unwillingly, to forego the mode of examination proposed by Sir H. Davy.

Dr. Bostock discovered that isinglass and glue (in the state in which we generally obtain them) both contain impurities: in isinglass, *the insoluble matter* sometimes amounts to $\frac{1}{20}$th of the whole; a circumstance rendering it necessary to separate this pure portion by solution, and resolidify it by evaporation. The glue is a still more uncertain article from the quantity of *water* it contains, (some pieces dried at 150° Fahr. for 24 hours, indicating so much as $10\frac{1}{2}$ per cent.) as well as from the *coagulated albumen and muriate of soda* which exist in it. Again, isinglass and glue differ remarkably in their *powers of concretion:* a solution of the former containing $\frac{1}{25}$th of solid matter would be when cold perfectly *concrete;* whilst a solution of the latter containing an equal weight would (though strongly adhesive) remain quite *fluid* when cold.

In his endeavours to procure pure tan, Dr. Bostock found that the extract of rhatany contained it in a state more free from impurities than any vegetable astringent we are acquainted with; and, therefore, with an infusion of this substance, and the purified isinglass formerly mentioned, he pursued his experiments.

In addition to the difficulties previously detailed, he found that all the precipitates of tanno-gelatine caught, as directed, on a filter, adhered so strongly to the paper that they could not afterwards be completely separated. Weighing the paper also (before and after) does not remedy this inconvenience, for the strong solutions so thoroughly pervade it, as to defeat all attempts at accuracy.

But the most striking result obtained by Dr. Bostock is, that the precipitates formed by the gradual mixture of solutions of tan and gelatine, *differ in their composition at almost every drop.* The first portion of gelatine throws down a solid curd containing 50 per cent. of tan: the ensuing additions form opaque compounds containing less and less of tan, till, at last, the gelatine has so little left to unite with, that it is unable to become a real solid, and thus the imperfect curd last formed (being nearly all gelatine) remains suspended through the fluid.

This one fact is sufficient to invalidate the whole process, and all the calculations founded on it, respecting the quantity of tan present in any solution; for they rested entirely on the presumption that tan and gelatine always combined in *one proportion only;* whereas it appears from Dr. B.'s researches, that they are capable of uniting in several: gelatine combining *chemically* with an *equal* weight of tan, if within its reach, and also influenced so strongly by a *smaller* portion, though the union here may be somewhat *mechanical,* as to leave its solution in water to unite with it. (Vide Nicholson's Journal, vol. 24, " On the Union of Tan and Jelly," and " On Vegetable Astringents.")

I am particularly anxious to draw to these masterly researches of Dr. Bostock, the attention they so well deserve, and have *yet* to receive. Hitherto it appears they have been almost unknown, or overlooked, as not containing facts so closely connected with the present subject.

In Sir H. Davy's " Agricultural Chemistry," published 1813, the process recommended in 1803 is repeated with little variation, and a table is given of the quantities of tan in various barks, estimated by the jelly test. This table is copied into the last edition of Brande's " Manual of Chemistry," without any expression of a doubt of its correctness in *principle,* and also into the last edition of Henry's " Elements of Chemistry," in which we find stated (vol. ii. p. 358): " In general, however, Dr. Bostock has been led to conclude that the compound formed by the union of jelly and tan consists on an average of somewhat less than two parts of tan to three of gelatine;" whereas Dr. B.'s last paper (abovementioned) leaves us no hope of *any* data to ground our calculations on.

In the Herculean task which an editor of a systematic work on chemistry necessarily undertakes, it is a moral impossibility that he can find time to consider the bearing which all the experimental facts, scattered through our numerous scientific journals, have on received opinions and theories. Such omissions are continually occurring in similar elementary works on other sciences, in the hands of most diligent and faithful compilers. For my own part I am so satisfied of the proper feeling entertained on such points by the gentlemen at the head of the science to which I have the honour to be attached, that having once called their attention to the matter, I will leave its adjustment entirely to them.

In endeavouring to strike out an unexceptionable process for the use of tanners, and complete this test in the spirit of *utility* in which Sir H. Davy had first conceived it, I found it necessary to take a different path from that pursued by Proust and Tromsdorff, who endeavoured by the action of reagents to deprive tan of the various matters naturally combined with it, and which essentially modify its action in every case hitherto brought under our notice. Now the test required ought to resemble in its action that which takes place in a tanner's pit; for if the mode of trial adopted differ materially in principle from the manufacturing process which it is framed to aid, any estimate of the value of astringents founded on it will be seriously in error. For instance, a tanner's profit chiefly depends on the *increase of weight* which a hide acquires during the process that converts it into leather. This in strong (sole) leather is generally one-third of the dry weight, or, what tanners are more accustomed to calculate on in Ireland, the finished leather is half the weight of the hide when fresh from the slaughter-house. The *extractive matter* forms an important part of this weight, and, therefore, any test which the manufacturer might apply to ascertain the tanning power of an astringent material, and which acted only on *pure* tan, would completely mislead him. I am inclined to think any gallic acid present is also absorbed by the skin. In spent ouze the power of striking black precipitates with solutions of iron is lost, and transferred to the leather, particularly that made with *oak* bark. In short the tanner wants something which, when presented to an astringent infusion, will seize on, and enable him to estimate every thing which would (in his process on the large scale) contribute to the weight of his leather.

I know nothing which can do this so well as the skin itself, and I find that by a little management it may be made to yield us the information we require, quicker than has hitherto been thought possible.

It cannot be doubted that a strong bull hide will continue to absorb tanning matter for two years, if the process be so arranged; but if we alter the usual proportion of the materials, the result, *as to time*, will differ exceedingly. If a fresh skin be shaven down to a very thin substance on a currier's beam, or split into fine leaves by a machine, so as to expose a great expanse of surface, and a quantity of these be steeped in a proportionally small measure of tanner's ouze, they will in a very very few hours imbibe all its useful tanning substance, and enable him to ascertain, by the difference of weight before and after steeping, the exact quantity of matter in solution, that can be made available in the manufacture of leather.*

This is a test which comes home to the business of every tanner; one which he can place confidence in, because he can clearly understand it; and though some niceties are requisite in this process also, the line of thought necessary to attain them

* The strongest ouze in a Dublin tan yard, prepared in the usual cold method, was exhausted of taste and colour by this mode in seven hours; a decoction of valonia (the strongest I was able to make) of sp. gr. 1065, was, with the aid of frequent manipulation, to change the ouze in the pores of the skin, deprived of all astringency in about nine hours.

is already so familiar to him, that I have great hopes it is calculated to become generally useful.

There can be no question of the correctness of the principle of this plan, it being that in daily operation in every tannery, yet the field is open for improvement, and the exercise of ingenuity in the *conduct* of it; but having placed the subject within the grasp of the manufacturer, I candidly confess his superior right to prescribe the details, and, therefore, look up to him for instruction in every thing connected with his handicraft operations.

As, however, I have made several experiments to ascertain the proper mode of proceeding, and acquired some experience in the matter, I willingly communicate it, and devote the remainder of this paper to hints which I hope may be of service to the tanner in going through the test on his own account.

As the object is to institute a comparison between two or more astringents, and decide quickly on their respective merits, whilst the articles are yet at market; a few pieces should be selected from each lot, so as fairly to represent every parcel. The whole of each sample should be separately ground to powder in a small coffee or pepper mill, and passed successively through the same sieve, to place each in similar circumstances. From these average samples, the operator may take equal weights, and obtain complete infusions of each by agitating them with successive portions of *warm* water till all the soluble matter is extracted.

Though *boiling* water will hasten the operation, it certainly tends to decompose the astringent liquor afterwards, and induces it to deposit a portion of insoluble matter which may interfere with correct results. Water at blood heat (98° Fahr.) may be safely applied; bottles to infuse and shake the powders in, and a piece of muslin to strain through, serve these purposes completely. Care must of course be taken to preserve and return any powdered bark which may remain in the strainer, with the next quantity of warm water. Successive additions in this manner are exceedingly more powerful solvents than the whole quantity applied at once. Their efficacy increases in a geometrical progression.

When the several infusions yielded by one sample are united, the average liquor will in general be found sufficiently weak to be acted on by skin with the greatest effect; that is, to afford all the *colouring matter* it contains along with the tan;—an advantage the tanner is prevented from obtaining in strong decoctions of bark. If his experience should lead him to think a particular infusion too strong (which may occur in the examination of astringent extracts similar to kino, rhatany, and catechu), he may add water to reduce it to what he would call a "safe tanning strength." Aliquot parts of these infusions (one-sixth of each, for instance) are now to be separately submitted to the action of test skins (to be described afterwards) which should be carefully handled in the liquors now and then for seven or eight hours to expose new surfaces to the action of the ouze, till the tanner ascertains by eye and tongue that the liquors are absolutely spent.

There are a number of critical appearances in various operations, altogether undescribable, and of which inanimate tests give us no warning, and keep no record: in such cases it fortunately happens that the organs of sense give perfect satisfaction to an experienced operator. In the process under consideration, habit renders their decision all-sufficient.

The skins intended for the trial should previously be well washed in tepid water to extract any lime which they may have absorbed in the process of depilation, together with all the loose gelatine which can be squeezed out of the pores along with it; so that nothing shall remain but the firm fibre, which will bear handling in the usual manner in weak ouze. They are, after this washing, to be dried in the shade, but not near a fire; then cut up into small pieces to fit the miniature tan-pits, and weighed in lots corresponding with the infusions; each lot containing bulk sufficient to fill up the quantity of ouze, and (like a sponge) present an absorbent surface on every side.

This *dry* skin (as every tanner knows) is in a very unfit state to absorb astringent matter and become leather. It is, therefore, previous to immersion in the ouze, to be worked with the hands for about five minutes in water just blood-warm (98° Fahr.), and induced by this treatment to soften and swell to its former dimensions, in which state it will be capable of fully exerting its absorbent powers; and if care be taken to give the ouze an *over dose* of it, the action will be completed in a few hours.

As each ouze is exhausted, its lot of skins should be taken up, dried in the shade as before, and the increase of weight in each lot separately ascertained. This additional weight can consist only of the useful tanning matter, so that the increase of each lot will directly show the true comparative value of the astringent in whose infusion it was steeped.

The skin most proper for this purpose is the strongest and freshest that can be procured, shaved down or split to the thinnest substance it can be safely reduced to. The large fresh currier's shavings from the strong hides intended for chaises or harness, can be obtained in quantity, and are well adapted to the process. The skins of ill-fed sheep and cattle that come to market hide bound from the mountain districts, as well as those of aged cattle in general, are also strong and fibrous enough for the purpose; but what I would prefer to all others (from the description I have received), are ox hides split very thin and evenly by the patent machine.

In Birmingham (I am informed) this branch of the leather manufacture is well understood. In Dublin we have but one splitting machine, and that is only constructed for splitting sheep skins. These, from the improvement which has taken place in our breed of sheep, are generally so full of fat, that they are quite unfit to act as a test in this case, the oil shielding the skin from the action of the tan, and where it exists in greatest quantity along the back and across the neck, retarding the evaporation of moisture during the two drying processes, and consequently leading to false results.

Calf skins, shaven down to the thinness of split sheep skins, are free enough from oil, but the fibre is in general so delicate, that it is liable to be injured, and partially dissolved, or rather *dispersed* through the warm water during the softening and swelling, preparatory to steeping in the astringent infusion. I found that several lots of this skin, previously dried and weighed for experiment, though beautifully transparent, and apparently perfect in every way, lost seven per cent. of loose gelatine when handled in tepid water. Thus this species of skin also appears improper for the purpose.

To avoid the last mentioned source of error, it will be prudent to reserve a piece out of every batch which undergoes the swelling process, to ascertain (by drying and weighing without tanning) whether the remaining pieces destined for experiment had lost any thing in that operation. As such a loss is only likely to occur in strong hides from carelessness in the usual operations of lining, washing, &c. the tanner has it completely in his power, by proper attention, to prepare his own test skins in the most perfect manner. Perhaps the calf skins that I operated on had been somewhat injured in these previous processes; whereas if they had been carefully treated, they might have remained strong enough. This is a point which peculiarly rests with the tanner to ascertain correctly, as a matter of economy and convenience. If calf skins be really strong enough to retain *all* their substance from one weighing to the other, tanners who manufacture upper leather will be much more at home in trials made with them. In Ireland, I believe, there is quite as much of it made as of sole leather.

In the shaving of strong hides, it is indifferent to the currier in what shape he takes off the pieces. A tanner who attends him during that operation may obtain shavings of the exact size he wants, and, therefore, need never sacrifice an entire hide to the experiment.

I need scarcely mention that the test skins employed in this trial should not be expected to become perfect leather, so as to enable the tanner to judge of the quality of the astringent also. That is an operation requiring length of time, and excess of tanning materials, both of which are here inadmissible.

In the course of experiments which led me to the adoption of the plan recommended in this essay, I have accumulated a number of comparative analyses of the several astringents used in the arts, made with a view to ascertain how the test would work in all cases, as an index to their tanning properties.

These I intended to annex to the present paper, but satisfied of the correct action of the test, I omit them for the present, convinced that each individual lot of astringent substance brought to market may differ so widely in composition and quality from every other, that such a table as I might be able to form from the examination of particular samples (not now at market) would only tend to mislead.

My chief hope is, that in the preceding sketch of a process, I have been sufficiently explicit to enable a tanner to proceed for himself towards the attainment of that important object,—a knowledge of the comparative value of all the astringent materials which appear at market, in time to regulate his purchase of any. Edward Bell Stephens.

Article II.

Observations on the Planet Venus, made during the Spring of the Year 1825. By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Great Ouseburn, Sept.* 9, 1825.

Since the time of Cassini, spots on Venus have rarely been seen. Dr. Herschel says, the planet has always presented to him a perfectly uniform surface, quite free from spots; and the only observations made since those of Cassini and Bianchini were by Short, who was fortunate enough once to see them.

During the spring of the present year, Venus was rarely entirely free from them, and, therefore, I hope, that my observations may not be the whole that have been made. The instruments I employed were an excellent Newtonian reflector of 6 inches aperture, using powers from 70 to 400; an aerial, not achromatic, of 18 feet focus, with powers of 70 to 150; an aerial of 50 feet, power 160; and I hope I shall be able to show that where the old aerial telescope has sufficient light and power, it possesses some very considerable advantages over other instruments.

Before I proceed with the immediate subject of this paper, it will be proper to convey a correct idea of the goodness of the instruments, to remove all doubts which otherwise might arise. With the reflector, with powers from 70 to 800, and occasionally 1200, I have repeatedly seen the double stars Castor, α Herculis, *α* Ursæ Minoris, *ξ* Ursæ Majoris, the quadruple star *ε* Lyræ, Rigel; together with the whole of those in Dr. Herschel's first class, which can be seen with the powers I can command. In the spring of the present year, I have shown the double ring and quintuple belt of Saturn, and once a spot upon his southern hemisphere, to several persons; it shows the most minute parts of the solar spots, as well as the mottled appearance of his surface, as perfectly as any instrument I have ever seen; and the spring of the present year afforded many opportunities of trying its powers in this respect; for example, March 3, within one umbra of small size were 11 nuclei, all distinctly defined.

The refractors consist of one convex object lens and a convex eye glass. I have often compared them with the reflector, and with other instruments, and in point of steadiness there is no comparison; on account of their great length, causes which produce great tremor in shorter instruments do not affect them; also, what is very curious is, that when the air is in such a state as to produce great undulations, when the reflector is used, it affects the others very little; so that with them I can observe objects when nearer the horizon than with other instruments. With the aerial of 18 feet, I see Saturn's ring beautifully distinct with a power of 36; with 70 some belts, and certainly a trace of the division of the ring; all seen with 50 foot, and power 160. I have not had opportunity to make many observations on double stars with these instruments; the stars are well defined through them: I have seen the double star Castor, and the trapezium in the nebula of Orion's sword, and many much closer ones. I have often compared these instruments with the reflector, in viewing the moon and the solar spots; every part of the moon, even the most intricate, is seen in high perfection; the limb beautifully defined; so also are the solar spots: indeed, except I am taking any micrometric measures, I almost always use the short aerial for viewing both the sun and moon. With it, the mottled appearance of the sun is very conspicuous, and the solar spots are blacker than with any reflector I have ever seen. The bright ridges of the sun are seen in the highest perfection by receiving the focal image of a lens of very long focus upon a white screen; the sun's image in the focus of the object lens of my long aerial is about 4½ inches in diameter: in this the ridges are seen in great perfection, together with the spots, which are absolutely free from colour. It is to be regretted that the simple astronomical refracting telescope has gone into disuse: if the proper dimensions be observed, it is as free from colour as any achromatic; it is remarkably free from tremors; the number of surfaces are small; and on account of the long focal lengths used, small imperfections in the lenses, or defects in the centering, produce but little effect. On these accounts it possesses many advantages. In light it exceeds not only a reflector of the same aperture and power, but every achromatic; indeed the quantity of light is astonishing. I have often compared two telescopes; one a triple achromatic by Dollond, fitted for terrestrial observations of $1\frac{1}{4}$ inch aperture, which is a very fine instrument; the other consists of two convex lenses; the object glass has an aperture 0·5 inch, and the same power as the former; the light is as nearly equal as possible, and in point of distinctness there is little difference. These remarks will show that no imperfections in the instruments employed in viewing Venus can have introduced any errors.

The first time I observed any appearance of spots on Venus was Feb. 21, 7^h; I used the reflector, having six inches aperture, and powers from 120 to 400. There were two narrow duskish irregular lines, which were best seen with 200. On the 22d, 7^h 30^m, the spots occupied a different part of the disc to what they did the day before, having moved according to the order of the signs. They were very faint, and ill defined. I saw no more spots until March 11, beginning a little before sunset, and continued to 9^h; the same instrument and powers as before. The air was in a very unfavourable state. The spots were very conspicuous, as in fig. 1, still they were not well defined; during the short time in which observations could be made, this evening, no conclusion could be drawn respecting the rotation of the planet. Dr. Wasse observed Venus with the same instrument, and made a drawing of the spots, which coincided with mine. The weather was so uniformly cloudy that no observations could be made before April 4: on that evening, from 8^h to 11^h, I had an imperfect view of spots; but the air was so extremely tremulous that I could not see them well defined, or perceive any change in their position. April 7, from 7^h to 9^h, I had a better view. At 7^h, the light was so strong that stars of the first magnitude were not visible. At 7^h the spots were as in fig. 2; at 8^h 30^m, they appeared to have sensibly advanced, as in fig. 3; the spots *a b* were not in view before 8^h 30^m. The horns projected considerably beyond the semicircle, which probably is owing to the planet's atmosphere. Reflector six inches aperture; powers 120 and 200.

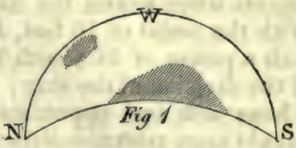


Fig 1

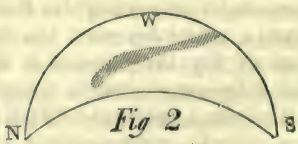


Fig 2

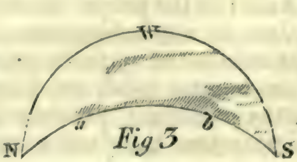


Fig 3

April 8, 8^h. The air in a good state. Venus appeared as in fig. 4. Dr. Wasse observed them with the same instrument, and made a drawing, which coincided with mine in all respects, except he did not see the two bright lines at *a*. In 1^h 30^m, Dr. W. and I both concluded that the spots had approached the W limb, and moved very little towards the N horn. The planet had the same appearance with both the aerial telescopes; indeed with that of 50 feet, I certainly saw them more evidently than with the reflector.

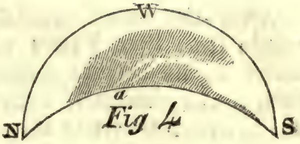


Fig 4

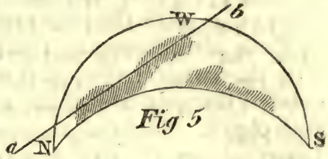


Fig 5

April 13, 4^h 30^m, nearly 2$\frac{1}{2}^h$ before sunset, I had a fine view of Venus, which is represented in fig. 5 with the reflector, powers 70 to 400; with 70 I could not see any spots; with 120 they were very distinct. Those towards the S were so considerable that whilst some fleecy clouds were passing, the whole to the S of the line *a b* disappeared, and that to the N remained visible for some minutes. The evening unfortunately proved cloudy; had it been fine, I might have obtained useful information respecting the time of rotation, as 7$\frac{1}{2}$ hours would have been allowed for observation; and I the more regret it, because this was the only time I saw Venus so early. Lieut. Hornby, RN. observed the planet with me, and confirmed my drawing of the appearances.

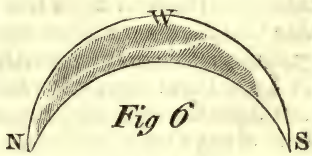
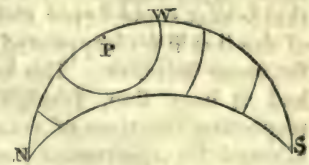


Fig 6

April 20, from 7^h to 9^h. Venus as in fig. 6; I used the reflector and both aerials, and saw the spots with all. The evening being very fine, I saw the spots better defined than usual; yet they were never so distinct as to allow the use of a micrometer with advantage. Between 7^h and 9^h, the bright ridge separating the two spots had evidently moved; the most S part had moved towards the W, and also a little to the N. I can speak more confidently of the motion than at any former time; and although they were not so well defined as to allow measures to be taken which would determine either the exact time of rotation, or the position of the axis, yet the motion seemed so sensible as to agree with the time determined by Cassini, and not to confirm that fixed by Bianchini. Beyond this I could not arrive at any conclusion; because Venus, except observed when the sun is above the horizon, is always in the worst part of the atmosphere, and of course appears tremulous; her light also is so very powerful that no telescope shows her free from radiating light, which is a great impediment.

After the most careful observations continued during the whole time that Venus was visible, I have not been able to arrive at any certain conclusion respecting her period beyond this, that a change in the place of the spots could be perceived in the space of two or three hours, when the air was in a serene state: this agrees with the period assigned by Cassini. The direction of their motion, deduced from these observations, agrees also with the position of the planet's axis, as determined by those astronomers. I noted down all the observations at the time they were made; and when the series was completed, calculated the position of the axis, as seen from the earth on the 20th April. The sun's place was ♉ 0° 5′ 18″; Venus's helioc. Longitude ♎ 11° 13′; her N pole is directed towards ♒ 20°, and elevated about 15° above the plane of the ecliptic; hence the N pole was 16° 49′ from the W limb, or $\frac{43}{1000}$ of the planet's radius; the illuminated part of the disc was 55° 49′, or $\frac{438}{1000}$ of her radius; so that the distance of the N pole from the W limb was about one-tenth of the illuminated part. In the figure, the position of the pole is shown, as the planet appeared in the telescope, inverted and reversed; the circular arcs show the apparent paths of the spots.



It is evident that the position of the planet was very unfavourable to these observations.

It is to be hoped that astronomers will in future make particular observations upon this planet. It is certainly very remarkable that for a century no spots have ever been seen, except once by Short; this may be partly ascribed to the instruments in use, for I see them best with the refractors. Several persons have seen them through my telescopes, and their drawings and descriptions always coincided with mine; therefore there could be no fallacy. I am fully persuaded that if the old aerial telescope were more generally applied, not only the spots of Venus, but other objects, might be better seen than with other instruments. I saw them better defined than with the reflector, the powers being equal; the same spots could not be seen with a good achromatic of 2¼ inches aperture, and very imperfectly by one of 3 inches, by Dollond, with higher powers than I used in the others. I examined the planet in hopes of seeing some appearance of mountains, which some observers speak of: I employed powers from 70 to 800: with the latter I see the double ring and all the belts of Saturn, and close double stars in great perfection; but in no instance could I perceive any trace of them, although I paid particular attention to the concave edge, sometimes without a screen glass; at others employing smoked glasses of every variety of shade. J. B. Emmett.

P. S. Since the above was written, I have completed a series of observations made upon some very fine solar spots, which confirm the statements made in my former paper on the same subject. July 22, 0^h 30^m, a spot which had been in view for many days disappeared. July 21, 23^h 25^m (app. time), a spot had entered which was not in view at 20^h; I suppose it entered about the 21st, 20^h, for 23^h 25^m it had just entered. This was the largest and finest spot I ever saw. On the 27th, I saw it without the aid of a telescope, and it continued visible to the naked eye until the 30th, when, near the centre of the disc, the umbra passed a vertical wire in $5^s{\cdot}5$. It was in the centre of its apparent path July 28, 1^h.

It had described half its apparent path	July 28,	1^h
It entered the disc	July 21,	20
Time of describing half its visible path. . .	6	5

therefore it must be visible 12^d 10^h.

Aug. 3, 1^h. The spot was very near the edge, so near that it could not remain visible more than eight or ten hours. The day was so unfavourable that I had only this one view of the sun, and this through rather dense clouds. Aug. 3, 21^h, there was not the least trace of it.

It disappeared	Aug. 3,	9^h
Came into view	July 21,	20
Therefore in.	12	13

the spot entirely crossed the disc.

Of the spot which disappeared on the 22d, there was no trace before Aug. 5, 12^h; I could not see any trace the day before; and on the 6th, 0^h, I saw it; it had been on the disc about 12 hours.

The spot returned	Aug. 5,	12^h
It disappeared	July 22,	0
It was invisible during	14	12

The sum of these two observations is less than the mean period of the sun; it amounts to 27^d 1^h, only, which arises from the difficulty of determining it in this manner; also one of the spots has moved.

Aug. 17, 22^h. The spot which returned on the 5th is quite off the disc; there is no trace of the feculæ near it: therefore it was not in view more than 12^d 10^h.

Aug. 19, 30^m, the spot which disappeared Aug. 3, 9^h, has returned; it may have been visible 12 hours; certainly not more.

It came into view	Aug. 18,	12^h	
Disappeared	Aug. 3,	9	
It was invisible during...........	15	3	
This spot first came into view ..	July 21,	20^h	
Returned after one revolution ..	Aug. 18,	12	
Therefore	27	16	is the time

of a revolution.

The spot which disappeared ..	July 22,	0^h
Disappeared after a revolution,	Aug. 17,	22
	26	22

Hence this spot has had a considerable motion on the sun's disc, which accords with the observations. Sheiner first took notice of this proper motion which the maculæ often have; many other astronomers have confirmed his statements: indeed there is nothing more common than to see the spots of a cluster change their relative places very considerably, even in the space of a few hours. Mr. Wollaston once saw a spot suddenly divided, and the parts fly off from each other very rapidly.

Article III.

On the Compounds of Antimony with Chlorine and Sulphur. By M. H. Rose.*

I. *Compounds of Antimony and Chlorine.*

It is known that a solid compound of antimony and chlorine, fusible at a very moderate heat, is obtained by distilling pulverised antimony with an excess of corrosive sublimate. It deliquesces by exposure to the air, and is converted into a liquid resembling an emulsion.† When treated with water it changes, without the evolution of heat, into hydrochloric acid, and a compound of the oxide and chloride of antimony. This white powder, which is precipitated by mixing the chloride with water, is wholly volatilized when heated in a little matrass by the blowpipe; consequently it contains neither antimonious nor antimonic acid. But since this chloride of antimony is converted by water into hydrochloric acid and oxide of antimony, its com-

* From the Annales de Chimie.

† The common butter of antimony of the shops, which forms a clear liquid, is not a solution of the solid chloride of antimony in a small quantity of water, but in muriatic acid; for the Pharmacopœias order it to be prepared with a larger quantity of acid than is necessary to form the solid chloride.

position much corresponds with the composition of those substances; and as oxide of antimony contains 3 atoms of oxygen, the antimony must be combined with 3 atoms of chlorine in the solid chloride, or it must contain per cent.

Antimony	54·85
Chlorine	45·15
	100·00

The analysis of chloride of antimony, however, by Dr. John Davy, gave a different result. According to him, it consists of

Antimony	60·42
Chlorine	39·58
	100·00

I, therefore, submitted it to a fresh analysis in the following manner:—I poured water on a quantity of the chloride, and then added tartaric acid till the liquid was perfectly clear, and ceased to become milky by the subsequent addition of a large quantity of water. I next passed a current of sulphuretted hydrogen through the liquid, till sulphuret of antimony ceased to fall down. This sulphuret, which had an orange colour, was washed on the filter, dried, and weighed, and then fused in a glass tube; it gave a black sulphuret of antimony, and merely traces of sulphur, consequently it was sulphuret of antimony containing 3 atoms of sulphur, or precisely that which ought to be formed under the circumstances. But as it contained traces of an excess of sulphur, in consequence of the sulphuretted hydrogen having been passed for a very long time through the liquid, I heated a part of the sulphuret in a bulb blown in the middle of a glass tube, and passed over it a current of hydrogen dried by chloride of calcium. The sulphuret of antimony was decomposed, and I obtained antimony, sulphuretted hydrogen, and traces of sulphur.

The liquor, separated from the sulphuret of antimony, was gently heated to drive off the sulphuretted hydrogen, but not the hydrochloric acid, which cannot be separated from water by heat when mixed with it in small proportion. The hydrochloric acid was then precipitated by nitrate of silver. The chloride of silver obtained had, however, a blackish colour, from a little sulphuret of silver which was mixed with it. The results of this analysis gave 1·937 gramme (29·9 grs.) of antimony, and 6·886 grammes (106·3 grs.) of chloride of silver, equivalent to 1·699 gramme (24·7 grs.) of chlorine. The chloride of antimony, therefore, is composed per cent. of

Antimony	53·27
Chlorine	46·73
	100·00

This result would accord much better with the calculated proportions, had I obtained the chloride of silver wholly free from sulphuret.

Another chloride of antimony is obtained by passing a current of dry chlorine over heated metallic antimony. The antimony burns vividly in the gas, emitting sparks, at the same time that a volatile liquid is formed. This liquid is white, or of a very light yellowish tint, and contains also chloride of iron if the antimony employed be not wholly free from that metal. That chloride, however, remains at the bottom of the vessel, and is not dissolved in the liquid, which resembles the fuming spirit of Libavius in all its external characters, having a strong disagreeable odour, and fuming in the atmosphere. Exposed to the air it attracts water, and is converted into a white mass, in which white crystals form, which afterwards dissolve without rendering the solution milky. This phenomenon is owing to a property of the liquid chloride of antimony, which it possesses in common with the fuming spirit of Libavius, of forming a crystalline mass when mixed with a small quantity of water.

The liquid chloride of antimony heats strongly when mixed with a larger portion of water. It becomes milky, and a precipitate forms which behaves exactly like hydrated antimonic acid. Gently heated it gives off water, and becomes yellowish; but at a high temperature it becomes white. The liquor contains hydrochloric acid. Since the liquid chloride of antimony is converted by water into hydrochloric and antimonic acids, the latter containing 5 atoms of oxygen to 1 of antimony, this chloride must contain 5 atoms of chlorine to 1 of antimony, and its composition per cent. is,

Antimony	42·15
Chlorine	57·85
	100·00

I analyzed the liquid chloride of antimony exactly in the same way as the solid chloride. I obtained sulphuret of antimony by sulphuretted hydrogen, which also had an orange colour, but rather paler than the sulphuret from the solid chloride. It contains 5 atoms of sulphur to 1 of antimony. Treated with dry hydrogen it is converted into metallic antimony and sulphur, and sulphuretted hydrogen is disengaged. I obtained 1·98 gramme (30·6 grs.) of metallic antimony, and the liquid freed from the sulphuret and precipitated by nitrate of silver gave

11·764 grammes (181·6 grs.) of chloride of silver, equivalent to 2·902 grammes (44·8 grs.) of chlorine. The chloride of silver, however, contained rather more sulphuret of silver than that obtained in the analysis of the solid chloride. The result of this analysis, therefore, gives per cent.

Antimony........................	40·56
Chlorine.........................	59·44
	100·00

which differs from the calculated result: the difference, however, is owing solely to the sulphuret of silver which remained mixed with the chloride.

If we pass dry chlorine over sulphuret of antimony containing 3 atoms of sulphur, we do not obtain the liquid chloride; but solid chloride of antimony and chloride of sulphur are formed. The latter may be separated from the chloride of antimony by heating them very gently in a matrass with a very narrow mouth; chloride of antimony alone remains. This is the same compound that is formed in the analysis of grey copper by chlorine; only chloride of antimony and chloride of sulphur are obtained, the first containing 3 atoms of chlorine, the second 2. No double chloride is formed, and the chloride of sulphur remains on the solid chloride of antimony. Heated so as merely to fuse the chloride of antimony, the latter dissolves entirely in the chloride of sulphur, and forms with it a homogeneous liquor; but the chloride of antimony separates in crystals on cooling. This is one method of obtaining large crystals of this chloride; but it must be quickly filtered through blotting paper to separate, as completely as possible, the adhering chloride of sulphur.

It is remarkable that the liquid chloride of antimony is produced by the action of chlorine on metallic antimony only, and that none is formed when the sulphuret of antimony is acted on by chlorine.*

II. *Compounds of Antimony and Sulphur.*

I have made many experiments on the sulphurets of antimony, but have only found three, which correspond with the oxides of that metal.

Sulphuret of antimony with 3 atoms of sulphur has different

* I frequently passed chlorine over sulphuret of antimony, and always with the same result. I fancied, for reasons to be stated in the sequel, that chloride of antimony with 5 atoms of chlorine was formed; I only obtained, however, the chloride with 3 atoms, if I drove off the chloride of sulphur. I was then led to think that 2 atoms of chlorine were separated from the chloride of antimony, and had combined with the chloride of sulphur to form, perhaps, a chloride with 4 atoms of chlorine. I, therefore, passed chlorine over chloride of sulphur, and freed it carefully by distillation from the sulphur dissolved, in order to discover such a chloride of sulphur. The colour of the chloride of sulphur became indeed a little darker, but it underwent no other change, although the chlorine was passed over it for a considerable length of time.

colours. The native is lead grey; its composition has been demonstrated by Berzelius. It is analogous to the oxide of antimony with 3 atoms of oxygen, for it dissolves in hydrochloric acid without leaving any residuum, and with the disengagement of sulphuretted hydrogen only.

The same sulphuret is obtained by passing a current of sulphuretted hydrogen through a solution containing oxide of antimony; but it has an orange colour, almost like that of the golden sulphuret. It becomes brownish by drying, and then assumes an aspect more similar to that of kermes. The same sulphuret is obtained by passing sulphuretted hydrogen through a solution of tartar emetic, or through a solution of butter of antimony in water and tartaric acid.

M. Berzelius first proved that the composition of kermes mineral is exactly similar; its colour, however, is brownish-red.*

The deuto-sulphuret of antimony with 4 atoms of sulphur has an orange colour a good deal like that of the golden sulphuret. It is formed when sulphuretted hydrogen is passed through a solution of antimonious acid. Tartaric acid, however, must not be added for the purpose of enabling us to dilute the liquid with water, but only hydrochloric acid.† The best method of making a solution of antimonious acid is to dissolve antimony in aqua regia, and evaporate the solution to dryness. The antimonic acid formed is then to be converted into antimonious acid by a red heat; the latter fused with caustic potash, and the fused mass heated with hydrochloric acid and water till a clear liquid is obtained. I precipitated this solution by sulphuretted hydrogen, and the sulphuret obtained, after being carefully dried, was decomposed by hydrogen. I obtained in one experiment 1·305 gramme (20·1 grs.) of antimony from 1·973 gramme (30·5 grs.) of sulphuret, and in another 0·977 gramme (15·1 grs.) of antimony from 1·468 gramme (22·7 grs.) of sulphuret. Its composition, therefore, according to the first trial, is per cent.

Antimony	66·14
Sulphur	33·86
	100·00

and, according to the second,

Antimony	66·55
Sulphur	33·45
	100·00

* I analyzed a kermes prepared by digesting black sulphuret of antimony in a solution of carbonate of potash. I expelled the hygrometric moisture by a gentle heat, and decomposed it by hydrogen. 0·719 gramme (11·1 grs.) of kermes gave 0·52 gramme (8 grs.) of antimony: its composition, therefore, was antimony 72·32, sulphur 27·68.

† The addition of tartaric acid to antimonious acid affords very remarkable results. I shall give a separate memoir on that subject.

According to calculation, its composition is,

Antimony	66·72
Sulphur	33·28
	100·00

The sulphuret of antimony with 5 atoms of sulphur to 1 of metal, which corresponds to antimonic acid, and by calculation contains 61·59 antimony and 38·41 sulphur, is realized in the golden sulphuret of the shops. The different modes of preparing it are well known. It is also obtained if we pass a current of sulphuretted hydrogen through solutions containing antimonic acid, as, for instance, that of the liquid chloride of antimony in water, to which tartaric acid has been added. The precipitate obtained is of a paler orange colour than the precipitate from solutions of oxide of antimony, and its colour does not change in drying.

I analyzed the golden sulphuret in two ways; I dried it at a heat not sufficient to decompose it, till it ceased to lose weight. It had then parted with all its hygrometric moisture. I usually analyzed it by passing a current of dry hydrogen over the heated golden sulphuret. Sulphuretted hydrogen was formed, but no water; sulphur sublimed, and metallic antimony remained behind. I also analyzed it by aqua regia, to which I added tartaric acid. I separated the undissolved sulphur, and precipitated the sulphuric acid by muriate of barytes; this method, however, is more tedious than that with hydrogen. We do not obtain accurate results by fusing the golden sulphuret in a small matrass in order to convert it into sulphuret of antimony with 3 atoms of sulphur, and calculating the composition of the former from the weight of the latter, not only because the sulphuret of antimony is not absolutely fixed, but also because some oxide of antimony is formed by the air in the matrass, which produces a *crocus antimonii* with the sulphur sublimed in its neck.

I do not mention the results of the analyses which I made of this sulphuret of antimony at a *maximum*, because they scarcely differ from the calculated result.

III. *Compounds of Sulphuret of Antimony with Oxide of Antimony.*

The compounds in which sulphuret of antimony is combined with oxide of antimony in various proportions are called in the shops by the name of *crocus* and *nitrum antimonii*. Kermes has also been supposed to be a similar compound. M. Berzelius, however, has shown that its composition does not differ from that of the sulphuret of antimony with 3 atoms of sulphur, and the analysis of kermes related above confirms his opinion.

There is, however, a combination of sulphuret of antimony with oxide of antimony in definite proportion, and that is the native kermes of mineralogists (*rothspiesglanzerz*). The result of my analysis of that substance differs very much from that obtained by Klaproth, in consequence of his having supposed that the whole of the antimony was both oxidated and sulphuretted, and of his not having determined the quantity of the antimony. According to him, its composition is,

Antimony	67·8
Oxygen	10·8
Sulphur	19·7
	98·3

I analyzed the rothspiesglanzerz from Braunsdorf, near Freiberg, in Saxony, which M. Weiss had the goodness to send me for the purpose. The analysis was made by hydrogen in the same manner as those of the different sulphurets of antimony. I added, however, to the apparatus a weighed tube containing chloride of calcium, to absorb the water formed. In one experiment I obtained 0·676 gramme (10·4 grs.) of antimony and 0·054 gramme (0·84 grs.) of water from 0·908 gramme (14 grs.) of the mineral, or 74·45 per cent. of antimony and 5·29 of oxygen; and in another, from 0·978 gramme (15·1 grs.) of the mineral, 0·74 gramme (11·4 grs.) of antimony, and 0·047 gramme (0·73 gr.) of water, or 75·66 per cent. of antimony and 4·27 oxygen. I then dissolved 0·34 gramme (5·24 grs.) of the mineral in aqua regia, added tartaric acid to the solution, and precipitated by muriate of barytes. I obtained 0·517 gramme (8 grs.) of sulphate of barytes, equivalent to 20·47 per cent. of sulphur.

If we take the mean of the oxygen in the two first analyses, viz. 4·78 per cent. and add to it as much antimony as is necessary to form the oxide, the remaining quantity of metal is sufficient, neglecting slight errors of observation, to form with the sulphur, the sulphuret of antimony with 3 atoms of sulphur. We shall find besides that the quantity of oxide of antimony is to the quantity of sulphuret as the weight of an atom of the first is to the weight of two atoms of the second, so that the native kermes consists of 1 atom of oxide of antimony and 2 atoms of sulphuret of antimony, or per cent. of

Sulphuret of antimony	69·86
Oxide of antimony	30·14
	100·00

This composition accords with that which M. Berzelius had already assigned to the native kermes. This compound is remarkable as presenting the only instance of a native crystallized oxisulphuret that has hitherto been discovered.

Article IV.

An Attempt to divide the Echinida, or Sea Eggs, into Natural Families. By J. E. Gray, Esq. FGS. &c.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *British Museum.*

Lamarck in his History of Animals without Vertebræ, established as a class of his Apathic animals a very natural group, which he called *Radiata*. Mr. Macleay has since proposed to consider this group as equal in rank to the subkingdoms, Vertebrata, Annulosa, Mollusca, &c.

Lamarck divided his Radiata into two great divisions, which Cuvier placed in distinct parts of his Zoophytes. The former separated the first of his divisions into two, and the second into three sections. Mr. Macleay, in his Horæ Entomologicæ (part ii. 116), has hinted at the connexion which exists between these five principal constructions, which, if Radiata is to be considered as a subkingdom, should, for uniformity sake, be called classes, although each group formed only a single genus in the works of Linnæus and his followers.

The following are the groups proposed by Lamarck and Macleay, which may be divided in the following manner:—

1. *Normal Group,* Echinodermata.
Echinida, Mac. Les echinides, *Lam.*
Stellerida, Mac. Les stellerides, *Lam.*

2. *Annectant Group.*
Medusida, Mac. Les medusaires, *Lam.*
Acalephida, Mac. Les anomales, *Lam.*
Fistulida, Mac. Les Fistulides, *Lam.*

Leaving the determination of the rank which these groups ought to sustain to be considered till more is known of their anatomy and habits, I shall at once proceed to the division of *Echinida*, the object of my present communication, into families.

Class 1.? Order 1.? Echinida, *Macleay.* Les echinides, *Lam.* Echinus, *Lin. Cuv.*

Essential character.—Body not contractile, nor radiately lobed, subglobular, covered with mobile spines; anus distinct from the mouth.

These animals are furnished with a distinct crustaceous skeleton, composed of numerous regularly disposed plates, united by a strait suture, and furnished externally with rounded tubercles, on which mobile spines are attached. These spines are affixed to the base of the tubercles by a circular ligament, and are furnished with numerous muscles for the purpose of moving them in every direction.

The manner in which the spines and plates enlarge has never been satisfactorily explained. The plates appear to be placed between two skins, and a small process of membrane seems to extend between each of them. The spines evidently grow by a disposition of matter placed under their outer edge, more especially at the apical extremity; the matter is, perhaps, deposited by the processes of the skin of the articulation being extended up the longitudinal grooves with which these spines are always furnished. Their manner of growth may easily be seen by cutting down longitudinally the spines of *Echinus mammilatus, Lam.* when the outer surfaces of each complete spine will be distinctly visible in the form of a darker line, occasioned by the outer edge being the hardest and most compact.

The older naturalists paid very great attention to the group, and several of them divided them systematically into "classes, sections, and genera." Morton (1712), in his History of Northamptonshire, divided the fossil species found in that county into three groups. Bryerius, in 1732, divided the *Echini* into seven genera from the position of their mouth and vent: these genera have all been adopted by Lamarck, but under other names. Klein, two years after, published his natural disposition of *Echinodermata*, where he divided them into nine sections, containing twenty-two genera, placing them according to two systems; first, after the position of their vent, forming them into three classes, called *Anocystos, Catocystos,* and *Pleurocystos;* secondly, with respect to the situation of the mouth, as *Emmesostomi* and *Apomesostomi.* Van Phelsum, in 1714, extended Klein's method by taking notice for the first time of the form and extent of the *Ambulacra.* He divided them into the same number of genera as Klein, but several of them were very different from those of the latter. Leske, in 1778, published an addition to Klein, and he reduced the number of Klein's genera to ten, which agree very nearly with the sections of his author, and he adopted the prior names of Bryerius for his genera. Davilla, in 1767, divided the *Echini* into six groups, according to the general form of the shell, but these groups are very indefinite.

Linnæus took no notice of the works of Klein or others, but considered the whole of the group as one genus. Muller divided it into two, under the names of *Echinus* and *Spatangus.*

The *Echinida* may be divided into two sections.

1. Typical Group.—*Body globular; mouth central, below; jaws conical, projectile, with five acute teeth; anus vertical, dorsal; ambulacra complete, forming bands extending from the mouth to the anus.*

The crustaceous covering of the body of these animals is formed of twenty perpendicular bands, each formed of several horizontal pentagonal pieces. These bands are placed symmetrically in pairs united together by a flexous suture, the projecting angle of one series being fitted into the concave angles of the other. The pairs of bands are united together by a strait suture. They are alternately broad and narrow. The broad ones are formed of a few plates, and always imperforated, and the outer edges of the narrow bands, which consist of very numerous narrow pieces, are perforated by two or more series of minute perforations placed in pairs; these perforations form bands, which Linnæus compared to the walks in a garden, calling them *ambulacra* or walks, and the tubercular parts *areæ, pulvilli,* or beds. As in describing the species, it is often necessary to distinguish the character of each of the beds, those of the broad bands might be called *extra,* and those of the narrow bands *intra ambulacral beds.*

The vent is surrounded by numerous small scale-like pieces attached to the skin; these are again surrounded by two series of plates, each formed of five pieces, which are affixed to the body of the crustaceous skeleton. The series of these plates which is next the body, are the smallest; they are placed just at the top of the ambulacra, and each is perforated with a minute hole, the use of which is quite unknown. The inner series is formed of larger pieces, each perforated with a considerable foramen, which lead to the ovaria. The latter may, therefore, be called the *ovarial,* and the former, as they are partly between them, the *interovarial pieces.* One of the ovarial plates is considerably larger than the rest, convex externally, and perforated like a sieve with numerous minute foramina, and internally thick and rugose. This plate is somewhat similar both in form, and perhaps in use, to the orbicular spot on the back of the *Stellerida* called *Corpus Spongiosum,* by Spix, figured in the Annals of the Museum (vol. xiii. t. 32, f. 1, a), where M. Spix considers that it may, perhaps, be the orifice of the organs of generation as it is perforated with two foramina (see p. 446).

The skin round the mouth is scaly, and furnished with ten somewhat prominent glands, placed in pairs; the jaws, which were compared by Aristotle to a lantern, consist of five conical triangular bones, each formed of two pieces, containing in their middle a long linear curved tooth; the teeth are externally convex, and furnished with an internal mid-rib, and the end hardens as they are worn away; these jaws are articulated together by the intervention of five oblong bones, converging towards the centre, and furnished with five other linear arched bones. The jaws are moved by muscles placed between them, and by some attached to five variously formed erect processes placed on the oral edge of the body of the shell; these parts are figured, but not very correctly, by Klein, t. 21.

Round the oral edge of the body of the shell are placed ten more or less distinct grooves, situated at the base of each arm of the internal processes, and diverging from them; and on the edge of the extra ambulacral beds just by the outer margin of the ambulacra. I am not quite sure what is the use of these grooves, but they have very much the appearance of pulleys, over which some muscles work, and examining a specimen of *Echinus mammillatus* preserved in spirits, I observed ten shrub-like bodies attached to filiform nerves, or muscles, which appear to extend up towards the body of the shell: I am, therefore, inclined to think that they are the means by which spines are moved and nourished, but I cannot at present decide, from the want of live and preserved specimens, to dissect and examine more minutely.

This group is synonimous with the genus *Echinometra* of Bryerius, and the section *Cidaris* of Klein. It is divisible into two families.

Fam. 1. Cidaridæ. Cidaris, *Lam.*

Body with two-sized spines; larger ones club-shaped, or very long; spine-bearing tubercles perforated at the apex.

Gen. 1. Cidaris, *Klein, Lam.* Turbans.

Body depressed, spheroidal; ambulacra waved; small spines compressed, two edged, two rowed, covering the ambulacra, and surrounding the base of the larger spines.

This genus may be divided according to the form of the larger spines; the extra ambulacral beads have only two rows of spines.

C. imperialis, *Lam.* *Klein,* t. vii. f. A.

2. Diadema. Diadems.

Body orbicular, rather depressed; ambulacra strait, spines often fistulous.

*D. setosa, *Leske, Klein,* t. 37, f. 1, 2. Echinus Diadema, *Lin.* **D. calamaria. Echinus, *Pallas, Spix. Zool.* t. 2, f. 4, 8.

3. Astropyga.

Body orbicular, very depressed; ambulacra strait; ovarian scales very long, lanceolate; beds with several series of spines.

A. radiata, *Leske,* t. 44, f. 1.

Fam. 2. Echinidæ.

Body with nearly uniform spines; spine-bearing tubercles not perforated.

Echinus, *Lin. Van. Phelsum.*

Body orbicular, subangular; beds with cross rows of spines.

E. esculentus, *Lin.*

Echinometra, *Bryerius. Van Phelsum.*

Body ovate or elliptical, each bed with two rows of large tubercles; ambulacra flexuous; allied to *Cidaris.*

*E. Lucuntur. Echinus, *Lin.* *Klein,* t. 30, f. A. B. **E. atratus, *Klein,* t. 47, f. 12. ***E. mammillatus, *Klein,* t. 29, f. 1. The last two sections will most probably form a new genus.

The genus *Clypeus* of *Klein* and *Leske,* the *Echinosinus* of *Van Phelsum,* appears to be very doubtful. Lamarck considers them species of *Galerites.*

2. ANNECTANT GROUP.—*Body not globular, variously shaped; jaws not projecting; anus lateral or below; anus and mouth covered with imbricate irregular scales.*

Fam. 3. SCUTELLIDÆ.

Body depressed or conical, covered with numerous minute equal-sized, immersed tubercles; spines short, conical, thin, equal; ambulacra in ten short bands bending together in pairs, like the petals of a flower, formed of two distinct lateral holes united by an external groove; mouth central; teeth blunt, not exserted; jaws formed of five pair of depressed triangular bones; internal cavity divided by numerous vertical columns supporting the jaws; internal oral edge with five pair of simple processes; ovarial pores 4-5, situated between the ambulacra surrounding the *Corpus Spongiosum*; interovarian pores minute, at the centre of the apex of the interambulacral area.

The crustaceous covering of the body is usually thickened internally by an additional coat, and, being strengthened by the internal columns, resists the action of the sea for a length of time. It is formed of twenty bands of pieces, but the continuation of the ambulacral bands are often very much dilated and perforated.

This family is allied to the *Echinidæ* by the equal spines, and having jaws, &c.; to the annectant families of the order *Stellerida,* by their jaws being only used for pressing the food, and by the radiated-lobed form of some of the species.

*Echinanthus, *Bryerius.* Clypeaster, *Lam.*

ECHINANTHUS, *nob.* not *Van Phel.* Echinorodum, *Van Phel.* Scutum angulare, *Klein.*

Body oval, or sub-pentangular, above convex, beneath concave, with five grooves; ambulacra in pairs, rounded; ovarial pores five; mouth central; anus marginal; the jaws, *Klein,* t. 33, f. p. q.

*Interambulacral area rounded, E. humilis, *Leske,* t. 17, f. A. t. 10 B. Echinus rosaceus, *Lin.* E. subdepressa, *nob. Klein,* t. 19, f. A B. *Seba,* iii. t. 15, f. 15 and 12. E. ambigena, scutella, *Lam.* *Seba,* iii. t. 15, f. 13, 14. **Interambulacral; area acute. E. altus, *Leske,* t. 53, f. 4.

LAGANA. Placenta lagana, *Klein.* Echinodiscus, *Van Phel.* not *Bryerius.*

Body sub-pentangular, depressed, below rather concave;

ambulacra in pairs, rounded; mouth central; anus between the margin and the mouth.

L. minor. Echinodiscus laganum, *Leske*, t. 22, f. *a*, *b*, *c*. L. scutiformis, *Seba*, iii. t. 15, f. 23, 24; see Scutella, n. 15, 16, *Lam.*?

**Echinodiscus, *Bryerius*. Scutella, *Lam.*

Echinarachnius, *Leske*. Arachnoides, *Klein*.

Body flattened, outline orbicular, or subangular, above rather convex, edge thin; ambulacra in pairs, like a flower; mouth central; anus marginal.

E. placenta. Scutella, *Lam.* Klein, t. 20, f. A B. E. parma, Scutella, *Lam.* E. lenticularis, Scutella, *Lam.*

Echinodiscus, *Leske*. Mellita and Rotula, *Klein*.

Body flattened, outline orbicular, above rather convex, edges thin; ambulacra in pairs, like a flower; mouth central; anus between the margin and the mouth. Jaws, *Klein*, t. 33, f. *r*, *s*.

*Entire. E. orbicularis. Echinus, *Gmelin*. *Leske*, t. 45, f. 6, 7. E. fibularis, Scutella, *Lam.* **Lobed. E. inauritus, *Seba*, iii. t. 15, f. 3, 4. E. auritus, *Seba*, iii. t. 15, f. 1, 2. E. dentata, *Klein*, t. 22, f. E F. ***Perforated. E. bifora, *Klein*, t. 21, f. A B. E. digitata, *Klein*, t. 22, f. A B. E. octodactylus, *Klein*, t. 22, f. C D, &c.

Echinocyamus, *Leske*. Fibularia, *Lam.*

Body subglobular; outline ovate, or orbicular, edge rounded; inside with columns; ambulacra in pairs, short, like a flower; mouth central; anus between the mouth and edge.

E. ovulum, Fibularia, *Lam.* E. pusillus, Spatangus, *Muller*, *Zool. Dan.* iii. t. 91, f. 5, 6. E. tarentina, *Lam.* E. trigona. Fibularia, *Lam.*

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Cassidulus, *Lam.*

Body elliptical; outline ovate, above rather convex; ambulacra 5 stellate; mouth central; anus between the vertex and the margin.

C. complanatus, *Lam.* C. scutella, *Lam.*

C. lapis Cancri by its figure appears to be allied to the genus *Echinolampas*, and will, perhaps, form a new genus.

Fam. 4. Galeritidæ.

Body ovate or conical, covered with numerous small, equal, sunk tubercles; spines short, small, equal; ambulacra complete, forming bands (rarely interrupted at the edge) from the mouth to the vertex; mouth mostly central; jaws ——?; internal cavity hollow, destitute of vertical pillars; ovarian pores 4; corpus spongiosum vertical, in the centre of the ovarial perforations;

the interovarial perforations minute, at the apex of the ambulacra. The body formed like *Echinidæ* of twenty bands, the ambulacral band being the narrowest; the sutures are not so distinctly sinuous as in *Echinidæ.*

GALERITES, *Lam.* Fibula conulus, *Klein.* Echinites, *Van Phel.* Echinometra, *Van Phel.*

Body conical; base orbicular or subangular; ambulacra ten, each formed of two series of perforations placed together in pairs, extending without interruption from the mouth to the vertex; mouth central; anus marginal; only found fossil.

G. vulgaris, *Lam. Klein,* t. 14, f. A K.

DISCODEA. Fibula discoidea, *Klein.* Galerites, *Lam.* Echino discoides, *Van Phel.*

Body orbicular, depressed; ambulacra ten, placed in pairs; alternately smaller, rest like *Galerites.*

D. rotularis. Galerites rotularis, *Klein,* t. 14, f. L—O.

ECHINANAUS, *Kœnig.* Echinoneus, *Van Phel.* and *Lam.* Echinoconi pars, *Bryerius.*

Body obovate or orbicular, rather depressed; ambulacra ten, placed in pairs, extending without interruption from the vertex to the mouth; mouth central; anus placed between the mouth and the margin.

E. cyclostomus, *nob.* Echinoneus cyclostomus, *Klein,* t. 37, f. 3, 4.

ECHINOCORYS, *Bryerius.* Echinus salaris and pelagius, *Van Phel.* Cassis Galea and Galeola, *Klein.* Anachites, *Lam.*

Body ovate, convex; base oval, flattened; ambulacra ten, placed in pairs, extending without interruption from the vertex to the mouth, where they become closed together; mouth lateral, transverse; anus marginal.

E. ovatus, *Leske,* t. 53, f. 3. Anachites ovata, *Lam.* This genus is very closely allied to the *Spatangidæ,* and, perhaps, should be referred to them.

ECHINOLAMPAS, *nob.* Echinanthus? *Van Phel.* Echinanthus, *Leske.* Clypeaster, *Lam.* Scutum ovatum, *Klein.*

Body ovate, convex; base ovate, flattened, extended posteriorly; ambulacra ten, placed in pairs, rather distant, near the vertex, interrupted at the edge, and close together near the mouth; mouth subcentral; anus marginal.

*E. Kœnigii, *nob.* Echinoneus lampus, *De la Beche, Trans. Geol. Soc.* i. t. 3, f. 3, 4, 5. **E. oviformis, *nob.* Echinus, *Gmelin.* Clypeaster oviformis, *Lam. Klein,* t. 20, f. *c d.* E. orientalis, *nob. Seba,* iii. t. 10, f. 23, 24.

ECHINOBRISSUS, *Bryerius.* Nucleolites, *Lam.*

Body ovate or cordiform, rather convex, grooved in front; ambulacra ten, in pairs, radiating without interruption from the vertex to the mouth ; mouth subcentral ; anus dorsal.

E. Bryerii, n. Nucleolites scutata, *Lam.* *Bryerius*, t. 6, f. 1, 2.

Fam. 5. Spatangidæ.

Body ovate or heart-shaped, rather gibbous, covered with numerous small, and some scattered, rather larger tubercles ; spines setaceous, depressed, unequal-sized, the larger tubercles perforated ; ambulacra subcomplete, interrupted at the edge, forming a cross, uniting by pairs, each formed of two rows of perforations ; mouth submarginal, below, transverse, destitute of jaws ; internal cavity destitute of vertical pillars ; ovarial pores four, close together, vertical ; interovarial pores very small ; corpus spongiosum vertical, anterior.

The crustaceous covering of the body of these animals is thin, and formed of twenty bands of pieces, like all the other *Echinidæ*, but the interambulacral areæ are unequal ; the posterior lateral ones are usually very broad, the lengthening of which is formed more especially by the extension of the pieces of the posterior band of this area ; the hinder middle area is rather irregular ; the posterior series of each of the two hinder ambulacra being extended into it just below the anus so as to form an isolated subpentangular piece, externally marked by a smooth groove, the ambulacra then being extended beyond it, leaving a central ovate, or lanceolate medial inferior area. Round the mouth there are five grooves, the continuation of the ambulacra, which are more or less perforated with holes, through which pass out branched tentacula, like those of Holothuria, see *Leske*, t. 43, f. 5.

The species like *Brissus purpureus*, which have very distinct larger spines, are allied to *Cidaridæ* by the tubercles of the larger spines being perforated. The whole family is allied to *Holothuriadæ* by the thin texture of the crustaceous covering, and by the mouth being destitute of jaws, and surrounded by branched appendages.

*Echino spatangus, *Bryerius.* Cor marinum, *Klein.*

Spatangus, *Klein.*

Body cordiform ; back with large perforated tubercles ; ambulacra four, the posterior one wanting, or not perforated.

S. purpureus, *Leske*, t. 43, f. 3, 5 ; t. 45, f. 5.

Echinocardium, *Van Phel.?*

Body cordiform ; back equal, tuberculated ; ambulacra five, the posterior one in a groove, perforated.

E. atropos. Spatangus, *Lam.* E. pusillus, *Leske*, t. 38, f. 5 ; *Klein*, t. 24, f. *c d e*. E. seba, *Seba*, iii. t. 10, f. 21, A B.

**Echino brissus, *Bryerius.* Ovum marinum, *Klein.*

Brissus, *Klein.* Nuces, *Van Phel.*

Body ovate; ambulacra four, the hinder wanting, all surrounded by a groove.

*B. ventricosus, *Leske. Klein*, t. 26, f. A.; unicolor, *Leske. Klein*, t. 26, f. B C. **B. carinatus, *Leske*, t. 48, f. 4, 5. *Seba*, iii. t. 14, f. 3, 4. B. columbaris, *Seba*, iii. t. 10, f. 19.

Ova, *Van Phel.* Brissoides, *Klein.*

Body ovate, deeply grooved in front; ambulacra five, impressed.

O. canaliferus. Spatangus, *Lam. Klein*, t. 27, f. A.

The Spatangus prunella, *Lam. Kœnig, Icones Foss. Sectiles*, t. 3, f. 34, appears to be the type of *Van Phelsum's* genus *Amygdala*, which is peculiar for the anus being nearly dorsal; and Spatangus radiatus, *Klein*, t. 2, 5, appears to form a new genus.

Article V.

Singular Fossil Nuts. By J. L. Levison, Esq.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, 54, *Berwick-street, Oxford-street.*

As I am in possession of some fossil nuts which I think highly interesting in a scientific view, particularly as I do not recollect having seen them noticed in any work on geology, I will, therefore, briefly state their history. They are, or appear to be, a similar kind to our common wood nuts, and were found near the Giant's Causeway in the North of Ireland; the kernels are changed into carbonate of lime, with a slight trace of iron; they have quite a chalcedonic aspect, are translucent, and much harder than common limestone fossils; in their general appearance they, in common with other organic remains, have all the peculiarities of the original substance; these appear to be worm-eaten, the places originally perforated remaining; but the phenomena I would more particularly request your attention to are, *that the shells of these nuts are unaltered in their peculiar properties;* they are slightly discoloured, they retain the ligneous appearance, burn with a bright flame, which converts them into charcoal: the only difference I can detect in these fossil shells from recent ones is, that during combustion they give out a sulphurous odour, and do not make any crackling noise. With these shells and nuts are found fragments of fossil wood (probably from the tree the nuts grew upon) perfectly converted into carbonate of lime; and what appears to me the most enigmatical is, that the shells have not any adventitious earthy matter, and that these pieces of wood do not retain any of the original substance. I applied heat to a specimen of the wood, but it only became hot giving off the acid, &c.; it effervesces with muriatic acid very strongly. If you think these curious facts worthy of a place in the *Annals of Philosophy*, they may probably induce some of your readers to offer a theory to elucidate these very natural questions; viz. Why the shells are unaltered in all the characteristic properties of wood? And why the fragments of the wood and the nuts (which were all found together) have undergone a perfect change? Whether we may consider the shells preserved (chemically) by essential oil of the nuts, bitumen, or the adventitious circumstance of being impregnated with sulphur?

I remain, Gentlemen, your very obedient servant,

J. L. Levison.

P. S. I shall be most happy to give you or any of your correspondents ocular proof of the facts stated, by calling on me.

*** Specimens of fossil nuts, precisely similar to those described by Mr. Levison, may be seen in the British Museum: they are from Carrickfergus Bay.—*Edit.*

Article VI.

On the Advantages of High Pressure Steam. By Mr. John Prideaux.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Plymouth, Oct.* 4, 1825.

If the following obvious remarks have been any where anticipated, this letter may be destroyed. But having seen an attempted demonstration in your fourth volume, and heard it lately repeated by distinguished practical engineers, that *no direct advantage results from the use of high pressure in steam engines, because the force of steam is as its density, and its caloric a constant quantity at all densities,* thus making the force just proportional to the fuel employed; it seemed to me worth while to occupy one or two of your pages with what I apprehend to be a more correct view of the subject.

It is established by sufficient experiments,

1. That the caloric of steam, *in contact with water,* is a constant quantity at all temperatures.

2. That every elastic fluid, *at a given density,* has its expansive force in proportion to its temperature, increasing $\frac{1}{480}$ for each ascending degree of Fahrenheit.

3. That every elastic fluid, *at a given temperature,* has its expansive force directly as its density.

From these premises it follows, that the force of steam is directly as its density, multiplied by $\frac{481}{480}$ for each degree of increased temperature, the caloric corresponding with the density alone.

For instance: steam at 212° has an elastic force = 30 inches of mercury; and at 300° = nearly 140 inches, neglecting fractions.

By the second law, steam of the density due to 212° raised 88° with a geometrical increase of $\frac{481}{480}$ for each degree, shall gain about 5·6 inches; or possess at 300° a force = 35·6 inches of mercury.

And by the third, the *density* due to 300° shall be as 35·6 inches to the force found = 140 inches, or about 3·9 times greater than at 212°.

But the caloric being constant is in simple proportion to this density; and the fuel consumed must be expected to correspond with the caloric.

Then 30 inches × 39 = 117 inches, the force due to the *density* at 300°, deducted from 140 inches, the force found by experiment, gives 23 inches, the profit by working at 300°.

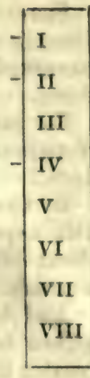
If this example be just, the weight of steam employed having its caloric constant, shall be a measure of the fuel consumed; and there is a direct profit in the ratio of $\frac{481}{480}$ for each ascending degree of Fahrenheit, as above stated.

It is plain from the nature of geometrical progression, that this profit shall increase as the temperature is more elevated: if we work at 600° the force of each *pound* of steam shall be double of that at 212°; and if we go up to 960° or 980°, it shall be quadruple; the caloric, and consequently the fuel, remaining a constant quantity.

It is easy to illustrate this from the reports of working engines, but the effects in these cases are dependent on such mixed causes, that no uniform conclusion can be drawn from them. Can you refer me to Watt's experiments on the *density* of steam?

In taking the caloric contained in the steam as a measure of the fuel consumed, there is not exact precision: radiation will of course increase with temperature; but I thought this might probably be more than compensated by the diminished surface of the vessels; and that, in the rapid action of a steam engine, it could hardly make an appreciable difference.

A collateral advantage of not less importance, well known to engineers, and which did not escape the sagacity of Mr. Watt, is gained, in allowing high pressure steam to expand in the cylinder. Mr. Watt has given a formula for calculating the profit on this proceeding; but for a much more perspicuous demonstration, I am indebted to a conversation with Mr. Perkins.



Suppose we have to work at a pressure of 10 lbs. on the inch.

Let the steam be raised to a force of 80 lbs. on the inch, and let in $\frac{1}{8}$th of the stroke; then stop the communication, the piston being at I.

We have thus $\frac{1}{8}$th at 80 lbs.

When the steam has expanded to II, the volume is doubled, and the force reduced to 40 lbs. (supposing the cylinder to keep the temperature constant), the mean from I to II being 60 lbs.

Hence we have $\frac{1}{8}$th at 60 lbs.

When the piston reaches IV, the volume is again doubled, and the force reduced to 20 lbs. the mean from II to IV being 30 lbs.

This gives $\frac{1}{4}$th stroke at 30 lbs.

On reaching VIII, the volume will again double itself, and the force will be reduced to $\frac{1}{2}$; thus becoming 10 lbs. on the inch as proposed; but the mean, from IV to the bottom, is 15 lbs.

Which makes $\frac{1}{2}$ stroke at 15 lbs.

Adding these quantities together, we have

	I.	$\frac{1}{8}$ at 80 lbs.	=	10lbs.
I to	II.	$\frac{1}{8}$ at 60 lbs.	=	7·5
II to	IV.	$\frac{1}{4}$ at 30 lbs.	=	7·5
IV to	VIII.	$\frac{1}{2}$ at 15 lbs.	=	7·5
				32·5 lbs. on the inch.

for the mean impetus communicated to the fly-wheel by each stroke of the piston: and as the cylinder full of steam is at a density of only 10 lbs. on the inch, the power thus gained appears, at first view, enormous.

But against this must be set, the irregularity of the impulse communicated to the fly, and of the temperature supplied to the cylinder; beside the additional weight and friction of the machinery, and other considerations; involving too many theoretical principles to allow of a satisfactory estimate from calculation, without direct and repeated experiment.

Enough, however, is known, to prove both in practice and theory, that great profit is attainable by working steam at high temperatures; and the limit of economy appears to me the degree at which water is decomposed by the containing vessels.

I am, Gentlemen, your very humble servant,

John Prideaux.

Article VII.

Astronomical Observations, 1825. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Sidereal Time.

1825.	Stars.	Transits.		
Oct. 21.	—c¹ Capric.	21h	35′	43·77″
21.	—345 Aquarii	21	49	06·47
21.	—30 Aquarii	21	54	07·97
21.	—44 Aquarii	22	08	02·05
21.	—Moon's First or West Limb	22	11	11·09
21.	—51 Aquarii	22	15	03·70
21.	—166 Aquarii	22	28	45·40
21.	—183 Aquarii	22	30	48·65
21.	—219 Aquarii	22	38	52·70
22.	—ρ Aquarii	22	19	53·41
22.	—η Aquarii	22	26	26·27
22.	—1 Piscium	22	46	07·11
22.	—2 Piscium	22	50	33·79
22.	—Moon's First or West Limb	22	56	50 47
22.	—a Piscium	22	59	47·37
22.	—γ Piscium	23	08	10·02
22.	—68 Piscium	23	14	38·38
22.	—κ¹ Piscium	23	18	02·27
24.	—Moon's First or West Limb	23	27	28·92
24.	—δ Piscium	23	39	41·53
24.	—297 Ceti	23	59	18·61
25.	—58 Piscium	0	37	59·12
25.	—75 Piscium	0	57	26·95
25.	—Moon's First or West Limb	1	14	00·78
25.	—η Piscium	1	21	51·54

Eclipses of Jupiter's satellites.

Oct. 25.	Immersion of Jupiter's first satellite	16h	23′	09″	Mean Time at Bushey.
		16	24	30	Mean Time at Greenwich.
Nov. 4.	Immersion of Jupiter's second satellite	14	55	12	Mean Time at Bushey.
		14	56	33	Mean Time at Greenwich.

Article VIII.

On Three new Salts of Soda. By Thomas Thomson, MD. FRS.

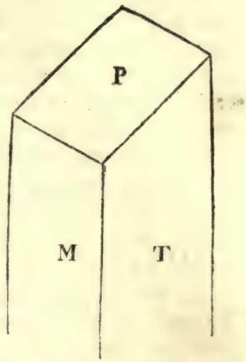
The salts which have been already examined by chemists with more or less attention amount to about 840. But this number, great as it may appear, constitutes but a very small proportion of the saline combinations, capable of being formed. We can hardly examine even the most common substances with any attention without being struck with some new and unexpected phenomena. Indeed our knowledge of saline combinations and of the limits within which these combinations are confined, is so important that we are not prepared to reason generally on the subject. Compounds are perpetually presenting themselves which are at apparent variance with our preconceived notions. For example, what opinion is more firmly established than that the saline compound of sulphuric acid and soda cannot crystallize unless it be combined with a considerable quantity of water? Yet I have lately met with this acid and base combined in definite proportions and in well-formed crystals totally destitute of combined water.

The muriatic acid of commerce is never free either from sulphuric acid or iron. On that account I am in the habit of preparing muriatic acid for the purposes of analysis by passing a current of muriatic acid gas through distilled water till the liquid refuses to absorb any more. The common salt from which the muriatic acid gas is evolved is put into a large retort, and the requisite quantity of sulphuric acid of commerce to decompose it is poured in at intervals through the tubulated opening in the retort. The retort is heated by a lamp.

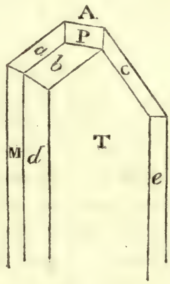
By this process a vast quantity of gas is evolved at first; but it gradually diminishes, and at last ceases altogether long before the whole of the common salt is converted into sulphate of soda. Indeed this complete decomposition cannot be effected without a degree of labour and a repetition of so many processes, that I find it not worth while to prosecute it beyond a certain point. There remains in the retort an indurated, white, and very sour tasted salt, which I dissolve out by filling up the retort with water, and digesting it on the sand-bath. The difficult solubility of this saline residue is so considerable that repeated digestions and a great deal of water are necessary to remove it out of the retort. If the first solution obtained in this manner, which is exceedingly acid, containing a great excess of sulphuric acid, be concentrated on the sand-bath and set aside for crystallization, the first crop of crystals formed is usually very similar in shape to glauber salt; but they are much firmer and heavier, and have an exceedingly acid taste. These crystals do not always appear, and I have not ascertained the circumstances upon which the appearance depends; though it is probably connected with the proportion of excess of acid which the liquid contains. But I have procured them several times successively in the circumstances just described, and see no reason to doubt that other chemists, by proceeding in the same manner, will be equally fortunate. These crystals constitute a new anhydrous salt, which, from its constitution, I shall call *sesquisulphate of soda*. I shall give a short account of the properties and analysis of this salt.

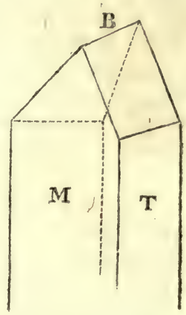
The primary form of common sulphate of soda is a doubly oblique four-sided prism with the following angles.

M on T 108°
P on T 101 30′
P on M 128 (by a common goniometer)



The acute edges of the prism are frequently truncated, making the prism six-sided. The crystals of the sesquisulphate of soda when first formed are perfectly transparent; but I have never been able to obtain them in a state fit for measurement.





I believe the primary form to be a four-sided right prism. The only forms which I have observed are represented in figures A and B. A represents an eight-sided prism terminated by a four-sided pyramid, having a rhomb P instead of its apex. The inclination of M on T was 90°. Hence I conceive M and T to be two primary faces of the original four-sided right prism. The other faces, *d, e,* are the truncation of the edges of the prism. P, I consider as the remains of the primary termination of the prism. Its position is oblique, though I have not been able to measure the angle very exactly. The four pyramidal faces, *a, b, c,* may be formed by decrements on the terminal angles of the primary prism. I have not met with any crystal exactly similar to figure B. The lateral edges of the primary prism are always truncated, but the bihedral summit represented in the figure occurs occasionally. It is obviously produced by a decrement on two of the edges of the base of the primary prism. Most commonly the two sets of decrements represented in these two figures occur together, giving the terminal truncated pyramidal figure seven faces instead of five.

The taste of the salt is very acid. When a crystal was laid upon blotting paper, the paper became moist and acid, and continued so; yet the crystal did not apparently absorb moisture; but continued hard and firm, and quite dry on the surface. Indeed the paper remained dry, if the crystals had been washed in water. There was not the least tendency to effloresce, though the salt was exposed for several days to the air during dry weather.

The specific gravity of the salt at 63° was 2·26. I determine the specific gravity of salts insoluble in alcohol by filling a narrow graduated tube with alcohol to a certain point. Into this a given weight of the crystals (say 40 grains) is put, and the bulk of this weight is determined by observing how much the surface of the alcohol is elevated. From this, knowing the weight of 100th part of a cubic inch of water at 62° to be 2·5272 grains, it is easy to deduce the specific gravity of the salt. Thus if 40 grains of a salt were equivalent to the bulk of 10-100ths of a cubic inch, or to 25·272 grains of water, its specific gravity would be 1·58.

At the temperature of 63°, 100 parts of water dissolve about 25 parts of this salt. The crystals were previously reduced to powder, and the solution was accomplished by agitating 100 parts of water with 10 parts of salt in a glass tube. As soon as the first 10 parts of the salt had dissolved, 10 more were added. Twenty parts treated in this manner dissolved completely; but when about the half of the third 10 parts was dissolved, crystals began to form in the liquid, and to subside in it. This put a stop to the process. The crystals were doubtless common sulphate of soda; for we learn from M. Gay-Lussac's experiments, that at the temperature of 64°, 100 parts of water dissolve only 16·73 parts of anhydrous sulphate of soda. But 25 parts of sesquisulphate of soda contain 19·5 parts of anhydrous sulphate. Accordingly when we separate the sulphate of soda from the liquid by crystallization, a very acid liquid remains behind.

When sesquisulphate of soda is heated on the sand-bath, it does not melt nor alter its appearance, and loses very little weight: 40 grains, when treated in this way, sustained a loss of 1·2 grain. Even when heated to redness in a platinum crucible, the loss of weight was inconsiderable. It was, therefore, mixed with a sufficient quantity of carbonate of ammonia, and heated over a spirit-lamp, till it ceased to give out any thing. Forty grains of it when thus treated lost 8·7 grains of weight. The residual 31·3 grains proved on examination to be anhydrous sulphate of soda. Composed of

Sulphuric acid	$17{\cdot}3\dot{8}$
Soda	$13{\cdot}9\dot{1}$
	31·30

Forty grains of the crystals of sesquisulphate of soda were dissolved in water, and precipitated by muriate of barytes. The sulphate of barytes obtained, after being washed, dried, and exposed to a red heat, weighed 75 grains, equivalent to 25·42 grains of sulphuric acid.

If from 25·42 we subtract $17{\cdot}3\dot{8}$, the quantity of acid in 40 grains of the salt when converted into neutral anhydrous sulphate of soda, there remain 8·03. Now 8·03 is very nearly the third part of 25·42. Thus the constituents of the salt are,

Sulphuric acid	25·42 or	7·31
Soda	13·91	4·0
Loss	0·67	0·19
	40·00	

If the loss be sulphuric acid, as is not unlikely, then the salt is anhydrous, and its constituents are,

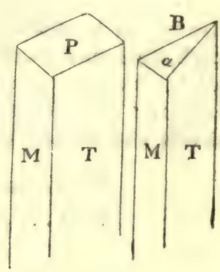
$1\frac{1}{2}$ atom sulphuric acid	7·5
1 atom soda	4·0
	11·5

As the liquid from which these crystals were obtained contained a quantity of common salt, I thought it possible that muriatic acid might have formed a constituent of the salt; but when a dilute solution of the crystals was tested with nitrate of silver, no precipitate whatever fell. This shows that the salt was free from muriatic acid. Its only constituents that I could detect are sulphuric acid and soda.

2. *Bisulphate of Soda.*

If we dissolve glauber salt in dilute sulphuric acid, and, after concentrating the solution sufficiently, set it aside, a number of crystals shoot in it, which are in transparent prisms, and at first sight bear a close resemblance to those of common sulphate of soda.

These crystals do not sensibly deliquesce when exposed to the air (at least the deliquescence is very slow). But when left upon blotting paper, that paper soon becomes moist, and continues so. The salt was four times successively transferred to dry paper; but the same moistening effect took place upon each. From this I am led to conclude, that the salt slowly attracts water from the atmosphere.



The crystals when formed in favourable circumstances were four sided prisms terminated by an oblique summit as represented in the margin, sensibly the same as the crystal of sulphate of soda; though in all probability the inclinations of the faces differed a little; but none of them were bright enough to admit of measurement by the reflective goniometer. All the crystals observed were four-sided prisms. In some the terminating plane had the form of *a*, figure B, though the prism was four-sided. The position of the face *a* was much more oblique with respect to the prism than the face P. Hence it was probably produced by a decrement on the terminating edge of the face M. I could perceive no corresponding face to *a* situated on the opposite face of the prism M′; but this was probably owing to the imperfect state of the crystal.

The taste of this salt is very acid. When a crystal is held to the flame of a candle, it melts like a piece of ice. It liquifies also when heated on the sand-bath, and remains liquid as long as we please, if the heat does not exceed 300°. When thus treated it loses scarcely any weight. 18·5 grains of the salt dried upon blotting paper were exposed to the flame of a spirit-lamp in a very small platinum crucible, and kept in a red heat as long as perceptible fumes continued to be given out. The salt first melted, then boiled, and gave out copious fumes of sulphuric acid. After some time it became a dry crust, which fused when the heat was increased, and remained in a liquid state during the continuance of the experiment. The loss of weight was 8·1 grs. and the salt still reddened vegetable blues as powerfully as ever. The object of this experiment was to ascertain whether the bisulphate of soda resembled the bisulphate of potash in the obstinacy with which it retains a certain portion of its excess of acid; for it is well known that bisulphate of potash cannot be freed from all its excess of acid by heating it over a spirit-lamp. We see that bisulphate of soda is characterized by the same property.

Specific gravity 1·800.

A quantity of water saturated with this salt at the temperature of 60° was evaporated on the sand-bath in a glass capsule till it ceased to lose weight. 226·7 grains of water thus treated left 109·07 grains of bisulphate of soda. Hence it follows that at the temperature of 60°, 100 parts of water dissolve 92·72 parts of this salt. It appears from this experiment that bisulphate of soda is more than twice as soluble in water of the temperature of of 60°, as sulphate of soda; for we learn from Gay-Lussac's table, that 100 parts of water at 60° dissolve only between 38 and 39 parts of the crystals of glauber salt.

To determine the composition of this salt, 20 grains of it were heated in a small platinum crucible over a spirit-lamp (some carbonate of ammonia having been previously mixed with it) till all the excess of acid and water were dissipated, and neutral sulphate of soda remained behind. The weight of this anhydrous and neutral sulphate was 9·7 grains composed of

Sulphuric acid	$5{\cdot}37\dot{7}$
Soda	$4{\cdot}32\dot{2}$
	9·7

Twenty grains of the salt were dissolved in water and precipitated by muriate of barytes. The sulphate of barytes obtained, after being washed, dried, and heated to redness, weighed 30·32 grains, equivalent to 10·278 grains of sulphuric acid. Now 10·278 approaches so nearly to $5{\cdot}37\dot{7} \times 2$, that we can have no doubt that the salt contains 2 atoms of sulphuric acid.

I ascertained by an experiment to be stated immediately that the reason why the sulphuric acid, obtained by means of the muriate of barytes, was not exactly double that contained in the neutral sulphate of soda was owing to a deficiency, in all probability proceeding from some additional portion of water adhering to the salt analyzed; for it is rather difficult to get the salt in the proper state of dryness to fit it for analysis.

If we consider the salt as a bisulphate, and reckon the acid twice as much as was found in the neutral sulphate from 20 grs. of the salt, then the constituents will be as follows:

Sulphuric acid..........	$10{\cdot}75\dot{5}$ or	10·0
Soda..................	$4{\cdot}32\dot{2}$	4·0
Water.................	$4{\cdot}92\dot{2}$	4·57
	20·000	

The numbers in the second column are the equivalents for the atomic weights of the constituents. 4·57 approaches so nearly to 4 atoms of water that I considered myself entitled to conclude that the salt is a compound of

2 atoms sulphuric acid..............	10·0
1 atom soda.......................	4·0
4 atoms water.....................	4·5
	18·5

To verify this supposition, 18·5 grains of the salt were dissolved in water and mixed with a solution of 26·5 grains of chloride of barium. After the sulphate of barytes had precipitated, the supernatant liquid was tested by sulphate of soda and muriate of barytes, but was not affected by either. It therefore contained no sulphuric acid nor barytes, showing that 18·5 is the true atomic weight of the salt, and consequently that its constituents have been rightly determined.

The specific gravity of anhydrous sulphate of soda......	2·640
———————— of crystallized sulphate of soda....	1·350
———————— of bisulphate of soda............	1·800
———————— of sesquisulphate of soda..........	2·260

It is a curious circumstance that in these three salts both the water of crystallization and the surplus acid (in the bisulphate and sesquisulphate) have undergone an expansion instead of contraction; for if the anhydrous sulphate and the water, constituted the crystal of sulphate of soda united without any change of volume, the specific gravity would be 1·75, instead of 1·35; so that the specific gravity of the water in the crystals is only 0·318.

If in the sesquisulphate we calculate the specific gravity of the sulphuric acid on the supposition that it combines with the anhydrous sulphate without any change of volume, we obtain 0·9. Now we are certain that the specific gravity of anhydrous sulphuric acid is at least 2. Calculated from the bisulphate, the specific gravity of sulphuric acid would be 1·01.

The specific gravity of sulphate of potash is.	2·880
——————— of bisulphate of potash	2·112

If we calculate the specific gravity of the second atom of sulphuric acid in the bisulphate, on the supposition that the anhydrous sulphate, the sulphuric acid, and the water, unite without any change of volume, we obtain 0·923.

Would it be premature to conclude from these facts that the water of crystallization in neutral salts, and the excess of acid in supersalts, undergo an increase of volume instead of a diminution? I could produce several additional examples of this increase, if this were the proper place for entering upon such an investigation.

3. *Prismatic Carbonate of Soda.*

I have been aware for some time that when the common octahedral crystals of carbonate of soda are liquefied by heat in their water of crystallization, and the solution set aside, new crystals of carbonate of soda are formed, having a different shape, and containing a smaller quantity of water. I have mentioned the fact generally in p. 267, vol. ii. of my "Attempt to establish the First Principles of Chemistry by Experiment." But my experiments had been made on too small a scale to enable me to determine the form of the crystals, or to subject the salt to an analysis sufficiently rigid to claim a place in my late work; though I had concluded from my trials that the water amounted either to seven or eight atoms, I did not succeed in determining which.

My friend Mr. Charles Tennant, of Glasgow, who manufactures carbonate of soda on a very extensive scale, and who is in the habit of continuing his processes during summer as well as winter, found himself obliged to stop the crystallizing of the salt during the very hot weather of the summer of 1825, which has just finished. Before the stop took place, several crops of crystals had been deposited in his evaporating pans quite different in their appearance from the crystals of common carbonate of soda. These crystals drew the attention of Mr. Thomas Clarke, an exceedingly ingenious chemical friend of mine, who has the management of the laboratory in Mr. Tennant's work. He collected a considerable quantity of these crystals, and subjected them to a chemical analysis; the result of which led him to conclude, that the constituents of these crystals were 1 atom carbonic acid, 1 atom soda, and between 7 and 8 atoms water.

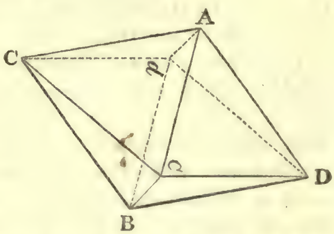
To this gentleman I was obliged for about a pound weight of very regular and pure crystals of this new salt, the properties of which I shall now describe.

The crystals are four-sided prisms terminated by four-sided pyramids, some of them an inch and a half in length, and more than one-fourth of an inch thick. They do not effloresce when exposed to the air, even in very dry weather. But my laboratory, in which this trial was made, is a damp room; for the College of Glasgow, on a ground floor of which is my laboratory, is built on a clay soil. Though I examined upwards of 100 crystals with care, I did not find one with faces sufficiently smooth to admit of measurement with the reflective goniometer. But with the common goniometer, I obtained the following measurements, which I consider as tolerable approximations.

M on P	90°
P on M′	90
M on *b*	115
M′ on *b*′	115
P on *a*	125
a on *b*, or *b*′	150

We may consider the primary form as a right rectangular prism with a rectangular base.

The common carbonate of soda is a bipyramidal octahedron, the common base of the pyramids of which is a rhomb with angles of 120° and 60°. If we suppose this form to be represented by the figure A B C D, then an idea of the common crystal of this salt will be obtained, if we suppose the acute angles A, B, of the rhomb, which forms the common base of the pyramids, to be truncated by a plane parallel to the axis C D of the octahedron. These truncations are more or less deep; but I have never met with a crystal without them, though I have examined several hundred crystals of all sizes from half an inch to eight inches in length.



It would be possible to derive the right rectangular prism from this octahedron, by supposing the four angles of the rhomb A *c* B *d* to be replaced by tangent planes; but this is unnecessary, as the two salts differ from each other in their composition; and as the pyramidal termination is quite different from the summits C, D, of the rhomboidal octahedron which constitutes the primary crystal of common carbonate of soda.

100 parts of water at the temperature of 63° dissolve 63·87 parts of these crystals. This is rather more than the quantity dissolved of common carbonate of soda at the same temperature, provided any confidence can be put in an old set of experiments on the solubility of this salt made in my laboratory, by which I find that 100 parts of water at 65° dissolve 51·03 parts of the crystals.

When heated, the salt partly liquefies, but not completely, as is the case with the octahedral carbonate. A portion remains always solid; and when the salt is cooled, imperfect crystals soon appear. This leads to the supposition that there exists a third species of crystal of carbonated soda containing still less water of crystallization. Its specific gravity is 1·51.

To determine the composition of this salt, a variety of experiments were made, the most important of which I shall briefly state.

(1.) 50 grains of the salt were dissolved in water, and neutralized with nitric acid. The solution being tested by muriate of barytes was found to contain no trace of sulphuric acid; but nitrate of silver threw down a quantity of chloride of silver, the weight of which was 1·58 grain. This is equivalent to 0·39 grain of chlorine, or 0·65 chloride of sodium; so that 100 grains of the salt contain 1·3 grain of common salt.

(2.) 50 grains of the salt lost, when exposed to a red heat in three several trials, 28·09 grains. Now common salt being anhydrous, we must deduct it from the weight of the carbonate of soda employed. This being done, we find that 49·35 grains of pure prismatic carbonate of soda lose, when heated to redness, 28·09 grs.; consequently 100 grains of the salt would lose by this treatment 56·92 grains of weight. This is the amount of the water of crystallization.

(3.) Into a small Woulf's bottle furnished with two mouths, one of which was stopped with cotton wool, were introduced through the other mouth 50 grains of the crystals of this salt. The bottle contained a quantity of concentrated and colourless nitric acid; it had been previously carefully weighed, and it was held in an oblique position when the crystals were introduced. The stopper was immediately put into the mouth of the bottle, and it was left at rest till the solution was completed. The stopper was then withdrawn, and a small sucker introduced, by which I extracted all the carbonic acid gas contained in the bottle, and allowed common air to take its place. The loss of weight sustained, owing to the escape of carbonic acid gas, was 8·47 grains. When dilute sulphuric acid was substituted for nitric, the loss of weight was always less; because a portion of the carbonic acid remains in the liquid, and is extricated when heat is applied. In two trials made in this way, the loss was 8·06 and 8·01 grains. From the experiment with the nitric acid, which was twice made, it follows that 100 grains of the prismatic carbonate of soda, if pure, would contain 17·163 grains of carbonic acid.

(4.) 50 grains of the salt were dissolved in nitric acid, and the solution evaporated to dryness. The nitrate of soda obtained weighed 35·59 grains, equivalent to 12·991 grains of soda.

50 grains were dissolved in sulphuric acid. The solution was evaporated to dryness, and heated to redness with some carbonate of ammonia to get rid of all excess of acid. The sulphate of soda weighed 29·73 grains, equivalent to 13·213 grains soda.

We cannot employ the quantity of sulphate of soda obtained for determining the quantity of soda in the carbonate, because the acid employed was the sulphuric acid of commerce, which is never quite free from lead. The soda in 50 grains of the carbonate determined by the nitrate is 12·991 grains. Hence 100 grs. of the salt contain 25·982 grains. If from this we subtract the 0·52 grain soda contained in the common salt present in the salt, there will remain 25·462 grains of soda as the constituent of 98·7 grains of pure carbonate. Hence 100 grains contain 25·797 grs.

Thus the constituents of the salt are as follows:

Carbonic acid............	17·163	or 2·661
Soda.....................	25·797	4·0
Water....................	56·920	8·824
	99·880	
Loss.....................	0·12	
	100·000	

The second column gives the atomic equivalents for the constituents. If we consider the loss as carbonic acid, which it was most likely to be, then the equivalent for the carbonic acid is 2·68, which is about $\frac{1}{40}$th less than the weight of an atom. The soda was originally in the state of sulphate. It was converted into sulphuret by heating it with combustible matter (common pit coal). The sulphuret thus formed was dissolved in water, evaporated to dryness, mixed with saw-dust, and exposed to a heat strong enough to consume the saw-dust. By this process the sulphur is disengaged, and carbonic acid takes its place. Mr. Tennant's soda usually contains a small portion of sulphate of soda, owing obviously to a little of the sulphur being acidified during the carbonating process; but the prismatic carbonate contained no sulphuric acid whatever; neither could I detect in it any sulphur, or sulphuretted hydrogen, by the most delicate tests that I could apply. It remains, therefore, somewhat doubtful, whether the small excess of soda perceptible in the preceding analysis be owing to an error in the experiments, or to the salt containing a small portion of hydrate of soda mixed or combined with the carbonate.

By the analogy 2·75 : 4 :: 17·283 : 25·136 = soda combined with carbonic acid; and by subtracting 25·136 from 25·797, we obtain 0·659 for the caustic soda that may be contained in 100 grains of the salt. This soda, supposing it present, will be in the state of a hydrate united to 0·185 water, and constituting 0·844 grain in weight. Subtracting these quantities, we have 17·283 + 25·138 : 56·735 :: 6·75 : 9·027 = the water united in the salt with 6·75 of anhydrous carbonate of soda.

I am rather disposed to admit an excess of soda, or rather the existence of a little hydrate of soda in the salt, because considerable pains were taken, after the deficiency of carbonic acid was observed, to determine the quantity of carbonic acid with every attention to accuracy, but all the experiments led to precisely the same result; and if we reckon the carbonic acid from the nitrate of soda, we obtain almost exactly the same weight of carbonic acid as by the direct method.*

There seems no doubt, from the preceding analysis, that the constituents of prismatic carbonate of soda (supposing it pure) are,

1 atom carbonic acid	2·75
1 atom soda	4·0
8 atoms water	9·0
	15·75

* It was shown that 49·35 grains of the pure salt lost by heat 28·09 grains. The remaining 21·26 grains, when decomposed by nitric acid, furnished (12·991 − 0·26) 12·731 grains of soda. Hence the 21·26 grains must have been composed of,

Carbonic acid	8·529
Soda	12·731
	21·260

Now 8·529 : 12·731 :: 4 : 2·666 = carbonic acid united to 4 soda in the carbonate.

Article IX.

Some Investigations respecting the Nature and Phenomena of Flame. By Mr. John Davies, MWS. &c. &c. Lecturer on Chemistry, &c.* (Communicated by the Author.)

The important researches of Sir H. Davy on the nature and phenomena of flame have shown that the subject combines interesting speculation with practical utility. Since the contrivance of his lamp, and the publication of his admirable investigations connected with it, the inquiry, which had been previously neglected, has excited the attention which it merits.

I have, therefore, presumed that an account of some results which I have obtained on the subject will, like my former papers, be received with indulgence by this Society.

Flame, or that species of combustion in which light is furnished, is produced by the rapid union of a combustible body with a supporter of combustion.

The cause of inflammation has never been clearly developed. It has, indeed, been ascribed to the agency of electricity; but this explanation, which is rather fanciful, is liable to the reproach which the President of this Society applied, in one of his late lectures, to certain hasty and fashionable speculations, when he remarked, that we are, in the present day, very apt to refer to the agency of electricity every thing which we do not understand.

It may, however, be expedient to offer here a brief statement of the hypothesis.

In most cases of inflammation, hydrogen is the burning body; and its combustion is effected in general by its union with oxygen. When, however, hydrogen is the only combustible present, the inflammation is always feeble; and in order to obtain a brilliant and powerful flame, carbon seems, in ordinary cases, to be indispensable.

In the instance of a common candle, the hydrogen and part of the carbon are supplied from the decomposition of the tallow; the remainder, which must be a very small quantity, arises from the wick, and the oxygen is furnished by the atmosphere. An elevation of temperature, such as is produced by a lighted taper, is required to give the first impulse to the combustion; but afterwards it goes on of itself, because the candle finds a supply of caloric in the successive quantities of heat which, it is conceived, result from the union of the two electricities given out by the gases during their combustion. This explanation, though rather gratuitous, is certainly countenanced by two striking facts: 1. The principal agents in the

* Read before the Literary and Philosophical Society of Manchester, Oct. 21, 1825.

operation are known, from other experiments, to be in opposite states of electricity; and, 2. Flame gives, under some circumstances, indications that electricity is developed during the changes which inflammation produces.

Respecting the nature of flame there are two opinions. The first is that of Mr. Sym, who has, in the eighth volume of the *Annals of Philosophy*, attempted to show, that flame is capable of being truncated, and that it presents only a superficial process of combustion. The other opinion is that of Sir H. Davy, who conceives that "flame cannot be regarded as a mere combustion at the surface of contact of the inflammable matter."

These opinions are manifestly at variance with each other. I shall request your attention to a brief examination of the subject.

Mr. Sym in his paper, the merits of which have been most unaccountably overlooked, has described some very amusing and easy experiments in illustration of his opinion. "When a wire gauze of the requisite fineness is held horizontally across the flame of a candle, the appearance is not that of repression, but of truncation. The part of the flame below the gauze has suffered no alteration in shape, size, or intensity; and the part which ought to be above has simply disappeared. In looking down, therefore, through the gauze into a flame thus truncated, we have an opportunity of examining a transverse section of it, and of thus inspecting its inside. Now it is immediately perceived that this transverse section consists of a narrow luminous ring surrounding a disk which is not luminous; and though the obscurity of the disk may at first sight be ascribed to the blackness of the wick, seen through intervening flame, it will be discovered, on more careful examination, that the wick occupies only the centre of the obscure space, which extends to some distance around it." Mr. Sym therefore contends, that "the only conclusion that remains, or rather the direct perception, is, that the lower segment of the apparent flame of a candle consists of only a thin superficial film of real flame, which has the shape of a cup, surrounding the wick, and closing in upon it below, but filled, beside, with volatilized wax."

Mr. Sym has given some interesting modifications of his experiments. What I have extracted is, however, sufficient for our present purpose. Those who desire more information on the subject would be gratified by consulting the paper referred to.

Sir H. Davy states, in page 46 of his Researches, that "the flame of combustible bodies may in all cases be considered as the combustion of an explosive mixture of inflammable gas, or vapour, with air. It cannot be regarded as a mere combustion, at the surface of contact of the inflammable matter. This fact, he adds, is proved by holding a taper, or a piece of burning phosphorus, within a large flame made by the combustion of alcohol. The flame of the taper, or of the phosphorus, will appear in the centre of the other flame, proving that there is oxygen even in its interior part."

The statements which I have here transcribed appear to be irreconcileable; I therefore thought it desirable to repeat the experiments mentioned by both, as the only way to arrive at a fair decision with respect to either.

I have found the experiments of Mr. Sym, which are so simple as almost to preclude the possibility of mistake, to correspond precisely with what he has stated. A piece of wire gauze, applied in the manner already described, showed a thin film of flame enclosing a mass of opaque carbonaceous matter. I then varied my experiments so as to submit the fact to a careful examination. The result was invariably in accordance with Mr. Sym's statements.

I have also repeated, under every variety of circumstances which has occurred to me, the experiments of Sir H. Davy.

By enlarging the wick of a common candle, and introducing into the flame small pieces of phosphorus and of sulphur, on the point of a needle, I soon found that the interior of ordinary flame would not support combustion.

Similar experiments were made in the flame of a spirit lamp, and the same results were obtained. A small portion of phosphorus, having accidentally attached itself to the wick of the lamp, remained there for a very considerable time, and was not burnt until it was brought to the edge of the flame.

Influenced by the high authority of Sir H. Davy, I have been anxious to conduct my experiments in such a way as to avoid, as far as I have been able, the possibility of exception.

A piece of phosphorus was placed upon a small wooden stand in a Wedgwood dish; spirit of wine was then poured into the dish in such a manner that it did not reach the phosphorus. The spirit of wine was now lighted, and its flame completely enveloped the combustible body. In the course of a few seconds the phosphorus became fluid, and remained in that state upon the stand; and never in a single instance inflamed, until the alcohol was consumed, or its flame extinguished; though, in several instances, the spirit of wine continued to burn for three or four minutes. The phosphorus always burst into a vigorous flame when the spirit of wine was extinguished; nor was the combustible power of the phosphorus, as far as I could judge, in the least impaired.

When the flame of the spirit of wine was blown upon, so that the edge of it came in contact with the phosphorus, the phosphorus immediately burst into a flame; but the flame was instantly extinguished, and the boiling resumed, as soon as the flame of the alcohol was restored to its natural position, so as to enclose the phosphorus. It would hence appear, not only that the interior of flame will not support combustion, but that it contains no oxygen.

This conclusion is further countenanced by the following addition to the experiment. The extremity of a common blowpipe was introduced into the flame of alcohol: it was found that every time the phosphorus was blown upon, and in that way furnished with oxygen, it instantly inflamed; but it was again extinguished as soon as its supply of oxygen was exhausted. In this manner the phosphorus, while surrounded by the flame of alcohol, was itself repeatedly inflamed and extinguished in the course of two or three minutes.

That the interior of the flame of alcohol is incapable of supporting combustion, and that it consequently contains no oxygen, is also shown by the following experiment. While a piece of phosphorus, about the size of a pea, was in the centre of a flame of alcohol, I repeatedly touched it with a red hot wire; every time the wire came in contact with the combustible body, there was a slight flash, often hardly perceptible; but the phosphorus never entered into combustion until the flame of the spirit of wine was extinguished, or blown aside in such a manner that the mere edge of the flame, as already mentioned, should touch the phosphorus.

I confess that I had some hesitation as to the correctness of my opinions, upon the first performance of this experiment; for in this case, the combustion of the phosphorus, though feeble and transient, seems to indicate the presence of oxygen. I am, however, induced to believe, that the oxygen which occasioned the combustion was supplied by the oxide of iron formed by heating the wire red hot. If the quantity of oxygen obtained in this way be thought small, it should be recollected that only a very small quantity is required to produce the effect.

I have tried several combustible bodies besides phosphorus, and the result, as far as respects the general principle, has been always the same. A wax taper, about half an inch long, was lighted and placed upright in a small cup, and surrounded by alcohol; as soon as the alcohol was lighted, its flame enveloped the taper, carrying away the flame of the latter in rather a singular manner; nor was the extinguished taper apparently affected during the operation by the surrounding flame. It sometimes happened that when the flame of the alcohol was burnt out, the flame of the taper would, like that of the phosphorus, be spontaneously rekindled.

It is hardly necessary to remark, that the result of the experiments which I have described, and of others which I might have stated, appears to be at variance with Sir H. Davy's opinion on the subject, while it is, at the same time, in accordance with the views of Mr. Sym.

From the nature of flame, as explained in this paper, we are enabled to assign a cause for many of the phenomena of combustion, some of which could not be easily accounted for on any other principle.

The great power of the Argand burner is owing, as is well known, to the stream of air which passes up the flame. This stream of air nearly doubles the surface of the flame; and, as upon the principle just stated, the intensity of the flame increases, *cæteris paribus,* in the same ratio, the effect is only that which might have been expected.

It shows us why combustion is comparatively feeble in rarified air: for in this case there is a deficiency in the supply of the oxygen, and the combustion at the surface of the flame must be accordingly diminished. We see, too, a reason for the vigorous combustion which is occasioned by introducing the burning body into oxygen gas.

Some of the researches of Sir H. Davy might, at first view, appear to militate against the principle which is here applied; since he found that, in an atmosphere so much rarified as to extinguish a small flame of hydrogen gas, a large flame of the same material might still be supported. This objection, though plausible, may, I think, be easily obviated. The languid action of the small flame does not enable it to make use of the scanty supply of oxygen; but the increased energy of the larger flame, presents, by its greater heat and surface, an augmented attractive force for the oxygen, which it seizes with avidity as long as any remains.

When candles and lamps produce, while burning, a quantity of smoke, the circumstance is owing to imperfect combustion arising from a deficiency of oxygen. If the lamp or candle in this state be put into a vessel containing oxygen gas, the smoke will, for obvious reasons, be no longer afforded.

It is found that gas burners are, to a certain point, capable of giving a greater quantity of light, in proportion to the number of holes made for the emission of the gas, although beyond that point the illuminating power is diminished. The fact may, I conceive, be explained upon the principles which I have attempted to establish. By increasing to a certain extent the number of perforations, we augment the external surface of the flame; and, therefore, according to the views of Mr. Sym, we obtain a greater quantity of light: but if we exceed that number of perforations, the flames, which were before distinct, become united, and form only one flame, the surface of which must obviously be less than it was in the other case; and the quantity of light will accordingly be, by theory, what we find it actually is in fact. It ought to be observed, however, that, as my friend Mr. Dalton has determined, the diameter of these perforations must not be diminished beyond a certain extent, otherwise the flame will not be so luminous as it is when they are of the ordinary size.

After the explanations which have been given of the nature of flame, it seems easy to assign a reason for the amazing power of the oxyhydrogen blowpipe. In this instance, the combustible body, or hydrogen, is so completely supplied with the supporter of combustion or oxygen, that the flame, instead of having, as in ordinary cases, only a superficial film of inflammation, is a solid mass of fire. The cause of the difference, therefore, between common flame and that of the oxyhydrogen blowpipe is evident.

The common blowpipe may also be explained upon the same principle. The power of the flame is increased by the introduction of a quantity of oxygen, which affords a thicker covering of combustion. Hence the reason that the mouth blowpipe is inferior to one of common air; since the air blown through it contains a less proportion of oxygen than is contained in the same bulk of the atmosphere.

I shall not lengthen this paper by adverting to any other topics that may be suggested by a consideration of the principle which it has been my object, on this occasion, to illustrate and confirm.

Article X.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1825. Part I.

Having already reprinted one of the papers contained in this part of the Philosophical Transactions, the titles of which are given below, in the *Annals* for August last, and offered some account of the contents of four others in our reports of the Proceedings of the Royal Society, but one of the remaining communications, that by Mr. Christie, which begins the volume, will now require to be noticed at any length.

1. *On the Effects of Temperature on the Intensity of Magnetic Forces; and on the Diurnal Variation of the Terrestrial Magnetic Intensity.* By S. H. Christie, Esq. MA. of the Royal Military Academy: communicated by the President.

Mr. Christie has already distinguished himself among the labourers in the rich field of philosophical research which Magnetism has for some years past afforded, in consequence principally of Professor Oersted's discovery of its relations to electricity on the one hand, and of the various investigations in pure magnetism carried on by Prof. Hansteen and Mr. Barlow on the other; and the paper now before us, another important contribution to our knowledge of this science, will still further tend to establish his reputation as a natural philosopher.—It commences as follows:—

"In the paper on the diurnal deviations of the horizontal needle when under the influence of magnets, which the President did me the honour to present, I stated that these deviations were partly the effects of changes that took place in the temperature of the magnets; and that although the conclusions which I drew from the observations respecting the increase and decrease of the terrestrial magnetic forces during the day would not be materially affected, it was my intention to undertake a series of experiments for the purpose of determining the precise effects of changes of temperature in the magnets, so as to be able to free the observations entirely from such effects.

"These experiments were immediately made: but I was induced from some effects which I observed, to carry them to a greater extent, in the scale of temperature, than was necessary for the object which I had at first in view. In consequence of this, and the length of the calculations into which I have been obliged to enter, the accomplishment of my purpose was delayed for a considerable time, and continued indisposition has since prevented me, until now, completing the arrangement of the tables of results.

"In the present paper, I propose to detail the experiments which I made in order to determine the effect of changes of temperature on the forces of the magnets, to the extent to which I observed their temperature to vary, during my observations on the diurnal changes in the direction of the needle, when under their influence; to apply the results which I obtained to the correction of the observations themselves, thereby accounting for the apparent anomalies noticed by Mr. Barlow and myself, in the observations made in doors and in the open air; and by means of these corrected observations, to point out the diurnal variations in the terrestrial magnetic intensity."

Having found it impracticable to determine purely from observation the portion of the arc of deviation due to the changes which he noticed in the temperature of the magnets, Mr. Christie was, therefore, under the necessity of having recourse to theory; and he adopted the simplest, and that which is most generally received, viz. that the forces which two magnets exert upon one another may be referred to two centres or poles in each, near their respective ends; and that for either pole in one of the magnets, one pole of the other magnet is urged towards it, and the other from it, by forces varying inversely as the squares of their respective distances from that pole.

After this statement he proceeds to explain and exemplify the application of the theory to the investigation detailed in the paper; and then, describing the compass and magnets made use of (the verbal description being illustrated by an engraving), he gives the subjoined account of the mode of experimenting adopted.

"A meridian line being drawn on a firm table, standing on a stone floor, the compass was accurately adjusted on it, so that the needle pointed to zero on the graduated circle. The magnets were fixed at the bottoms of earthen pans, secured in such a way to rectangular pieces of board that their positions could not be accidentally changed, and projecting from these boards were small pieces of brass, on each of which a line was drawn to indicate the position of the axis of the magnet; the horizontal distance of the edge of each of the projections nearest to the needle from the corresponding end of the magnet within the pan, was exactly three inches; I could therefore, in any instance, determine very accurately the distance of the centre of the magnet from that of the needle. The pans were placed on the table, so that the indexes on the pieces of brass coincided with the meridian line. Water was now poured into the pans, and the temperature of the magnets was varied by varying the temperature of the water. The temperature of each magnet was ascertained by a thermometer placed in the water, with its bulb resting on that pole of the magnet which was nearest to the centre of the needle. In my first observations I however made use of only one thermometer, which was moved, during them, from one magnet to the other."

* * * * * * *

"The observations contained in the tables were made thus: I first noted the time .... and then the temperature of the north magnet; after which I placed the thermometer on the pole of the south magnet; I next observed the westerly point, at which the needle was held in equilibrio by the terrestrial forces and those of the magnets, slightly agitating the needle, that it might the more readily assume the true position; from this it was led, by means of a very small and weak magnet, held on the outside of the compass-box, towards the easterly point of equilibrium, which was observed in the same manner; and from this it was led in the same way towards the southerly point. .... After these observations of the points of equilibrium, the temperature of the south magnet being observed, the time .... at which the observations concluded, was noted. The temperature of the water in the pans was now increased or diminished, according to circumstances, by the addition of other water, and the pans covered over, to prevent any rapid changes of temperature during the observations: after allowing a short time for the magnets to acquire the temperature of the water, the observations were repeated. .... The scale made use of for the temperature was in all cases that of Fahrenheit."

From the results of the observations given in the tables described in the paragraph last quoted, we extract the following:

" *Tables of the Magnetic Intensities corresponding to different Temperatures of the Magnets. 6th June,* 1823.

Mean temperature of the magnets.	Diff. of temp. in successive observations.	Magnetic intensity or values of $\frac{F}{M}$.	Variation of $\frac{F}{M}$ for 1° Fahr. or $\Delta . \frac{F}{M}$.
62·05°		212·5620	
	− 3·00		0·1268
59·05		212·9423	
	+ 18·60		0·1247
77·65		210·6228	
	− 3·65		0·1004
74·00		210·9892	
	− 3·35		0·1279
70·65		211·4178	
	− 3·50		0·1193
67·15		211·8353	
	− 3·35		0·1138
63·80		212·2167	
	− 1·75		0·1413
62·05		212·4640	

7th June, 1823.

Mean temperature of the magnets.	Diff. of temp. in successive observations.	Magnetic intensity.	Variation of $\frac{F}{M}$ for 1° Fah. or $\Delta . \frac{F}{M}$.
57·00°		212·9803	
	+ 10·00		0·1059
67·00		211·9209	
	+ 3·85		0·1377
70·85		211·3907	
	+ 4·15		0·1219
75·00		210·8848	
	− 14·50		0·1148
60·50		212·5489	

13*th June,* 1823.

Mean temperature of the magnets.	Diff. of temp. in successive observations.	Magnetic intensity or value of $\frac{F}{M}$.	Variation of $\frac{F}{M}$ for 1° Fah. or $\Delta . \frac{F}{M}$.
62·80°		218·5687	
	− 1·72		0·0920
61·08		218·7269	
	+ 9·97		0·1430
71·05		217·3014	
	− 5·10		0·1182
65·95		217·9040	

Some anomalies observed by Mr. Barlow between the daily changes in the direction of a needle when placed in the house and when in the open air,* which Mr. Christie also noticed, and stated, in a former paper, his opinion that they had arisen from the difference in the changes of temperature in the magnets in the two situations, are next investigated in the memoir before us; observations on the temperature of the magnets having been made in the open air, corresponding to those made in doors.

We select the subjoined tables from among the results of this branch of Mr. Christie's inquiry:

17th and 18th June, 1823.

Mean temperture of the magnets.	Diff. of temp. in successive observations.	Magnetic intensity, or value of $\frac{F}{M}$.	Variation of $\frac{F}{M}$ for 1° Fah. or $\Delta \cdot \frac{F}{M}$.
49·30°		224·0981	
60·25	+10·95	222·8171	0·1179
68·25	+ 8·00	221·7046	0·1391
74·60	+ 6·35	220·7198	0·1551
61·75	−12·85	222·3967	0·1305
73·80	+12·05	220·8778	0·1260
55·58		222·6462	
66·00	+10·42	221·2655	0·1315
73·60	+ 7·60	220·1532	0·1461
56·90	−16·70	222·5145	0·1314

18th, 19th, 20th, and 22d June, 1823.

Mean temperature of the magnets.	Diff. of temp. in successive observations.	Magnetic intensity or value of $\frac{F}{M}$.	Variation of $\frac{F}{M}$ for 1° Fah. or $\Delta \cdot \frac{F}{M}$.
55·40°		222·8859	
73 80	+18·40	220·6103	0·1237
55·40	−18·40	222·9113	0·1251
55·45		222·5640	
64·30	+ 8·85	221·4144	0·1299
73·90	+ 9·60	220·0549	0·1415
64·28	− 9·62	221·3962	0·1394
55·55	− 9·73	222·4610	0·1220
55·15		222·7217	
66·75	+11·60	221·3013	0·1224
50·65		222·9322	
55·80	+ 5·15	222·3848	0·1063
51·35	− 4·45	222·8660	0·1059
57·20	+ 5·85	222·2080	0·1125
54·08		222·5974	
51·35	− 2·73	222·9263	0·1203
55·94	+ 4·59	222·4377	0·1064

* These anomalies are described by Mr. B. in his paper on the daily variation of the horizontal and dipping needles under a reduced directive power, of which an abstract was given in the *Annals* for March, 1824.

A double series of observations on the diurnal changes in the positions of the points of equilibrium at which a magnetic needle was retained by the joint action of terrestrial magnetism and of two bar magnets, having their axes horizontal and in the magnetic meridian, and their centres at the distance 21·21 inches from the centre of the needle, afford by correction and calculation the following

Tables of the mean Terrestrial Magnetic Intensities at different Hours during the Day.

1. From observations made within doors.

Time of observation.	Mean of the observations of May 22, 23, 24, 25, 26.		Mean of the observations of May 27, 28, 29, 30, 31.		Mean of the two sets.
	Azimuth of the points of equilibrium.	Terrestrial magnetic intensity.	Azimuth of the points of equilibrium.	Terrestrial magnetic intensity.	Terrestrial magnetic intensity.
6h 00m	81° 27·3′	1·00175	81° 56·9′	1·00170	1·00173
7 30	82 19·9	1·00100	82 27·4	1·00128	1·00114
9 00	83 13·9	1·00031	83 33·6	1·00046	1·00039
10 30	83 40·5	1·00000	84 16·2	1·00000	1·00000
Noon.	82 22·8	1·00096	83 40·3	1·00038	1·00067
1 30	81 43·5	1·00151	82 39·5	1·00112	1·00132
3 00	81 29·1	1·00173	81 57·2	1·00170	1·00172
4 30	81 11·5	1·00199	82 10·8	1·00151	1·00175
6 00	81 17·7	1·00190	81 41·7	1·00192	1·00191
7 30	81 00·9	1·00216	81 20·5	1·00224	1·00220
9 30	80 52·6	1·00229	81 14·5	1·00233	1·00231
11 20			81 19·7	1·00225	1·00225

"From the mean obtained here, it appears that the terrestrial magnetic intensity was the least between 10 and 11 o'clock in the morning, the time, nearly, when the sun was on the magnetic meridian; that it increased from this time until between 9 and 10 o'clock in the evening; after which it decreased, and continued decreasing during the morning until the time of the minimum."

2. From observations made in the open air.

Time of observation.	Mean of the observations of June 20, 21, 22.	
	Azimuth of the point of equilibrium.	Terrestrial magnetic intensity.
6h 00m	79 30·0	1·00112
7 30	79 51·7	1·00061
9 00	80 24·7	1·00028
10 30	80 42·2	1·00000
Noon	80 32·7	1·00015
1 30	79 23·0	1·00134
3 00	78 53·2	1·00188
4 30	78 34·8	1·00223
6 00	78 20·3	1·00251
7 30	78 26·5	1·00239
9 00	78 42·3	1·00209

"From these it appears, that the minimum intensity happened nearly at the time the sun passed the magnetic meridian, and rather later than in May, which was also the case with the time of the sun's passage over the meridian :* the intensity increased until about six o'clock in the afternoon, after which time it appears to have decreased during the evening, and to have been decreasing from an early hour in the morning.

"The general agreement of these intensities with those deduced from the observations made in doors, is as near as could be expected, considering that an interval of twenty days had elapsed between the two sets of observations. From this, and the agreement in the manner in which the westerly and easterly points of equilibrium approach and recede from the north in the two cases, which I have before pointed out, we may conclude, that there is nothing anomalous in the action which takes place on the needle under the different circumstances of its being placed in doors or in the open air; and that the apparent

"* The diurnal variation, both in the direction of the needle and in the magnetic intensity, appears to have a reference to the position of the sun with regard to the magnetic meridian; it is therefore probable, that the sun is the principal cause of both these phænomena. The circumstance of the situation of the magnetic pole in what appears to be, independent of elevation, the coldest region of the globe, supported as it is by the fact of a diminution of temperature causing an increase of magnetic intensity, would lead us to infer, that the effect produced by the sun is principally to be attributed to the heat developed by it; but should any periodical effects, corresponding to the time of the sun's rotation about its axis, be observable in the diurnal variation, we must suppose that the sun, like the earth, is endued with magnetism, and look for a cause of this magnetism, common to all the planets. Being engaged more than two years ago in making some experiments on the effects produced on the needle by unpolarized iron, I discovered that a peculiar polarity was imparted to the iron by simply making it revolve about an axis; and this naturally suggested the question to me, whether the magnetism of the earth, and consequently, that of the other planets and the sun, might not be owing to their rotation? From the effects which I have observed to be produced on iron by its rotation, it appears probable, if the magnetism of these bodies be not caused by their rotation, that at least the effects will be modified by, and, to a certain extent, dependent on such rotation. Since first observing the fact, that simple rotation will cause a peculiar polarity, if I may be allowed the expression, in iron, I have made a great variety of experiments on the subject, which have enabled me to trace the laws according to which this polarity in the iron affects a magnetic needle, independently of the effect produced by the mass. It would lead me to too great a length here to state the several effects that are produced by the rotation of iron, or the laws which govern them; but I will briefly mention one. Let us imagine a plane to pass through the centre of a horizontal needle, at right angles to the meridian, and making an angle with the horizon equal to the dip; then, if the plane of a circular plate of iron coincide with this plane, and the plate be fixed on an axis passing through its centre at right angles to its plane, so that it can be made to revolve in its own plane, the direction of the needle will be different, according as the several points of the plate are brought into any particular position by making it revolve in one direction or the opposite, excepting in four positions of the centre of the plate. If the centre of the plate be successively placed to the east or west of the centre of the needle in the same horizontal line, and over the needle in the plane of its meridian, then the deviation of the needle due to the rotation of the plate will be in contrary directions in the two cases, the plate revolving in the same direction in both. These and other peculiar effects arise entirely from the rotation of the iron, and are not produced by any friction on the axis. As the effects are not very considerable, to render them conspicuous it is necessary to make use of a plate eighteen inches in diameter, and to have its centre within sixteen inches of that of the needle. If the needle is under the influence of magnets, as in the foregoing observations, the effects produced by the rotation of the plate are considerable."

anomaly in the directions of the needle in the two cases, which was observed by Mr. Barlow and myself, arose from the cause which I have assigned for it in my former paper; namely, the difference in the changes of temperature in the magnets when in doors and when in the open air.

"The diurnal changes in the terrestrial magnetic intensity have been determined by Professor Hansteen, by means of the vibrations of a needle delicately suspended. From these observations it appears, that in general the time of minimum intensity was between ten and eleven o'clock in the morning; that the maximum happened between four and seven for the month of May, 1820, and about seven o'clock in the evening for the month of June. The intensity which, in these observations, is taken as unity, is that deduced from an observation made during an aurora borealis; but for the purpose of comparison, I have, for the months of May and June, taken the intensity deduced from his observations at $10^h\ 30^m$ in the morning as unity, reduced the intensities, which he gives for other times in the day, to this standard, and placed them in the following table, with the corresponding intensities deduced from my own observations.

Intensity deduced from Hansteen's observations in 1820.			Intensity deduced from the preceding observations in 1823.		
Time.	May.	June.	Time.	May.	June.
$8^h\ 00^m$ a. m.	1·00034	1·00010	$7^h\ 30^m$ a. m.	1·00114	1·00061
10 30	1·00000	1·00000	10 30	1·00000	1·00000
4 00 p. m.	1·00299	1·00251	4 30 p. m.	1·00175	1·00223
7 00	1·00294	1·00302	7 30	1·00220	1·00239
10 30	1·00191	1·00267	9 30	1·00231	1·00209

"The principal difference to be observed in the nature of the changes of intensity during the day, in the two cases, is, that from my observations, the intensity appears to decrease more rapidly in the morning, and increase more slowly in the afternoon, than it does from those of Professor Hansteen; but the general character of these changes is as nearly the same as we can expect from methods so different, at different times, and at places where both the variation and dip of the needle are different. My object however was, to point out what might be deduced from a series of such observations as I have detailed, rather than to compare the results deduced from them with those obtained by others, for which purpose it would have been necessary to have continued them for a greater length of time.

"We have seen that with the magnets I made use of, their intensity being nearly 218 M, at the temperature 60°, a change in their temperature of 1° would cause a change of intensity of

0·123 M; or taking the intensity of the magnets 1, for each degree of increase in temperature we should have a decrease of intensity of 0·000564. Now if the same, or nearly the same, take place with all magnets, it is evidently necessary, in all cases where the terrestrial magnetic intensity is to be deduced from the vibrations of a needle, that great care should be taken to make the observations at the same temperature; or, the precise effect of change of temperature having been previously ascertained, to correct the observations according to the difference of the temperatures at which they were made. I am not aware that any one has yet attempted to make such a correction; but it is manifest from the experiments I have described, that it is indispensible, in order to deduce correct results from the times of vibration of a needle in different parts of the earth, where the temperatures at which the observations are made are almost necessarily different, that these temperatures should be registered, and the times of vibration reduced to a standard of temperature. It appears to me, that the effects will be the most sensible in large and powerful needles; and consequently, in making use of such, the reduction for a variation of temperature will be most necessary. There would be no difficulty in this reduction, if we could give in terms of the intensity of any magnet the increment or decrement of intensity corresponding to a certain decrement or increment of temperature at all temperatures. To determine this accurately would however require a great variety of experiments to be made with magnets of very different intensities; but as I have not made these, I must content myself for the present with pointing out some of the facts which I have ascertained from more extended experiments than those I have already given, reserving the detail of these experiments for another opportunity, should they be deemed of sufficient interest.

"These experiments were made with a balance of torsion, the needle being suspended by a brass wire $\frac{1}{450}$ inch in diameter: by them I ascertained the following facts:

"1. Commencing with a temperature − 3° Fahrenheit, up to a temperature 127°, as the temperature of the magnets increased, their intensity decreased. Owing to the almost total absence of snow during the winter, I was unable to reduce lower the temperature of the large magnets which I made use of; but from an experiment I made at the Royal Institution, in conjunction with Mr. Faraday, in which a small magnet, enveloped in lint well moistened with sulphuret of carbon, was placed on the edges of a basin containing sulphuric acid, under the receiver of an air pump, I found that the intensity of the magnet increased to the lowest point to which the temperature was reduced, and that the intensity decreased on the admission of air into the receiver, and consequent increase of temperature in the magnet. This is in direct contradiction to the notion which has been entertained of destroying the magnetism of the needle by the application of intense cold.

"2. With a certain increment of temperature, the decrement of intensity is not constant at all temperatures, but increases as the temperature increases.

"3. From a temperature of about 80° the intensity decreases very rapidly as the temperature increases: so that, if up to this temperature, the differences of the decrements are nearly constant, to ascertain which requires a precision in the experiments that perhaps their nature does not admit of, beyond this temperature, the differences of the decrements also increase.

"4. Beyond the temperature of 100°, a portion of the power of the magnet is permanently destroyed.

"5. On a change of temperature, the most considerable portion of the effect, on the intensity of the magnet, is produced instantaneously; showing that the magnetic power resides on or very near the surface. This is more particularly observable when the temperature of the magnet is increased, little change of intensity taking place after the first effect is produced; on the contrary, when the temperature of the magnet is diminished, although nearly the whole effect is produced instantly, yet the magnet appears to continue to gain a small power for some time.

"6. The effects produced on unpolarized iron by changes of temperature are directly the reverse of those produced on a magnet; an increase of temperature causing an increase in the magnetic power of the iron, the limits between which I observed being 50° and 100°. That the effect on iron of an increase of temperature should be the reverse of that produced on a magnet, is, I think, a strong argument against the hypothesis, that the action of iron upon the needle arises from the *polarity* which is communicated to it from the earth.

"It may be objected to the method which I have adopted for determining the diurnal changes in the terrestrial magnetic intensity, that, after the observations have been made, they require a correction for temperature, which can only be determined by experiments previously made on the magnets and needle employed. The same objection may, however, be made against the method of determining the intensity by the vibrations of a needle. As such a correction has not in the latter case been hitherto applied, the results which have been obtained relative either to the diurnal changes of intensity, or the intensities in different parts of the earth, by means of observations on the vibrations of a needle, will be so far incorrect as the needle may happen to have been affected by differences in the temperature. The method I have described, however, possesses advantages over the other: a very considerable one is, that whatever effects are produced may easily be observed with considerable precision, the time required for each observation being not more than five minutes; another is, that, the magnets being immersed in water, as far as regards them, we may command the temperature at which the observations are to be made, and thus limit the correction for temperature to a very small quantity; and it possesses another decided advantage, that whatever are the effects produced on the needle by atmospheric changes, they are, by means of it, rendered immediately visible, and can be observed as they occur."

II. *The Croonian Lecture. On the Existence of Nerves in the Placenta.* By Sir E. Home, Bart. VPRS. (See *Annals* for January last.)

III. *Observations on the Changes the Ovum of the Frog undergoes during the Formation of the Tadpole.* By the same Author.

"In the year 1822," Sir Everard observes, "I laid before the Society a series of observations on the progress of the formation of the chick in the egg of the pullet, illustrated by drawings from the pencil of Mr. Bauer, showing that in the ova of hot-blooded animals, the first parts formed are the brain and spinal marrow. I have now brought forward a similar series on the progress of organization in the ova of cold-blooded animals, illustrated in the same manner by microscopical drawings made by the same hand."

By comparing together the first rudiments of organization in the ova of these very distinct classes of animals, he shows that in both the same general principle is employed in the formation of the embryo, although the respective ova are not composed of similar parts. Those of the frog, which have been selected for this investigation, being found to have no yelk.

IV. *A general Method of calculating the Angles made by any Planes of Crystals, and the Laws according to which they are formed.* By the Rev. W. Whewell, FRS. Fellow of Trinity College, Cambridge.

"It has been usual," Mr. Whewell states in the commencement of this paper, "to calculate the angles of crystals and their laws of decrement from one another, by methods which were different as the figure was differently related to its nucleus; which were consequently incapable of any general expression or investigation, and which had no connexion with the notation by which the planes of the crystals were sometimes expressed. And the notation which has hitherto been employed, besides being merely a mode of registering the laws of decrement, without leading to any consequences, is in itself very inelegant and imperfect. The different modes of decrement are expressed by means of different arbitrary symbols; and these are combined in a manner which in some cases, as for instance in that of intermediary decrements, is quite devoid both of simplicity and of uniformity, and indeed, it may be added, of precision. The object of the present paper is to propose a system which seems exempt from these inconveniences, and adapted to reduce the mathematical portion of crystallography to a small number of simple formulæ of universal application. According to the method here explained, each plane of a crystal is represented by a symbol indicative of the laws from which it results; the symbol, by varying the indices only, may be made to represent any law whatever: and by means of these indices, and of the primary angles of the substance, we obtain a general formula, expressing the dihedral angle contained between any *one plane* resulting from crystalline laws, and any *other*. In the same manner we can find the angle contained between any *two edges* of the derived crystal. Conversely, knowing the plane or dihedral angles of any crystal, and its primary form, we can by a direct and general process deduce the laws of decrement according to which it is constituted. The same formulæ are capable of being applied to the investigation of a great variety of properties of crystals of various kinds, as will be shown in the sequel. We shall begin with the consideration of the rhomboid, and the figures deduced from it; and we shall afterwards proceed to other primary forms."

We cannot transfer to our pages the formulæ, occupying ten sections, in which the author proceeds to develope his method; and must, therefore, refer the student in crystallography to the Transactions for them.

Article V. is Dr. Roget's *Explanation of an Optical Deception*, already given in the *Annals* for August last.

VI. *On a new Photometer, with its Application to determine the relative Intensities of Artificial Light, &c.* By William Ritchie, AM. Rector of the Academy at Tain: communicated by the President.

The accuracy of Mr. Ritchie's photometer is founded, he states, "on the axiom, that equal volumes of air are equally expanded by equal quantities of light, converted into heat by absorption by black surfaces: and also on the well established principle that the quantity of light diminishes as the square of the distance of the luminous source from the object on which it is received.

"The instrument [of which a plate is given] consists of two cylinders of planished tin plate from 2 to 10 or 12 inches in diameter, and from a quarter of an inch to an inch deep. One end of each cylinder is inclosed by a circular plate of the same metal soldered completely air tight, the other ends being shut up by circular plates of the finest and thickest plate glass, made perfectly air tight. Half way between the plates of glass and the ends of the cylinders, there is a circular piece of black bibulous paper for the purpose of absorbing the light which permeates the glass, and instantly converting it into heat.

"The two cylinders are connected by small pieces of thermometer-tubes which keep them steady with their faces parallel to each other, but turned in opposite directions, and also serve to make the insulation as complete as possible. The chambers are then connected by a small bent tube in the form of the letter U, having small bulbs near its upper extremities, and containing a little sulphuric acid, tinged with carmine. The instrument is supported upon a pedestal, having a vertical opening through the stem to allow the glass tube to pass along it, and thus secure it from accidents.

"The accuracy of the instrument evidently depends upon the perfect equality of its two opposite ends. To ascertain if it be accurately constructed, place it between two steady flames, and move it nearer the one or the other till the liquid in the tube remains stationary, at the division of the scale at which it formerly stood. Turn it half round without altering its distances from the flames, and if the liquid remains stationary at the same division, the instrument is correct. To show the extreme delicacy of the instrument, place it opposite a single candle, and it will be sensibly affected at the distance of 10, 20, or 30 feet, provided it be of sufficient diameter, whilst it will not be sensibly acted upon at the same distance by a mass of heated iron affording twenty times the quantity of heat. In order to cut off effectually the influence of mere radiant heat, I sometimes use screens composed of two plates of glass, placed parallel to each other, with a quantity of water interposed.

"Place the instrument between any number of steady lights whose intensities are known, as for example, between four wax candles opposite one end, and one candle opposite the other, and move the photometer till the fluid remain stationary at the division where it formerly stood, and it will be found that the distances are directly as the square roots of the number of candles; or in other words, that the intensities of the lights will be inversely as the squares of the distances. If gas lights be employed, having burners capable of consuming known quantities of gas in equal times, and the photometer be placed between them, so that the effect upon the air in each chamber shall be the same; it will be found that the quantities of gas consumed by each will be exactly proportional to the squares of the distances of their respective flames from the ends of the photometer."

VII. *The Description of a Floating Collimator.* By Capt. Henry Kater, FRS. (See *Annals* for Feb. 1825, p. 143.)

VIII. *Notice on the Iguanodon, a newly discovered Fossil Reptile, from the Sandstone of Tilgate Forest, in Sussex.* By Gideon Mantell, FL. & GS.: in a Letter to Davies Gilbert, Esq. MP. VPRS. (See *Annals* for March, 1825, p. 228.)

IX. *An Experimental Inquiry into the Nature of the Radiant Heating Effects from Terrestrial Sources.* By Baden Powell, MA. FRS. (See *Annals* for March, 1825, p. 224; and for May, 1825, p. 360.) E. W. B.

Article XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The Royal Society resumed its sittings on the 17th of Nov. when the name of Major-Gen. Sir Benjamin D'Urban, Lieut.-Governor of Demerara, who had been elected a Fellow in the course of the last session, was ordered to be inserted in its printed lists; and the following papers were read:—

On the Changes that have taken place in some ancient Alloys of Copper, in a letter from John Davy, MD. FRS. to Sir Humphry Davy, Bart. Pres. RS. In this letter, Dr. Davy, who is pursuing a train of scientific researches in the Mediterranean, describes the effects which time and the elements have produced on various Grecian antiquities. The first he examined was a helmet of the antique form found in a shallow part of the sea between the citadel of Corfu and the village of Castrartis, which was partly covered with shells and with an incrustation of carbonate of lime. Its entire surface, as well where invested with these bodies as where they were absent, presented a mottled appearance of green, white, and red. The green portions consisted of the submuriate and the carbonate of copper, the white chiefly of oxide of tin, and the red of protoxide of copper in octahedral crystals, mingled with octahedrons of pure metallic copper. Beneath these substances the metal was quite bright, and it was found by analysis to consist of copper, and 18·5 per cent. of tin. A nail of a similar alloy from a tomb at Ithaca, and a mirror from a tomb at Samos, in Cephalonia, presented the same appearances, but in less distinct crystallization: the mirror was composed of copper alloyed with about six per cent. of tin, and minute portions of arsenic and zinc. A variety of ancient coins, from the cabinet of a celebrated collector at Santa Maura, presented similar appearances, and afforded corresponding results; the white incrustations being oxide of tin, the green consisting of carbonate and submuriate of copper, and the red of the protoxide of the same metal; some having a dingy appearance arising from the presence of black oxide of copper mingled with portions of the protoxide. Dr. Davy was unable to detect any relation between the composition of the respective coins and their state of preservation, the variations in this respect which they presented appearing to arise rather from the circumstances under which they had been exposed to the mineralizing agents. In conclusion, Dr. Davy observed, that as the substance from which these crystalline compounds had been produced could not be imagined to have been in solution, their formation must be referred to an intimate motion of its particles, effected by the conjoint agency of chemical affinities, electro-chemical attraction, and the attraction of aggregation. He suggested the application of this inference to explain various phænomena in mineralogy and geology.

Observations on the apparent Positions and Distances of 468 Double and Triple Fixed Stars, made at the Observatory at Pasy, near Paris, during the Summer of 1825. By James South, Esq. FRS.

Nov. 24.—At this meeting a paper was read On the Comparison and Adjustment of the New Standards of Weights and Measures. By Capt. H. Kater, FRS.

LINNEAN SOCIETY.

Nov. 1 and 15.—At these meetings a paper was read, entitled, "Observations on the unimpregnated Vegetable Ovulum, and on the Nature of the Female Flower in Coniferæ and Cycadeæ." By Robert Brown, Esq. FRS. FLS. &c.

ASTRONOMICAL SOCIETY.

Nov. 11.—The Society resumed its sittings this evening: and the President took the opportunity of calling the attention of the members to the remarkable circumstance of the appearance of no less than *four* comets during the recess: an occurrence unparalleled in the history of astronomy. The *first* of these (he observed) was discovered by M. Gambart, at Marseilles, on May 19, in the head of *Cassiopea.* The *second* by M. Valz, at Nismes, on July 13, near χ *Tauri.* The *third* by M. Pons, at Florence, on Aug. 9, in *Auriga.* The *fourth* (which was the most interesting and important of the whole, since it had been the object of solicitude at every observatory, and was anxiously expected and looked after by every astronomer) was discovered about July or August last. The President remarked this last comet (which is better known by the name of the *comet of Encke*) has now made 13 revolutions within the last 40 years: six of which have been regularly observed by astronomers. It was first seen in 1786; afterwards in 1795, 1805, 1819, 1822, and in the present year. It makes a complete revolution in about 1207 days, or about $3\frac{1}{3}$ years.

A paper was read, on the latitude of the Royal Observatory of Greenwich, by the Astronomer Royal. The co-latitude of this observatory, as computed from Dr. Bradley's observations under the direction of Dr. Maskelyne, is $38° 31' 22'',0$; a determination which is subject to the sum or the difference of two separate errors, one, in determining the zenith distance of *γ Draconis*, the other, in the measure of the distance of that star from the pole.

After the new mural circle was erected in 1812, another attempt was made to determine this important element, the result was 38° 21′ 21″,5; a result, however, in which it was thought probable that an error of half a second might exist.

In the year 1822 a new method of observing was introduced at Greenwich, by means of the *reflected* images of stars from an artificial horizon. To apply this to the determination of the element in question, by comparing two catalogues, one formed by direct vision, the other by reflection; that co-latitude being assumed to be the true one which made the sum of the small positive and negative differences equal to *zero;* and that was found to be 38° 31′ 21″, differing by *one second* from the determination furnished by Bradley's observation. This result, however, may involve an error of from a quarter to half a second, which subsequent observations may diminish.

The same paper includes some remarks on observations upon the pole-star, and an interesting circumstance, which is this:—The undulation to which a mass of mercury is liable, even with the greatest care, is, in itself considered, unfavourable to the exact bisection of an image; but a circumstance occurs in the formation of the image in the telescope, which, in some measure, compensates the inconvenience. The vibrations of the mercury in a longitudinal trough, occasion an elongated image of the star in the direction of the wire, appearing like a succession of stars which become smaller and smaller, as they recede from the central undefined mass, exhibiting an appearance like beads threaded on the wire, which is extremely favourable to bisection.

The elements of one of the comets above-mentioned, were announced to the Society, as computed by Mr. Taylor, sen., and Mr. Taylor, jun. of the Royal Observatory, and M. Capreci, of Naples. They are respectively, as below.

	Taylor, sen.	Taylor, jun.	Capreci.
Passage of perihelion	Dec. 10d·9338 Greenwich. M.T.	Dec. 10d·4559 Greenwich. M.T.	Dec. 8d·895 Naples. M.T.
Longitude of ditto	318° 3′ 57″	319° 10′ 26″	317° 24′ 40″
Longitude of ☊	35 46 58	35 45 36	35 19 50
Inclination of orbit....	33 20 40	33 30 42	32 44 20
Perihelion distance....	1·22951	1·24633	1·20808
Motion	Retrogade.	Retrograde.	Retrograde.
	From 3 observ.	From 3 observ.	From 4 observ.

A letter was read from Mr. R. Comfield, a member of the Society, to Dr. Gregory, describing an appearance noticed by him with a Gregorian reflector, power 350, and by Mr. J. Wallis, the lecturer on astronomy, with a Newtonian telescope, power 160, in reference to the occultation of *Saturn* on Oct. 30th. To each of them that part of the ring of *Saturn* which last *emerged* from the moon's dark limb (neither of them could observe the immersion) was rendered sensibly more obtuse, and at the instant after separation approximating to a rectilinear boundary. At the emergence of the eastern limb of the *globe* of *Saturn*, a similar effect was observed by Mr. Comfield, but not by Mr. Wallis.

A paper was next read, on the determination of latitudes by observations of azimuths and altitudes alone, by M. Littrow, Assoc. Ast. Soc. This paper includes the consideration of four cases. In the 1st, the latitude is computed from the observed azimuth and altitude. In the 2d, two observed altitudes are taken, and the two instrumental azimuths at the same respective moments; and the latitude is found from the corrected altitudes, and the *difference* of the azimuths, with the addition of an *approximate* latitude. In the 3d case, three observed latitudes, and three corresponding azimuths, or two azimuthal differences, are required; and the latitude is thence determined. In a 4th case, the problem is solved by means of a watch instead of an azimuth circle; there are supposed given, the time of culmination only within half or three quarters of an hour, three altitudes taken within that distance of the meridian, and their intervals in time; to find the true latitude. The solutions to all the four cases are exceedingly simple, and the resulting formulæ admit of the utmost facility of application.

Lastly, there was exhibited to the Society, a model of one of the large Reflecting Telescopes, made by Mr. John Ramage, of Aberdeen, and of the stands, frame, and mechanism, for facilitating its motions and adjustments. The reading of a descriptive paper by Mr. Ramage, was also commenced; but its termination was postponed until the December meeting.

MEDICO-BOTANICAL SOCIETY.

Oct. 14.—This being the first meeting of the Society after the recess, the Director (Mr. Frost) delivered an introductory discourse, describing the objects of the Society, &c.

A Report of the various medicinal plants now in flower in the gardens and stoves round the metropolis, was read.

Some fine specimens of several species of Nicotiana, presented by Mr. Anderson, were exhibited.

Nov. 11.—At this meeting, M. C. Friend, Esq. FRS. who had just returned to England from Sierra Leone and Demerara, announced to the Society that he had collected during his voyage many kinds of bark, seeds, &c. used by the natives of Cape Coast and Accra, as medicines, which he announced his intention of presenting at the next meeting.

Some observations were made on the properties of the preparations of iodine, from which it appeared that very untoward symptoms had ensued in several cases in which they had been employed to lessen glandular swellings.

The Director read some remarks on the use of the pulp of Adansonia digitata as an article of the Materia Medica.

SOCIETY OF PHYSICIANS OF THE UNITED KINGDOM.

At a meeting of the Society of Physicians of the United Kingdom holden Nov. 2, the following officers were elected for the ensuing year:—

President.—Dr. Birkbeck.

Treasurer.—Dr. Clutterbuck.

Secretary.—Dr. Shearman.

Communications, whether from members or others, addressed to the Secretary, No. 30, Northampton-square, will be submitted to the consideration of the Society, and the most interesting and important of them selected for publication, as soon as sufficient materials shall be collected to form a volume.

Article XII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Analysis of the Ashes of the Coal of Anzin.* By M. Feneuille.

M. Feneuille's analysis of these ashes gave him per cent.

Sulphuret of calcium	0·02
Sulphate of lime	1·19
Silica	43·92
Alumina	28·88
Protoxide of iron	17·38
Oxide of manganese	1·86
Carbonate of lime	3·18
Magnesia	0·90
Sulphates of soda, alumina, iron, and magnesia	0·50
	97·83

(Bullet. des Sciences.)

2. *Table of Equivalents.*

The following numbers should be substituted for those given in the Table of Equivalents, inserted in the *Annals* for October last.

Acid, fluoboric	34
iodic	164
Ammonia, fluoborate	51
iodate	181
Barium, iodide	194
Iridium, chloride	66
Manganese, chloride	64
Mercury, iodide	324
Soda, tartrate of potash	212

Zoology.

3. *Letter from Mr. Dillwyn to Mr. Gray.*

MY DEAR SIR,

I send you a letter I have just received from Mr. Dillwyn, which may prove interesting to some of your readers. Do me the favour to insert it amongst your Notices. Yours truly,

J. G. Children, Esq. J. E. Gray.

DEAR SIR, *Athenæum, Nov.* 21, 1825.

In the last breeding season, my gamekeeper at Pentlergare killed a Hen Harrier when sitting on her eggs; and being informed of the circumstance I directed him to watch for the arrival of her mate, which was shortly afterwards shot as he alighted on the side of the nest. It proved, as I expected, to be a Ringtail; and this, as well as many other observations which I have made, is sufficient to dispel the prevailing doubt which you mentioned of these birds being more than the male and female of the same species.

I am, dear Sir, yours truly, L. W. Dillwyn.

Miscellaneous.

4. *Private Tuition.*

The Rev. J. B. Emmett, of Great-Ouseburn, near Boroughbridge, in Yorkshire, a gentleman well known to our readers by his many valuable papers published at various times in the *Annals of Philosophy,* proposes taking three or four pupils to be instructed in every branch of mathematics and philosophy; their course of study will be the same as that pursued at Cambridge, and elucidated with suitable apparatus; and those who have sufficiently advanced in mathematical knowledge, will have the use of a complete astronomical observatory; they will also have the advantage of access to a select library. Candidates for Holy Orders will be instructed in the Classics and Theology.

5. *Developement of Electricity by Muscular Contraction.*

Since the discovery of electricity, the most distinguished philosophers have concurred in regarding it as the principal agent in some of the most important phenomena of animal life. Their opinion, however, for a long time seemed to be supported rather by analogy than by direct evidence. Comparatively but a few months have elapsed, since two Swiss physiologists, Prevost and Dumas, proved that muscular contractions in whatever manner exerted, whether mechanically or chemically, are invariably accompanied by a developement of electricity. It still remained to be decided whether this electricity be necessarily present as the essential cause, or merely as an accidentally associated phenomenon. The following experiments seem to carry us a step further towards the decision of the question.

Dr. Edwards has investigated the effects produced by touching a nerve in a manner which had been but little attended to. It consists in passing a solid body along a nerve, in the same manner in which we pass a magnet along a bar of steel which we wish to magnetize; he conducts the experiment in the following manner. He lays bare the sciatic nerves of a frog in that part of their course in which they are situated on the sacrum, leaving unimpaired their connection with the spinal marrow, and with the muscles to which they are directed. He removes the skin from the posterior extremities, that the movements of the muscular fibres may be visible, and intercepts volition by dividing the spinal marrow just below the head; he then places under the nerves a slip of oiled silk, by which they are raised and supported on a level with the bone. If a metallic rod be now drawn lightly along the denuded nerve, in the mode above-mentioned, muscular contractions will be excited. This effect is produced whatever be the metal employed. It is not even necessary that the rod should be metallic. Horn, glass, ivory, or any other solid body will answer the purpose, but their influence is by no means the same. Though Dr. Edwards clearly ascertained this fact, continual variations in the irritability of the animal precluded the possibility of establishing a scale. The Doctor then substituted for the oiled silk, which is a very complete non-conductor of electricity, a slip of muscle perfectly similar as to form and size, but which, it will be remarked, is an excellent conductor. A repetition of the contact now no longer caused contractions, or at most they were extremely feeble.

In the first experiment, the electricity developed by the contact of the nerve is retained, and its influence is concentrated on the nerve itself. In the second case, the electricity is abstracted. If the presence of electricity were merely adventitious, Dr. Edwards thinks that the same mechanical excitation ought in both cases to produce the same effect; but the difference is decided. He therefore concludes that electricity is essential to muscular contraction.

Article XIII.

NEW PATENTS.

T. Steele, Magdalen College, Cambridge, for improvements in the construction of diving bells.—Oct. 28.

J. and S. Seaward, Poplar, engineers, for an improved method of propelling boats, craft, and all kinds of vessels, on canals, rivers, and other shallow waters.—Nov. 1.

W. Ranyard, Kingston, tallow chandler, for a circumvolution brush and handle.—Nov. 1.

V. Royle, Manchester, silk manufacturer, for improvements in the machinery for cleaning and spinning of silk.—Nov. 1.

J. I. Hawkins, Pancras Vale, civil engineer, for improvements on certain implements, machines, or apparatus, used in the manufacturing and preserving of books, whether bound or unbound.—Nov. 1.

J. and W. Ridgway, both of the Staffordshire Potteries, manufacturers of china, stone, and earthenware, for an improved cock tap or valve, for drawing off liquors.—Nov. 1.

T. Seaton, Bermondsey, shipwright, for improvements on wheeled carriages.—Nov. 7.

G. Hunter, Edinburgh, clothier, for an improvement in the construction, use, and application of wheels.—Nov. 7.

T. S. Brandreth, Liverpool, for an improved mode of constructing wheel carriages.—Nov. 8.

S. Brown, Old Brompton, Middlesex, for improvements in machinery for manufacturing casks and other vessels.—Nov. 8.

W. E. Cochrane, Regent-street, for improvements in cooking apparatus.—Nov. 8.

J. W. Hiort, Office of Works, Whitehall, architect, for an improved chimney or flue, for domestic and other purposes.—Nov. 8.

C. L. Giroud, Lyons, France, for a chemical substitute for gall nuts in all the different branches of the arts or manufactures in which gall nuts have been accustomed or may hereafter be used.—Nov. 8.

J. Wilks, tin-plate worker, and J. Erroyd, grocer, both of Rochdale, Lancashire, for an engine for cutting nails, sprigs, and sparables, on an improved system.—Nov. 8.

J. J. A. M'Carthy, Pall Mall Place, Westminster, for improved pavement, pitching, or covering, for streets, roads, ways, and places.—Nov. 10.

B. Cook, Birmingham, brass founder, for a new method of rendering ships' cables and anchors more secure, and less liable to strain and injury while the vessel lays at anchor.—Nov. 10.

B. Cook, Birmingham, brass founder, for improvements in the binding of books and portfolios of various descriptions.—Nov. 10.

J. G. Deyerlein, Mercer-street, Middlesex, smith and tool maker, for improvements on weighing machines.—Nov. 10.

S. Parker, Argyle-street, Middlesex, bronze and iron founder, and W. F. Hamilton, Nelson-street, Long-lane, Surrey, engineer, for a certain alloy or alloys of metals.—Nov. 12.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1825.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
10th Mon.							
Oct. 1	S E	30·02	29·89	65	53	—	—
2	S E	29·89	29·85	65	49	—	—
3	S E	29·98	29·84	65	53	—	39
4	S W	30·23	29·98	68	48	—	17
5	S	30·23	30·18	67	57	—	14
6	S	30·18	29·72	67	53	—	52
7	S W	30·20	29·72	59	48	—	—
8	S W	30·19	30·09	64	50	—	11
9	W	30·35	30·10	64	58	—	
10	S W	30·48	30·35	61	57	—	
11	S W	30·48	30·30	65	43	·83	
12	S E	30·30	30·27	68	48	—	
13	N W	30·31	30·28	65	40	—	
14	S E	30·57	30·31	63	40	—	
15	N W	30·61	30·57	63	32	—	
16	N W	30·61	30·39	61	35	—	—
17	S W	30·39	30·22	56	40	—	—
18	S W	30·22	29·57	54	45	—	32
19	N W	29·57	29·23	53	33	—	40
20	N	29·56	29·24	45	36	—	
21	N	30·03	29·56	47	36	—	
22	N	30·28	30·03	57	25	—	
23	N W	30·28	30·12	49	38	—	
24	W	30·12	30·09	55	38	—	—
25	N W	30·25	30·10	58	27	—	
26	N W	30·25	30·20	45	35	—	
27	N	30·20	30·15	51	40	—	
28	N W	30·18	30·15	58	50	·98	
29	W	30·18	30·15	60	41	—	12
30	W	30·16	30·15	57	42	—	08
31	N W	30·15	30·10	61	44	·20	02
		30·61	29·23	68	25	2·01	2·27

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Tenth Month.—1. Cloudy. 2, 3. Rainy in the mornings. 4. Fine. 5. Cloudy and fine. 6. Cloudy: night stormy. 7. Cloudy and fine. 8. Fine. 9. Cloudy. 10—12. Fine. 13. Fine: a *stratus* on the marshes. 14. Fine. 15. Fine: foggy at night. 16. Foggy morning: very fine day. 17, 18. Cloudy. 19. Rainy. 20. Fine: a little snow at four, p.m. 21—28. Fine. 29. Cloudy. 30. Cloudy. 31. Fine.

RESULTS.

Winds: N, 4; SE, 5; S, 2; SW, 7; W, 4; NW, 9.

Barometer: Mean height

For the month.................................... 30·118 inches.

Thermometer: Mean height

For the month.................................... 51·129°

Evaporation.................................... 2·01 in.

Rain.................................... 2·27

Laboratory, Stratford, Eleventh Month, 19, 1825. R. HOWARD.

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