L. R. 1.

# ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS,

*NATURAL HISTORY,*

AGRICULTURE, AND THE ARTS.

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ANNALS

OF

PHILOSOPHY.

*JULY*, 1816.

## Article I.

*On the Introduction of the new Mode of Bleaching into Great Britain.* By Thomas Thomson, M.D. F.R.S.

On reading the statement given by Mr. Parkes, in his Chemical Essays lately published (vol. iv. p. 45), that Professor Copland, of Aberdeen, communicated the method of whitening linen and cotton by means of oxymuriatic acid to the Messrs. Milnes, of the house of Gordon, Barron, and Co. about the end of July, 1787, and that these Gentlemen were the *first* that actually applied oxymuriatic acid to the purpose of bleaching either linen or cotton goods for sale, I felt much surprise. For a period of nearly 30 years, the merit of introducing the new mode of bleaching into Great Britain had been universally ascribed to Mr. Watt. A posthumous claim like that brought forwards by Mr. Parkes cannot be listened to unless it be supported by very complete evidence. Mr. Parkes' evidence consists in the testimony of Professor Copland and of the Duke of Gordon, that the experiment was exhibited to them in 1787 by Saussure, and that Mr. Copland communicated the fact to Messrs. Milnes about the end of July, 1787, and that they immediately bleached by means of it a hank of yarn. (*Annals of Philosophy*, vol. vii. p. 100.) So far the evidence is unexceptionable and satisfactory. Professor Copland was the first person who introduced the new mode of bleaching into the county of Aberdeen, and the Messrs. Milnes were the first persons in that county who put that method into practice.

But before this claim of Professor Copland can supersede that of Mr. Watt, it would be necessary to show that Mr. Watt's experi-

ments were not of a prior date. Now this Mr. Parkes does not attempt to do. He merely says, "I have the utmost reason to believe, in opposition to an account lately given in a very respectable publication, that theirs (Messrs. Milnes) was the first actual application of the oxymuriatic acid in Great Britain to the purpose of bleaching either linen or cotton goods for sale." (Essays, vol. iv. p. 46.) In his answer to Dr. Henry he says, "I think I have completely established the fact that oxymuriatic acid was employed at Aberdeen in preparing goods for sale many months prior to any such application of it at Manchester, or at any other place in Great Britain, Mr. Macgregor's works in Scotland, where the operations of Mr. Watt were conducted, being alone excepted." (*Annals of Philosophy*, vol. vii. p. 101.) In this passage he appears to admit the priority of Mr. Watt, though in somewhat equivocal terms, and consequently to give up the claim of priority on the part of Professor Copland and Messrs. Milnes, which he maintained in his Essays. Indeed, the letters of Mr. Watt quoted by Dr. Henry in his observations on Mr. Parkes' statement (*Annals of Philosophy*, vol. vi. p. 421) seem to me to establish Mr. Watt's claim in a satisfactory manner.

But as I regarded it to be a matter of considerable importance in a subject of this nature to procure correct information while it could be had, I took advantage of a late visit to Birmingham to apply to Mr. Watt, as the only person capable of setting the dispute entirely at rest. Fortunately Mr. Watt has preserved copies of all his letters since the time that he invented the *copying press*, which was in 1782, or perhaps earlier. He was so obliging as to allow me to examine these letters. Out of several relating to the new mode of bleaching, I shall notice two, of the earliest dates that I could find, relative to this process. It may be necessary to mention, in the first place, that Mr. Watt was in Paris in 1786, that he left it before the end of that year, and that it was during this visit that Berthollet communicated to him the new process of bleaching.

The first letter which I shall mention is dated Birmingham, March 19, 1787, and is addressed by Mr. Watt to his father-in-law, Mr. Macgregor. In this letter he describes the new process of bleaching, points out its advantages, and informs Mr. Macgregor that he had sent him a quantity of the whitening liquor.

The second letter is dated Birmingham, May 9, 1787, and is addressed to M. Berthollet. I transcribe the following paragraph: "Je ne sais pas si j'ai encore fait la liqueur acide si fort que vous avez fait, mais je vous donnerai les moyens de juger. Je trouve que 4 onces de mon acide melé avec la quantité necessaire d'alkali de pearl-ash peut blanchir un gros de toile brune telle comme j'ai vu chez vous. Il est vrai qu'il ne le fait tout a fait blanc, mais il le fait aussi blanc que je puis le faire, meme en ajoutant une second dose d'acide. Je bouille la toile par avance dans une solution d'alkali faible; et a mi blanc, je la bouille une second fois. Je

trouve que le savon est meilleur que l'alkali pur pour la second bouillon. J'ai blanchi toute á fait le coton, mais je ne suis encore parvenu a blanchir *parfaitement* la toile de lin."

When the dates of these two letters are contrasted with the *end of July*, 1787, the time at which the experiments of Messrs. Milnes commenced, it is unnecessary to say any thing further on the subject.

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## Article II.

*An Account of the Aluminous Chalybeate Spring which has lately appeared on the Property of Sir Andrew Lauder Dick, Bart. at Fountain Hall, in East Lothian.* By Thomas Lauder Dick, Esq. F.R.S.E.

(To Dr. Thomson.)

SIR, *Relugas, near Forres, April* 13, 1816.

The spring which I propose to describe appears in a hollow at the bottom of a wooded bank facing the north, with a gradual acclivity. Its situation is in a coal-field, the strata accompanying which dip gently in a direction somewhat to the north. In the very spot where the spring now rises, a shaft was sunk above 60 years ago, in order to admit of the coal being relieved from water, by means of a pump, which was kept constantly going in it, whilst working shafts were put down higher up the bank, and to the rise of the strata, for the purpose of mining the coal, and bringing it to the surface. The length of pump required for drawing up the water to the mouth of the lower pit was about 15 or 20 fathoms. The operations of mining having been carried on to a very considerable extent in the immediate neighbourhood, large wastes were formed by the workings; but the proprietor having at last given them up in that particular quarter, the several pits were filled with rubbish, leaving only such small concavities as generally remain in similar cases. It is not more than seven or eight years since water was first observed to run from that hollow, marking the situation of the lower shaft, where the pump had been at one time used; nor did this circumstance, when first noticed, awaken any particular attention: but while I was in East Lothian, in the month of April of last year, a Gentleman who happened to be walking near the spot had his curiosity excited by his dog being dyed of a bright rust colour, from having waded in amongst the weeds and rotten branches which choaked the mouth of the spring. Though water coming from coal commonly gives this appearance to the earth, stones, decayed vegetables, &c. coming in contact with it, and deposits an ochrey sediment as it flows along, yet it was here so remarkable as to lead the Gentleman to drink a little of the spring, when, being much struck with its very peculiar taste, and having accidentally commu-

nicated the circumstance to me, I was induced to investigate it more particularly. The first object was to clear out the fountain head; and accordingly my father, the proprietor, employed workmen to remove the rubbish from its mouth, and directed a properly shaped well to be constructed, the small basin of which being about three feet wide each way, might in depth contain a foot of water, having a wide arched cover of sod over it, to prevent the leaves from dropping into it, but so constructed as to be perfectly open in front, so as to afford full access to it. The run then appeared very great, and might have probably filled a pipe of between two and three inches diameter. Having tasted the water, I found it powerfully astringent, and slightly acid. On adding a few drops of strong tea to some of it in a wine-glass, it became black, indicating the presence of iron; and having next evaporated an English gallon of the water to perfect dryness, I obtained 85 grains of product, which, from the shape and colour of some of the crystals, being in rhomboidal prisms, and of a greenish hue, appeared to be sulphate of iron, together with a mixture of pearl-coloured scales of sulphate of alumina. I may remark, also, that having evaporated about a tea-cup full of the water in union with tea, I obtained a black viscid residuum, which, when used on paper with a brush or pen, I found had all the appearance of the best Indian ink, being, like that pigment, capable of dilution with water to any shade, and scarcely in any way distinguishable from it, excepting by a faint and delicate greenish tinge, which rather added to the softness of its tone. On making the experiment, I found that this matter could be employed with considerable advantage in drawing, and some sketches I did with it at the time have remained unchanged, though it is now a year since they were executed.

Being destitute of the agents necessary for the analysis of mineral waters, and being moreover inexperienced in the practice, I sent a bottle filled from the spring in question to my friend Mr. Ellis, in Edinburgh, together with some of the dry product obtained by evaporation of the water; and he and Dr. Murray having been so obliging as to submit it to the following tests, I give the results from Mr. Ellis's letter to me, trusting to the forgiveness of those Gentlemen for the liberty I take; and I use this freedom with the less ceremony, that they have been already printed in some of the newspapers, having been inserted without authority by some person who may have probably seen the letter by accident:—

1. Litmus paper was reddened by the water, proving the existence of an acid.

2. Tincture of galls produced blackness, proving the presence of iron.

3. Water of potash threw down a greenish precipitate, indicating the existence of an earth in combination with the iron.

4. The precipitate (3) was partly redissolved by the addition of more potash, proving that the earthy matter was alumina.

5. Muriate of barytes occasioned copious precipitation, showing

that the acid in combination with the alumina was either the sulphuric or carbonic.

6. The precipitate (5) did not disappear on adding muriatic acid, proving that the acid present was not the carbonic.

A portion of the dry product was redissolved in common water, and afforded by the same tests nearly the same results. Another portion was submitted to a red heat, and was in great part converted into red oxide of iron by dissipation of its sulphuric acid.

From these results it is inferred that iron and alumina, in combination with sulphuric acid, are the chief, if not the sole, ingredients of the water. It is, therefore, an aluminous chalybeate spring, being something similar to that of the Hartfell, at Moffat. Such waters are common in the neighbourhood of the greywacke, from which they derive their impregnation; but this rock exists nowhere nearer to the spring in question than the Lammermoor range, from which it is at least seven miles distant. Its mineral qualities are, therefore, to be attributed to the iron pyrites of the coal-field whence it originates. From my ingenious friend Mr. Scott, whose accurate knowledge of the whole of the coal formations, and other strata, from the Firth of Forth to the English border, has been acquired from personal surveys, and examinations frequently repeated for a long series of years; and who, from his residence at Ormiston, and management of Lord Hopetoun's coal-mines, is particularly enabled to give information about the coal district more immediately under consideration, I learn that iron pyrites of all degrees of hardness, from that of striking fire with steel, to that of being friable between the fingers, accompanies this coal (as indeed it does most others), and large quantities of it are left amongst the rubbish in the wastes from whence the coal has been wrought out. The circulation of air through these wastes has the effect of reducing, in process of time, even the hardest parts of the pyrites into a powder, so fine as to be soluble in water. This fact is well known to those who are acquainted with the mode of making sulphate of iron from iron pyrites. There thus appears to be nothing, either extraordinary in the particular ingredients of this water, or incomprehensible in the cause of their being found in it. But the sudden appearance, after a lapse of above 50 years, of such a strong discharge from the *mouth* of an old filled up shaft, to lift the water from the *bottom* of which formerly required the employment of a long pump, is rather more curious; and though alternating springs are, I believe, by no means very uncommon, yet the history of this new one, since the period of its being cleared out and shaped into a well, may perhaps be calculated to give rise to some interesting speculations, and from the form and arrangement of the subterranean cavities being in this instance in some measure already known, it may even throw some general light on the causes of the alternations of other springs, where conjecture can alone be employed.

Having remained at Fountain Hall after the construction of the

basin of the well until May 13, I had an opportunity of paying almost daily visits to it, during which time the discharge of water continued undiminished. The fame of its tonic powers began to spread, and people came in considerable numbers to drink the water, or to carry it off to a distance for medicinal purposes, and its efficacy was further established by sanction of the example of an eminent medical practitioner at Haddington, who sent a cart and cask above seven miles for it. But at last, in the month of September, my father remarked a considerable diminution in the discharge, and soon afterwards a total failure of the water occasionally taking place, and having mentioned these his observations in his letters addressed to me here, he was, at my request, induced to keep a register of its variations. When his observations became regular, they were made in the morning, and, according to the length of the day, from six to eight o'clock; and in the afternoon at three or four o'clock.

In considering the memoranda which I am now about to lay before you, the letters employed in marking the increase or decrease of the depth of water in the basin of the well, with the time of each observation, are to be attended to; R, signifying that the well was running over; F, that it was full; In., means the inches down, unless when specified otherwise; M, is morning; and A, afternoon. It is also to be noticed that the degree of Fahrenheit's thermometer having been remarked, it is indicated by T; and afterwards the elevation of the barometer having been also attended to, its height is indicated by B.

*Register of the intermittent Appearances in the Fountain Hall Chalybeate Spring.*

1815.

Sept. 29, R, stopped.
Oct. 3, R, slowly.
5, R, increased.
6, R, stopped.
8, M, 2 in.—4 o'clock, A, F.
9, M, R.
10, R, increased considerably.
17, R, stopped.
18, M, 2 in.—11 o'clock, R.—A, R, greatly increased.
19, 10 o'clock forenoon discharge about 12 English pints in the minute.
21, M, R, stopped.—Great fall of snow on the 19th and 20th.
22, A, 2 in.
23, six o'clock, M, R.—10 o'clock, R, strong.
25, M, F, but not R.
28, M, 4 in.
29, M, 4 in.—A, 2 in.
30, M, F.
31, A, F.
Nov. 1, M, R, a very little.—A, ditto.
2, M, K, stopped.
3, M, 2 in.
4, M, ½ in.—A, F.
5, M, ¼ in.—A, F.
6, M, R.—10 o'clock, R, briskly.—A, ½ in.
7, M, 1¼ in.—10 o'clock, ¼ in.—A, R.
8, M, F.—9 o'clock, ¼ in.—A, R.

Nov. 10, M, 2 in.—A, ¼ in.
11, M, 2 in.—A, F.
12, M, eight o'clock, R, at the rate of four English pints per minute.—nine o'clock, R, stopped, but F.—A, R.
13, 11 o'clock forenoon measured, and found to be R, at the rate of 30 English pints in the minute.—A, R, apparently still brisker.
14, M, 2 in.—A, 3 in.
15, M, 3 in.—A, 3 in.
16, M, 3 in.—A, 3 in.
17, M, 5 in.—A, 5 in.
18, M, 5¾ in.—A, 5¾ in.
19, M, 4¼ in.—A, 3 in.
20, M, 2 in.—A, 2 in.
21, M, 2¼ in.—A, 2¼ in.
22, M, 2½ in.—A, 2½ in.
23, M, 5 in.—A, 5 in.
24, M, 5 in.—A, 5 in.
25, M, 4¼ in.—A, 4¼ in.
26, M, 4 in.—A, 4 in.
27, M, 2 in.—A, 2 in.
28, M, less than 2 in.—A, 1 in.
29, M, less than 1 in.—A, 2 in.
30, M, 2 in.—A, R.
Dec. 1, M, 2 in.—A, 2 in.
2, M, 3 in.—A, 6 in.
3, M, not quite 3 in.—A, ditto.
4, M, 1 in.—A, 3 in.
5, M, 3 in.—11 o'clock, 1 in.—A, R, as on Nov. 30, equal to a pipe of ¼ inch bore.
6, M, 3 in.—A, 6 in.
7, M, 9 in. which is lower than has as yet been observed.—A, 9 in.
8, M, 7 in.—A, 4 in.
9, M, 4 in.—A, 5½ in.
10, M, 6 in.—A, 6 in.
11, M, 3½ in.—A, 1½ in.
12, M, 4½ in.—A, 4½ in.
13, M, 2 in.—A, 6 in.
14, M, 5 in.—A, 3 in.
15, M, R, stream thickness of a goose-quill,—11 o'clock, ¼ in.—A, ¼ in.
16, M, R, as yesterday.—10 o'clock, F.—A, F.
17, M, 3 in.—T, on a tree, 16 deg. Snow, 1 inch thick.—A, 7 in. T, 19 deg.
18, M, 6 in. T, 26 deg.—A, 6 in. T, 19 deg.
19, M, 6½ in. T, 26 deg. Continued fall of snow.—A, 6½ in., and covered with a sheet of ice. T, 27 deg. Snow, eight inches deep.
20, M, 4 in. T, 32 deg. Ice melted. Snow continues to fall.—A, 4 in. T, 32 deg. Snow, 12 inches deep.
21, M, 7 in. T, 33 deg.—A, 8 in. T, 32 deg.
22, M, 8 in. T, 29 deg. Continued fall of snow.—A, 9 in. T, 27 deg. Snow about a foot deep.
23, M, 3 in. T, 32 deg.—A, 1½ in. T, 36 deg.
24, M, 2 in. T, 34 deg.—A, 4 in. T, 34 deg.
25, M, 10 in. T, 27 deg. at eight o'clock, and 23 deg. at nine o'clock.—A, at four o'clock, 11 in., not one inch of water remaining in it. T, 18 deg.
26, M, 4 in. T, 27 deg.—A, 1 in. T, 32 deg.
27, M, 7 in. T, 32 deg.—A, 11 in., as on the 25th. T, 26 deg.
28, M, 9 in. T, 32 deg.—A, 6½ in. T, 36 deg.
29, M, 6½ in. T, 39 deg.—A, 3 in. T, 46 deg.
30, M, not a drop of water in the well, notwithstanding the great flood occasioned by the melting of the snow. T, 36 deg.—A, same as in the morning. T, 33 deg.
31, M, ½ an inch of water in the well. T, 43 deg.—A, one inch of water in the well. T, 43 deg.

1816.

Jan. 1, M, 8 in. T, 42 deg.—A, 7 in. T, 43 deg.
2, M, 6 in. T, 42 deg.—A, 6 in. T, 43 deg.
3, M, 6½ in. T, 34 deg.—A, 8 in. T, 32 deg.
4, M, 8 in. T, 37 deg.—A, 7 in. T, 43 deg.
5, M, 7 in. T, 43 deg.—A, 7 in. T, 43 deg.
6, M, 3 in. T, 37 deg.—A, 2 in. T, 36 deg.
7, M, 8 in. T, 32 deg.—A, 10 in. T, 32 deg.
8, M, 2½ in. T, 41 deg.—A, ¼ in. T, 46 deg.
9, M, 6½ in. T, 39 deg.—A, 7 in. T, 37 deg.
10, M, 6 in. T, 37 deg.—A, 6 in. T, 42 deg.
11, M, 1½ in. T, 37 deg.—A, 6 in. T, 37 deg.
12, M, not above a spoonful of water in the well. T, 35 deg.—A, 6 in. T, 35 deg.
13, M, 3½ in. T, 35 deg.—A, 7 in. T, 35 deg.
14, M, not a drop of water in the well. T, 32 deg.—A, no water. T, 32 deg.
15, M, 7½ in. T, 32 deg.—A, 7 in. T, 35 deg.
16, M, 9½ in. T, 32 deg. Fall of snow ¼ of an inch deep.—A, 9½ in. T, 32 deg.
17, M, 1½ in. Fall of snow, with a violent storm of wind at west. T, 33 deg.—A, 6 in. Storm of wind continues. T, 34 deg.
18, M, not an inch of water in the well. T, 32 deg. Snow, 3 inches deep.—A, same as in the morning. T, 32 deg.
19, M, one inch of water in the well. T, 30 deg.—A, not an inch of water in the well. T, 39 deg.
20, M, 7 in. T, 28 deg.—A, 7 in. T, 30 deg.
21, M, 7¼ in. T, 33 deg.—A, 9 in. T, 34 deg.
22, M, 9 in. T, 34 deg.—A, 9 in. T, 34 deg.
23, M, 8 in. T, 33 deg.—A, 7½ in. T, 34 deg.
24, M, 7½ in. T, 34 deg.—A, 7 in. T, 34 deg.
25, M, 7 in. T, 34 deg. An additional fall of snow and sleet, which melted as it fell.—A, 7 in. T, 34 deg.
26, M, 8½ in. T, 34 deg.—A, 9 in. T, 34 deg. Great fall of sleet during the day, and much surface water from it and the melting of snow.
27, M, no water in the well. T, 32 deg.—A, no water. T, 32 deg.
28, M, well dry. T, 28 deg.—A, well dry. T, 30 deg.
29, M, well dry. Leaves frozen in the bottom. T, 21 deg.—A, a small quantity of water in the well, not an inch deep. T, 23 deg.
30, M, 9 in. T, 19 deg.—A, 7 in. T, 23 deg. Water frozen.
31, M, 6 in. T, 27 deg.—A, 6 in. T, 35 deg.

Feb. 1, M, 5½ in. T, 27 deg.—A, 5½ in. T, 33 deg.
2, M, 5 in. T, 33 deg.—A, 5 in. T, 35 deg.
3, M, 4 in. T, 35 deg.—A, 4 in. T, 37 deg.
4, M, 6 in. T, 35 deg.—A, 6 in. T, 37 deg.
5, M, 6 in. T, 35 deg.—A, 7 in. T, 35 deg.
6, M, 7 in. T, 35 deg.—A, 6 in. T, 29 deg.
7, M, 7 in. T, 25 deg. Fall of snow, with high wind at north-east, since yesterday afternoon.—A, 7 in. T, 27 deg.
8, M, 7 in. T, 22 deg. Water in the well frozen. Fall of snow from north-west.—A, 7 in., and frozen. T, 28 deg. Snow, two inche deep.
9, M, 7 in. Hard frozen. T, 17 deg.—A, 7 in. T, 23 deg.
10, M, 5 in. T, 28 deg.—A, 5 in. T, 34 deg.
11, M, 5½ in., and hard frozen. T, 27 deg.—A, 8 in. T, 28 deg.
12, M, a cake of ice about an inch thick taken off, and not a drop of water below. T, 27 deg.—A, a little water.
13, M, 7 in. T, 36 deg.—A, 7 in. T, 38 deg.
14, M, 7 in. T, 38 deg.—A, 7 in. T, 38 deg.
15, M, 6 in. T, 40 deg.—A, 5 in. T, 43 deg.
16, M, 3 in. T, 42 deg.—A, 3 in. T, 38 deg.
17, M, 7 in. T, 31 deg.—A, 7½ in. T, 31 deg.
18, M, 6 in. T, 31 deg.—A, 4 in. T, 42 deg.

Feb. 19, M, 6½ in. T, 26 deg.—A, 4 in. T, 31 deg.
20, M, 4 in. T, 42 deg.—A, 2 in. T, 45 deg.
21, M, 7½ in. T, 35 deg.—A, 8 in. T, 41 deg.
22, M, 5½ in. T, 42 deg.—A, 5 in. T, 46 deg.
23, M, 8 in. T, 35 deg.—A, 6 in. T, 45 deg.
24, M, 6 in. T, 44 deg.—A, 4 in. T, 43 deg.
25, M, 2 in. T, 38 deg.—A, 4 in. T, 35 deg.
26, M, 9 in. T, 32 deg.—A, 9 in. T, 38 deg.
27, M, 1 in. T, 42 deg. Between the hours of 10 and 11 o'clock forenoon stopped a leak in the basin, when the water rose to the brim. An attempt was then made to empty the well to the bottom; but in lowering, the growth of water gradually increased; and when about five or six inches down, the rush became so strong, that two men relieving one another were not able to gain upon it; and when they desisted, it again rose weaker and weaker until it was brim full, when it stopped. Between three and four o'clock in the afternoon, a similar attempt was made to empty the basin, but was again unsuccessful.—A, 5 o'clock, 1 in. T, 41 deg.
28, M, 8 in. T, 29 deg. 10 o'clock, 8 in., when one man with ease emptied the basin, and cleaned it out; it then soon rose to its former level of 8 in.—A, 4 o'clock, 8 in. T, 32 deg.
29, M, 6 in. T, 29 deg.
March 1, M, 6 in. T, 22 deg. Snow on the ground, but soon melted —A, 5 in. T, 33 deg.
2, M, 2 in. T, 32 deg.—A, 2 in. T, 36 deg.
3, M, 1¼ in. T, 33 deg.—A, 1¼ in. T, 38 deg.
4, M, 1½ in. T, 33 deg.—A, 3 in. T, 36 deg. Heavy shower of sleet.
5, M, 2½ in. T, 32 deg. These last four days the ground white with snow in the morning, which melted during the day.—A, 5 in. T, 38 deg.
6, M, 5 in. T, 33 deg. B, 28·86 inches.—A, 4½ in. T, 38 deg. B, 28·86 inches.
7, M, 6 in. T, 32 deg. B, 28·90 inches. Trees and ground white. Snow still falling.—A, 7 in. T, 34 deg. B, 28·91 inches. Frequent showers of sleet during the day, which soon melted.
8, M, 6½ in. T, 29 deg. B, 28·96 inches. Fall of snow two inches deep, and a continuance of heavy showers from the east.—A, 7 in. T, 32 deg. B, 29·02 inches. Snow near three inches deep.
9, M, 9 in. T, 26 deg. B, 29·24 inches. Snow as yesterday.—A, 7½ in. T, 35 deg. B, 29·30 inches. Bright sun-shine, and part of the snow melted.
10, M, 6½ in. T, 27 deg. B, 29·30 inches.—A, 6½ in. T, 36 deg. B, 29·30 inches.
11, M, 2 in. T, 41 deg. B, 29·16 inches.—A, ¾ in. T, 48 deg. B, 29·02 inches. Snow all gone.
12, M, 5 in. T, 39 deg. B, 29·10 inches.—A, 1 in. T, 43 deg. B, 28·98 inches. Tremendous shower of hail and rain, with a hurricane of wind.
13, M, 5½ in. T, 36 deg. B, 29·08 inches. Ground partly white with snow.—A, 7½ in. T, 36 deg. B, 29·19 inches.
14, M, 7½ in. T, 31 deg. B, 29·42 inches.—A, 3½ in. T, 35 deg. B, 29·10 inches. Fall of sleet.
15, M, R, which it has not done since Dec. 16. T, 40 deg. B, 28·80 inches. 10 o'clock, T, 33 deg. 11 o'clock, 1 in.—A, 5 in. T, 37 deg. B, 29 inches. Fall of snow and sleet during the day, and the ground white.
16, M, 7 in. T, 31 deg. B, 29·25 inches. 11 o'clock, 8 in.—A, 6½ in. T, 36 deg. B, 29·26 inches. Showers of sleet.
17, M, 6 in. T, 29 deg. B, 29·29 inches. The ground white with snow —A, 4 in. T, 37 deg. B, 29·20 inches. Showers of sleet.
18, M, R, T, 40 deg. B, 28·98 inches. 11 o'clock, stopped running, but brim full.—A, R, but hardly perceptible. T, 38 deg. B, 28·98 inches.

19, M, 3 in. T, 36 deg. B, 29 inches. 11 o'clock, 6 in.—A, four o'clock, 9 in. T, 42 deg. B, 29·28 inches.
20, M, six o'clock, 10 in. T, 32 deg. B, 29·68 inches.

It had occurred to me that the causes of the alternations in this spring might be chiefly owing to a body of water in the coal wastes, being balanced between a column of air at their extremity and the atmospheric air at the mouth of the well, the variations in the weight and pressure of the latter being the origin of the reciprocations in the discharge. A consideration of the foregoing register, which I lately received, gives me greater confidence in the truth of this theory. That this may be better understood, let Plate LI. Fig. 7, represent a section of the wooded bank, of which let A be the mouth of the well, directly over B, the old filled up shaft, and let C, D, E, represent the old coal waste. Now it is to be remarked that the dotted line, G *g*, in the hollow, being the boundary between the properties of Fountain Hall and Wood Hall, the whole of the coal under the bank, F, on the north side of the boundary, had been previously wrought out by its proprietor. Some of the miners, therefore, in C, D, E, took advantage of this circumstance, and broke through a small opening at the point *g*, so as to allow the water to escape from C, D, E, at the bottom of the pit, B, into the wastes below F, and thus to do away the necessity of continuing to employ a pump in B, to lift the water of their mine to the surface. The coal-mine under F being furnished with a level driven from a different quarter, this expedient of the miners in C, D, E, would probably serve to prevent the water from accumulating in their mine, not only whilst the coal was working there, but for years after it was abandoned, until, from the operation of some unknown cause, most likely from the falling in of the roof at that place, the small opening at *g* must have been at last so effectually choaked, as to allow no more water to escape that way, when in consequence it would gradually collect, and swell up considerably above the point D, the line of level of the basin of the well, A, until its weight, aided perhaps at first in some degree by a species of capillary attraction existing in the loose and comminuted materials with which B was filled at the time of the work being abandoned, the water would be at first drawn, and afterwards forced up, through B, and gradually forming a pipe for itself, would burst out in a strong current, forming the spring, A. It would thus continue to run from A for a length of time equal to that required for draining the water down to the level, D, which, from the great extent of surface of the under ground reservoir, would be very considerable. It is obvious that this vast subterranean body of water, operating as a solvent on iron pyrites, previously rendered friable, and prepared by the action of the air, would become highly impregnated with sulphate of iron; and as this water, D, E, must naturally be supplied in some measure from springs running through the remainder of the waste, C, it is probable that small quantities of the mineral

will be constantly washed from the rubbish through which these supplies must pass into the large reservoir; the strength of the impregnation, therefore, as well as the quantity of water in D E, will thus be kept up.

And now as to the alternating phenomena of the spring. The waste, C, being unoccupied by water, must be filled with air, which is confined in it, something like that in the upper end of the tube of an air thermometer. This air is probably in a great measure carbureted hydrogen gas, which, being of a lower specific gravity than atmospherical air, possesses more elasticity and compressibility. It is, therefore, evident that the body of water, D, E, A, will be pressed upon by two different columns of air, viz. at the two extremities, D and A; and it will then depend upon the different relative degrees of pressure, exerted by these respective columns, whether the water shall be prevented from flowing out at A, at least until a sufficient accumulation of it occurs in the waste to raise it to such a height at the extremity, D, as by its superior weight to overcome the pressure from the external atmosphere at A. The irregular ebbing and flowing of this spring is, therefore, owing to the irregular increase or diminution of the pressure of the exterior atmosphere, the effect of which is perhaps in some degree modified by some causes of a different description, existing in the interior of the excavations. An abstract of the averages of the state of the barometer, in each respective stage of the depression of level of the water in the basin of the well, drawn out from that part of the register where the barometer is marked, will in some degree prove the truth of this theory, though the period of the barometrical observation is hardly extensive enough for the purpose of obtaining any approximation to an absolutely correct conclusion. It will be observed, however, that (if we except the anomaly as to No 8, which number is not from an average, but the result of one observation only) No. 1 in the following table, where the well runs over, has the least atmospherical pressure upon it, whilst No. 16, where it is nearly empty, has the greatest:—

| No. | Height of water in the basin. | Average of Barom. | No. | Height of water in the basin. | Average of Barom. |
|---|---|---|---|---|---|
| | | Inches. | | | Inches. |
| 1 | Running over. | 28·92 | 9 | 5 in. down | 28·98 |
| 2 | $\frac{3}{4}$ in. down | 29·02 | 10 | $5\frac{1}{2}$ | 29·08 |
| 3 | 1 | 28·98 | 11 | 6 | 29·04 |
| 4 | 2 | 29·16 | 12 | $6\frac{1}{2}$ | 29·20 |
| 5 | 3 | 29· | 13 | 7 | 29·06 |
| 6 | $3\frac{1}{2}$ | 29·10 | 14 | $7\frac{1}{2}$ | 29·30 |
| 7 | 4 | 29·20 | 15 | 9 | 29·31 |
| 8 | $4\frac{1}{2}$ | 28·36 | 16 | 10 | 29·68 |

The failure of the attempt made to empty the well on Feb. 27 is easily accounted for by the same theory; for in proportion as the water came to be diminished in height at A, the weight at D would be increased by the level there being rendered higher than that at A, and would consequently by its pressure increase the discharge of water into the well. And as the surface of water at D must be supposed to be very extensive, the level at that place would take an infinitely longer time to reduce in height than that of the limited surface A would do, by the exertions of the men there, and so the weight at D would operate for a proportionably longer time. The success attending the attempt of Feb. 28 must have been owing to a peculiar increase of atmospherical pressure at the time, which is indeed manifested by the water in the basin sinking from one inch to eight inches down. The following extract from Mr. Scott's letter shows that his opinion as to the causes of the alternations of this spring nearly coincides with mine:—

"With respect to the ebbing and flowing of the water in the well, it is evidently occasioned by the changes which take place in the atmosphere; for when the atmosphere becomes dense, the air will act on the surface of the water in the well so as to force part of it to enter the wastes; and when the atmosphere becomes rare, the expansion of the air in the wastes will act upon the surface of the water there, so as to occasion that in the basin of the well to rise. This effect would be instantly destroyed, if an opening was made from the surface into the wastes, so as to make a free communication between the air of the wastes and that of the atmosphere. Some years ago, as I was travelling along a road near to East Houses, during a heavy summer shower of rain, my attention was arrested by the noise occasioned by the forcible blowing of water, in a hollow recently filled by the shower. On examination, I found that this hollow was immediately over an old coal waste, which led me to conclude that the boiling was occasioned by a sudden change in the atmosphere. The boiling was equal to the force of that occasioned in water by the pipe of a small pair of hand bellows. I had no opportunity of learning if a sudden fall in the barometer had taken place at the time, but there was no reason to doubt it. The difficulty found in attempting to empty the well shows that the water in it is on a level with a large quantity in the wastes. The well that supplies the house at Ormiston Hall (the Earl of Hopetoun's) is also connected with an old coal waste. An attempt was once made to empty it, but it was found impracticable."

The rationality of the explanation which I have ventured to offer, of the alternations in the discharge of water from the Fountain Hall alumino-chalybeate spring, would be better established by a regular and more extensive journal being kept, of the height of the barometer, and that of the water in the well, and by comparisons made between the two at the same period. Should this communication seem to merit your attention, the barometrical observations

might be resumed on this plan. But I may have already enlarged too much on a subject which may perhaps be more interesting to myself than to any of your readers.

I am, Sir, your obedient humble servant,

THOMAS LAUDER DICK.

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## ARTICLE III.

### *Of the Quadrature of the Circle.*

OF all the problems of speculative geometry that have attracted the attention, and occupied the ingenuity of the most eminent mathematicians, both ancient and modern, there is none of greater celebrity than the quadrature of the circle. The attempts, indeed, at solving this problem have been for some time abandoned; and it is now generally reduced to the same class of *desiderata* as the transmutation of metals, or the universal *panacea*; that is, it is considered as of impossible attainment. Its impossibility, however, though now so generally admitted, has never been clearly proved, nor the cause of it by any means distinctly understood. Indeed, there are good reasons for supposing that even yet very erroneous opinions are entertained respecting the nature of this problem by our most eminent mathematicians; and that although its solution be impossible by any of the methods which have been most in repute among the moderns for that purpose, it may be considered as possible by another method, which, though perhaps strictly geometrical, has been in modern times treated with general, though undeserved, neglect. On this account our mathematical readers will probably think their time well employed while we recal their attention to the history of this celebrated problem, and of the various attempts which have been made towards its resolution, as well as to the general principles by which alone we can decide upon its possibility or impossibility.

Euclid, the celebrated father of the ancient geometry, has made no direct attempt towards the quadrature of the circle, although he has done much to facilitate the solution of this problem. He has shown the proportion which all circles bear to one another, viz. the duplicate proportion of their diameters; and he has taught us how to inscribe in a circle a regular polygon of so great a number of sides that it should not touch another circle concentric with the former, although their circumferences should be ever so near to each other. It was evident, therefore, that to approximate within any required limits to the rectification or measurement of the circular perimeter, it was only necessary to invent a method of measuring the perimeter of such an inscribed polygon. This was accomplished to a great degree of accuracy by Archimedes, who took

up the subject where Euclid had left it, and has displayed an uncommon portion of ingenuity in the mensuration of this and of various other curvilinear magnitudes. Archimedes demonstrated that the area of a circle is equal to that of a right-angled triangle whose base is equal to the radius, and its altitude equal to the circumference, of the circle; and consequently may be found by taking half the rectangle or product of the radius into the circumference. The quadrature of the circle, therefore, is reduced to the rectification of its perimeter, or to the determination of the proportion between that and the radius or diameter.

To find this proportion as nearly as might be required, Archimedes set himself about the measurement of the perimeters of polygons inscribing and circumscribing a given circle with any required number of sides. From his various calculations, it appears that the perimeter of a circumscribing regular polygon of 192 sides is to the diameter in a less ratio than that of $3\frac{10}{70}$ to 1, and that the perimeter of an inscribing polygon of 96 sides is to the diameter in a greater ratio than that of $3\frac{10}{70}$ to 1. It follows that, *à fortiori*, the circumference of a circle is to its diameter in a less ratio than that of $3\frac{10}{70}$ to 1, and in a greater ratio than that of $3\frac{10}{71}$ to 1. It may, therefore, be assumed as very nearly in the ratio of $3\frac{1}{7}$ to 1, or, in whole numbers, as 22 : 7, which would give the proportion between the area of the circle and the square of its diameter as 11 to 14.

Some of the ancient mathematicians carried the approximation to the quadrature of the circle further than had been done by Archimedes; but they have been greatly excelled in this respect by the moderns. Vieta and Metius made the proportion between the diameter and the circumference to be as 133 : 355, which is within about $\frac{8}{100000000000}$th parts of the true ratio. This accuracy, however, was greatly exceeded by that of Ludolph van Collen, or Ceulen, who with astonishing industry and perseverance extended the approximation to 36 places of decimals; and the process was afterwards repeated and confirmed by his editor, Snellius. According to this investigation, if the diameter of a circle be 1, its circumference will be 3·141592653589793238462643383279502 88+.

About this period several false pretensions were advanced to the perfect quadrature of the circle by men of some degree of science, who either deceived themselves, or were induced by an unjustifiable vanity to attempt at deceiving others. Indeed, nothing has given rise to more paralogisms and false pretensions among mathematicians than the endeavours to supply this great desideratum in the science. It was a pretension of this kind in Joseph Scaliger, who was more of a man of letters than a mathematician, that gave rise to the violent contest between him, on the one side, and Vieta, Clavius, &c. on the other, who showed that the measure assigned by Scaliger was a little less than that of the inscribed dodecagon. Soon after the famous Hobbes imagined that he had found out the quadrature of the circle; and being refuted by Dr. Wallis, he wrote a

treatise to prove that the whole system of geometry, as then taught, was nothing but a series of paralogisms. This work he entitled, De Ratiociniis et Fastu Geometrarum. Longomontanus, also, a very respectable mathematician, was among the number of those that pretended to have discovered the real quadrature of the circle, and to be able to assign it in numbers.

Equally absurd were the pretensions of those who endeavoured to square the circle by certain mechanical and practical expedients. Thus the Cardinal de Cusa thought to succeed by rolling a circle or cylinder over a plane, and measuring the length of the line described during a complete rotation; and Oliver de Serres believed that by weighing a circle and a triangle equal to the equilateral triangle inscribed he had found that the circle was exactly double of the triangle, not being aware that this double is exactly the hexagon inscribed in the same circle. Others have been so infatuated as to offer considerable sums of money to those who should prove their pretended quadratures of the circle false. This was the case with a Sieur Mathulon, mentioned by Montucla, who, from a manufacturer of stuffs at Lyons, commenced geometrician and mechanist; as well as with another Frenchman, of some property, who, according to the same authority, about 40 years ago, deposited a sum of money to this effect; and whose pretended quadrature consisted in dividing a circle into four quadrants, and turning these outward, so as to form a square. Such ridiculous pretensions can only be classed with the notion of Henry Sullamar, a real Bedlamite, who found the quadrature of the circle in the number 666 inscribed on the forehead of the beast in the Revelations.

No sooner was the method of fluxions invented by Sir Isaac Newton, and made known to the mathematical world, than mathematicians began again to form sanguine hopes of being able to square the circle, on account of the admirable expedients which this method possessed for facilitating the quadrature of all kinds of curves. According to this method, the quadrature of any space was reduced to the determination of the fluent of a given fluxion; but this is a problem not capable of a solution in general terms. For though in all cases we can assign the fluxion of a given fluent, the reverse of this problem can be effected only in particular cases. Among the exceptions was found to be the circle, in respect to all the forms of fluxions by which its area can be expressed, to the no small mortification of mathematicians. Another expedient was adopted, with a view of obtaining a more fortunate result, by means of infinite series. The fluent expressing the fluxion of any area in general, though not accurately assignable, could always be expressed in the form of an infinite series, which, therefore, gave a general expression for every area. On substituting for particular cases, this series was often found to break off and terminate, and so to afford an area in finite terms. But here also the area of the circle proved refractory; for, notwithstanding every substitution, it always remained an infinite series.

The labours of mathematicians, however, towards the quadrature of the circle by means of the fluctionary calculus, were not altogether useless. They assiduously employed themselves in discovering and selecting the best forms of series for approximating to the circular area; among which those were evidently to be preferred which were at once simple and quickly converging; but these qualities were but rarely united in the same series, the quality of simplicity being found almost incompatible with that of a rapid convergency. Those series which converged the quickest, and were at the same time possessed of most simplicity, were found to be those in which, besides the radius, the tangent of some certain arc of the circle was the quantity by whose powers the series converged. A remarkable series of this kind was given by Dr. Edmund Halley from the tangent of 30 degrees, by means of which the industrious Mr. Abraham Sharp computed the area of the circle to 72 places of decimals. But even this was afterwards far exceeded by Mr. John Machin, who gave a series at once so simple and so rapidly converging, that by means of it, in a very little time, he extended the quadrature of the circle to 100 places of figures; to which M. de Lagny, in the Mem. de l'Acad. 1719, has added 28 places of figures more. To give some conception of the minuteness of this approximation, we may remark, that a series of 60 figures only is sufficient to express a number of grains of sand more than sufficient to fill a hollow sphere of a diameter equal to that of the orbit of the planet Saturn.

From these fruitless attempts to express the ratio between the diameter and circumference of a circle by numbers, even when brought to ever so minute a fractional division, it seems fairly deducible that this ratio is incapable of being so expressed, or is of that kind which is called *incommensurable.* This had indeed been more than suspected by many of the ancient geometricians; but has not to this day been strictly demonstrated, nor any otherwise proved than by the failure of all attempts to reduce this ratio to a commensurable form, or to express it by the ratio of one number to another. It would, however, be too much to assert from all this that the incommensurability of the circumference and diameter of a circle is incapable of geometrical proof, any more than that of the side and diagonal of a square, which has long ago been so elegantly demonstrated by Euclid; and we are rather to seek for the cause of this desideratum in mathematical science to the neglect into which the doctrine of incommensurable magnitudes has fallen with the moderns, although it formed so important a branch of the ancient geometry.

The doctrine of incommensurables forms the subject of the 10th book of Euclid's Elements, where it is fully, though rather prolixly, handled; this being one of the longest books in the whole work, and concluding with that remarkable proposition that the diagonal of a square is incommensurable to its side. Whether it be from the prolixity of this book of the Elements, or from whatever

cause it may arise, it is certain that it is now very seldom studied, or even consulted; and we conceive that we shall be rendering a very acceptable service to our mathematical readers by presenting them with the elementary principles of incommensurables in another and more concise form, and drawn from other sources, though proved in a manner not less strict and mathematical than that of Euclid. This will render the present paper on the quadrature of the circle much more complete than it would otherwise be, and will properly prepare the way for the remarks upon that subject, with which we intend to conclude.

### *Of the Doctrine of Incommensurables.*

Commensurable quantities are such as have a common measure, or to which some common aliquot part may be found. Incommensurable quantities are such as have no common measure, or common aliquot part.

All numbers, whether integral, fractional, or mixed, are commensurable. For any two whole numbers have the common measure of unity; and any two other numbers, whether fractional or mixed, may be reduced to fractions with a common denominator; when it is evident that unity set over that common denominator will measure both numbers.

Geometrical quantities, as lines, surfaces, and solids, may be either commensurable or incommensurable to each other. Thus a triangle and a parallelogram upon the same base, and between the same parallels, are always commensurable to each other; for the parallelogram is always double of the triangle. But the side of a square and its diagonal are always incommensurable to each other; that is to say, into whatever number of equal parts we should suppose the side of the square to be divided, the diagonal can never contain a precise number of these parts, or the side and the diagonal have no common aliquot part, however minute it may be conceived to be taken. The same is true of the perpendicular and side of an equilateral triangle, and of various other geometrical magnitudes, as may be proved by means of the following

### *Theorems concerning Proportion.*

1. If two equal ratios are combined, that is, multiplied together, they shall be to one another in the ratio of two square numbers. Thus if 2 : 4 be multiplied by 5 : 10, the resulting product, 10 : 40, reduced to its lowest terms, is as 1 : 4, the ratio of two square numbers. For the two ratios being equal are each as some common ratio; in the present case as 1 : 2; and their product will, therefore, be as that common ratio squared, or here as 1 : 4; and must, therefore, manifestly be in the ratio of two square numbers.

*Cor.*—In like manner, if three equal ratios be combined together, the product will be in the ratio of two cube numbers, and so in other cases.

2. The duplicate, or triplicate, ratio of any ratio of number to

number, will be in the first case as two square numbers; and, in the second, as two cube numbers. For, by the definition of duplicate ratio (see 5th book of Euclid), it is compounded of two equal ratios, and must therefore (by the preceding theorem) be as two square numbers; and, in like manner, triplicate ratio is compounded of three equal ratios, and must therefore (by the corollary) be as two cube numbers.

3. If three numbers in continued proportion be reduced to their lowest terms, the first and last shall be square numbers, and the middle term shall be the product of their roots. For the first number being to the third in the duplicate ratio of that in which it is to the second, the first and third will be to one another as two square numbers; that is, will be two square numbers when reduced to their lowest terms. Again, as a mean proportional is found by extracting the square root of the product of the two extremes; and as the square root of the product of two squares is the product of their roots, the square root of $a^2 b^2$ being $a b$, it follows that the mean proportional between two square numbers must be the product of their roots.

*Cor.*—In like manner, if four magnitudes be in continued proportion, the ratio of the first to the fourth will be as two cube numbers.

4. Two magnitudes are incommensurable to one another when their ratio is the simple ratio either of a duplicate ratio which is not as two square numbers, or of a triplicate ratio which is not as two cube numbers. For all magnitudes are incommensurable to one another, if they are not to each other as number to number; and it is proved (Th. ii.) that the duplicate or triplicate ratio of number to number is as two square or two cube numbers respectively. Wherefore if the duplicate or triplicate ratio of the ratio of two magnitudes be not as two square, or as two cube, numbers, these magnitudes cannot be as number to number, or are incommensurable to each other.

*Cor.* 1.—Two squares which are not to one another as two square numbers have their sides incommensurable. Consequently the diagonal of a square is incommensurable to its side; for the square of the diagonal being equal to the sum of the squares of two sides (Eu. i. 47), is to the square of the side as 2 : 1, which not being both square numbers, the diagonal and side are incommensurable.

*Cor.* 2.—If three magnitudes are in continued proportion, and the first is to the last not as one square number to another, the second shall be incommensurable to the first and last.

*Cor.* 3.—If four magnitudes are in continued proportion, and the first is to the last not as two cube numbers, each magnitude shall be incommensurable to that which follows it.

5. If three magnitudes be such that the square of one of them is equal to the squares of the other two, and at the same time the least be an aliquot part of the greatest, the remaining magnitude shall be incommensurable to the least and greatest. For as the

least is an aliquot part of the greatest, the square of the least will be an aliquot part of the square of the greatest. Hence when its square is subtracted from the greater square, the remainder will be expressed by a fraction whose denominator exceeds its denominator by unity; such as $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$, &c., since this fraction wants only some aliquot part to make it equal to unity. If, then, we call the greatest square 1, some such fraction as this will represent the middle square, which by the supposition, together with the lesser square, is equal to the greater square. But such a fraction as this can never be a square number; that is, have both its numerator and denominator square numbers; for no two square numbers have so small a difference as 1. Therefore if the squares of the greatest and least of the magnitudes be represented by square numbers, the middle one cannot be so represented, and is, therefore, incommensurable to the other two.

*Cor.*—Hence the side of an equilateral triangle is incommensurable to its perpendicular; for (Eu. i. 47) the square of the side is equal to the sum of the squares of the perpendicular and of half the side; whence, by the above theorem, the perpendicular and side are incommensurable.

6. If three magnitudes are in continued proportion, and the greatest is equal to the sum of the other two, they are all incommensurable to one another. For if we suppose that the magnitudes could be represented by numbers, these will not be all even when the ratio is reduced to its lowest terms, every even number being divisible by 2. Hence they must be represented in some of the seven following ways: odd, odd, odd; or odd, even, even; or odd, odd, even; or odd, even, odd; or even, even, odd; or even, odd, even; or even, odd, odd. It is manifest that in none of these cases can one extreme be equal to the sum of the other two numbers, except in the third, fourth, and last. But none of these can belong to three numbers in continued proportion. Since the quotients of the first and second, and second and third terms, can never correspond in any of them. Thus in the third case, or odd, odd, even, the quotient of the first and second is odd; and of the second and third, even. In the fourth case, or odd, even, odd, the quotient of the first and second is even; and of the second and third, is no whole number. And in the last case, the quotient of the first and second is no whole number; and of the second and third, is odd. Hence the assigned magnitudes cannot be represented by numbers, or are incommensurable.

In this very simple manner may the fundamental principles of incommensurable ratio be established, and the incommensurability of certain well known geometrical magnitudes demonstrated. We are unable, however, to apply these principles to the ratio between the diameter and circumference of a circle, for want of any known method of notifying the circumference, or finding a straight line to which it shall be precisely equal. This is a problem which is as yet unresolved, at least by the elementary geometry; and till its solution

is obtained, it cannot be expected that we can prove, by that geometry, either the commensurability or incommensurability of the circular diameter and circumference.

There are, however, other considerations, besides the failure of all the attempts that have hitherto been made to express this ratio by numbers, that tend directly to prove that it is an incommensurable ratio. Of these some are stated by Dr. Barrow at the end of his 15th mathematical lecture, by which that eminent mathematician is induced to conclude that the radius and circumference of a circle are lines of such a nature as to be not only incommensurable in length and square, but even in length, square, cube, biquadrate, and all other powers to infinity; for, says he, the side of the inscribed square is incommensurable to the radius, and the square of the side of the inscribed octagon is incommensurable to the square of the radius; and consequently the square of the octagonal perimeter is incommensurable to the square of the radius; and thus the ambits of all regular polygons inscribed in a circle may have their superior powers incommensurate with the co-ordinate powers of the radius; from whence the last polygon, that is, the circle itself, seems to have its periphery incommensurate with the radius.

Were we permitted to argue from analogical considerations in a question so strictly geometrical, we might remark that there is a great analogy between the circle and square, the one being the simplest of curves, and the other the simplest and most regular of rectilineal figures admitting of a diagonal. Hence that kind of ratio which is true of the diameter and periphery of the one may reasonably be expected to hold of the diagonal and periphery of the other. In this view the circle may be called a rounded square, and the square may be called a circle dilated into a cornered rectilineal figure. We shall add but one other analogy of this general or looser kind, and then proceed to speculations of greater certainty and precision. The angle subtended by the diagonal of a square, that is, any angle of the square itself, is always a right angle; but so also is the angle subtended by the diameter of a circle, and bounded by its circumference; for Euclid has proved that the angle of a semicircle is a right angle, however obliquely it may be drawn. May we not, then, infer from these analogies between the circle and square in respect of their diameters and circumferences, that since these are incommensurable in the one, they are so also in the other?

Supposing, then, the incommensurability of the circumference and diameter of a circle to be demonstratively, or at least inductively, established, what are we thence to infer respecting the quadrature of the circular area? Only this, that the quantity of that area can never be accurately expressed in numbers bearing a proportion to the length of the diameter; or that, when we have a number accurately denoting the length of the diameter, we cannot by any number whatever express either the exact periphery, or the exact area, of the circle. It does not, however, follow from the incommensurability of the circumference and diameter that we

cannot accomplish the quadrature of the circle geometrically, or that it should be impossible to describe a square by some geometrical construction, which shall be precisely equal to any given circle. On the contrary, the *possibility* of this cannot reasonably be controverted, since squares may be described differing from each other by the least possible quantity, and therefore precisely equal to any assigned area whatever. The incommensurability of such magnitudes, or their not being expressible by numbers, is no impediment whatever to the resolution of this problem; for we can find squares, by a very easy geometrical process, which shall be exactly equal to the sum or difference of any two assigned squares, whether these squares, when denoted by numbers, have a sum or difference accurately expressible by a square number or not. The problem by which this is accomplished is a manifest corollary from the 47th proposition of Euclid's first book, which teaches us that the square of the hypothenuse of a right-angled triangle is equal to the sum of the squares of the two sides; so that we have only to form a right-angled triangle of a proper length of sides and hypothenuse to find at once the side of a square equal to the sum or difference of any two given squares.

On similar principles, although it be impossible to find a square number which shall be exactly equal to the area of a circle whose diameter is equal to a specific number, it by no means follows that it is impossible to describe a square geometrically which shall be precisely equal to the area of that circle; or rather we are certain that such a solution of the problem of the quadrature of the circle is possible, since squares may be described of every possible dimension that may be required.

The ancients divided geometrical problems into three different classes, according to the manner of their solution. Those which could be resolved by means of the straight line and circle only were called plane problems; those which required the conic sections were called solid problems; and those which could not be performed without the aid of curved lines of some other species were called linear problems. The geometrical quadrature of the circle is a problem of the third class, since it cannot, as far as we know, be accomplished by means of the straight line and circle, or even by the assistance of any of the conic sections. It is, however, not the less strictly geometrical on this account, since there are various other problems on which the ancients have exercised their ingenuity, which labour under a similar restriction, and yet are considered as having been resolved by the purest principles of geometry. Such is the finding of two mean proportionals between any two given right lines, for the resolution of which Diocles invented the curve called *cissoid*, and Nicomedes invented that called *conchoid*; such also is the trisection of a given rectilineal angle which is resolvable by means of the same conchoid, although, indeed, it may also be resolved by the assistance of the hyperbola.

The same celebrated geometricians among the ancients who sought after solutions of these two remarkable problems were by no

means deficient in attempts to accomplish the quadrature of the circle by like expedients. We learn from the fourth book of the Mathematical Collections of Pappus that a curve line was invented by Dinostratus and Nicomedes, and by some mathematicians of more modern date, with a view towards the quadrature of the circle, which, on account of its office, they denominated τετραγωνιζουσα, or the *squaring line*. Pappus, indeed, objects to the admission of this curve into strict geometry, as the *genesis* by which these mathematicians suppose it to be described assumes principles that are inconsistent with each other, and in some measure involve the assumption of the commensurability of the diameter and periphery of a circle of which we have every reason to doubt the truth. He, however, thinks this curve of such importance, that he has given a geometrical demonstration of some of its properties; and of that, in particular, by which it leads to the quadrature of the circle. Clavius, the well known commentator upon Euclid, has treated much more fully of this curve, which he calls *quadratrix*, than Pappus has done; and has shown a method of describing it by which the objections of Pappus are in a great measure obviated. Its description by this method is so easily accomplished, even by the use of the ruler and compasses alone; and it may be so readily applied, when described, to the resolution of the great problem of the quadrature of the circle, that we are persuaded our readers will thank us for introducing this curve once more to their notice, and showing them its more remarkable properties, and the mode of its application to the problem in question.

## *Of the Quadratrix.*

The genesis of the quadratrix, according to Dinostratus and Nicomedes, is as follows:—Let A D, D C, be two equal right lines, perpendicular to one another, in the point D. (Plate LI. Fig. 1.) While the line A D revolves with an equable motion about A, as a centre, so as to describe the quadrant D B, let the line D *c* move equably and parallel to itself along D A, so as to coincide with the radius A B at the precise moment that the point D arrives at B. By this compound motion the intersection of the two lines A D and D C, or the point D, will describe a curve as D 12345678, which is called *quadratrix*. It is objected by Pappus to this genesis, that, if both motions be equable, they could not terminate at the same instant; since, as far as is yet known, the radius of a circle is not commensurable to its circumference or to its quadrant. To obviate this objection, Clavius proposes the following genesis:—Divide the quadrant D B into a number of equal parts, as D *a*, *a b*, *b c*, &c. which may be accomplished geometrically by repeated bisection to any required degree of minuteness. Divide the radius D A into a like number of equal parts, as D *k*, *k l*, *l m*, &c. Draw right lines from the centre A to the different divisions of the quadrant, *a*, *b*, *c*, &c.; and from the different divisions of D A, as *k*, *l*, *m*, &c. draw lines parallel to A B, intersect-

Fig. 1.

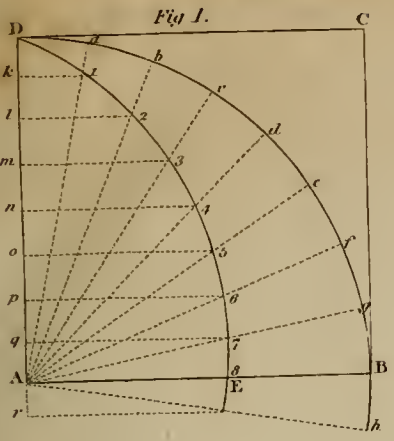



Fig. 2.

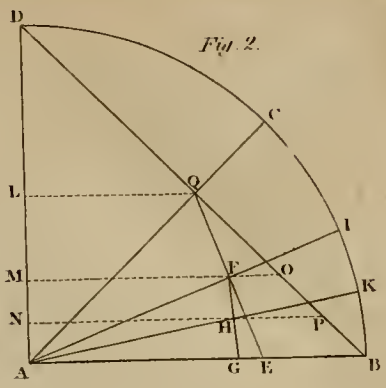



Fig. 3.

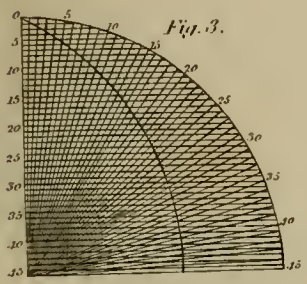


Fig. 4.

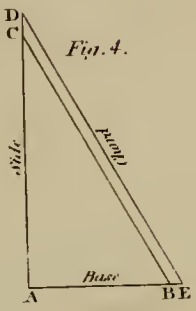
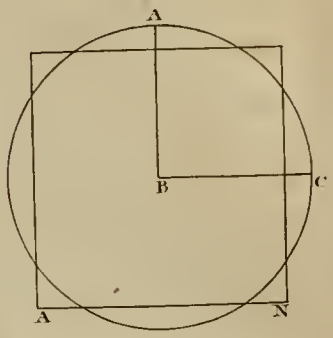



Fig. 6.



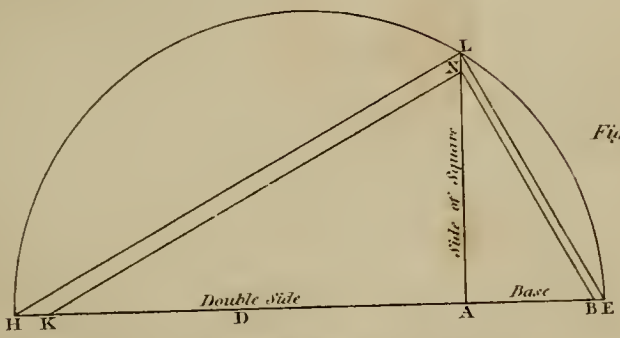
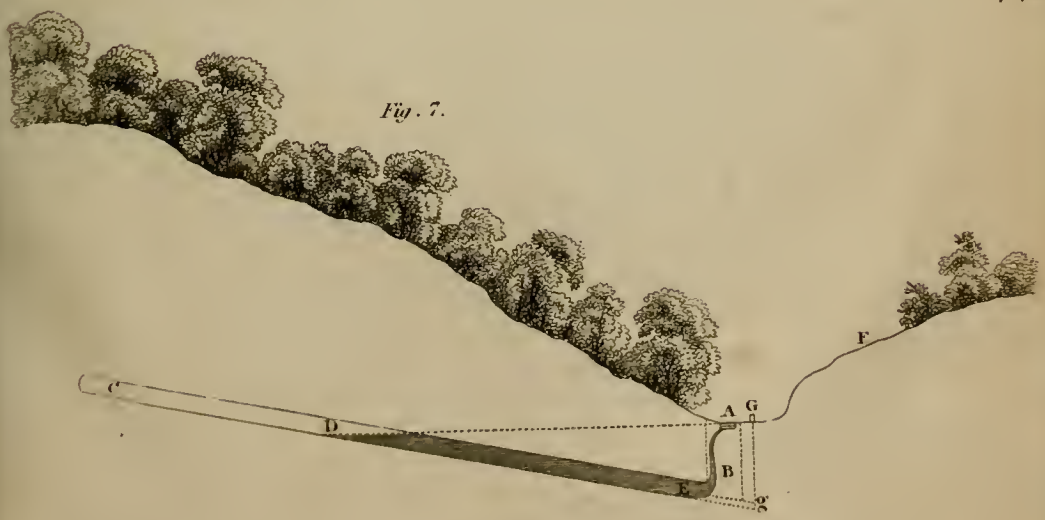




Fig. 5.



Fig. 7.

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ing the radii drawn to the corresponding divisions of the quadrant, in the points 1, 2, 3, &c. These points carefully joined will form a curve which is manifestly of the same nature with the quadatrix of Dinostratus and Nicomedes. To determine with the greatest possible accuracy the point, E, in which the curve intersects A B, the divisions should there be rendered more minute, and should be continued on the other side of A B, as at *r* and *h*. The point A is called the centre of the quadratrix; A D, its side; and A E, its base.

This mode of construction cannot reasonably be objected to, as it is admitted as geometrical in the case of the conic sections, which have been formed by ascertaining the position of any required number of points in the periphery of the curve by Apollonius, and all his followers down to the present day. In the case of the quadratrix, this manner of description is greatly more simple than in the case of the conic sections, which require the finding of means proportional, and other complex problems; whereas the above description requires nothing but the bisection of a right line and circular arch, and the drawing of parallel lines; and it is laid down by the immortal Newton, in the appendix to his Arithmetica Universalis, that "that curve ought to be the readiest admitted into geometry whose geometrical construction is the easiest, rather than that whose numerical calculation is the simplest." With respect to the objection that may be urged to this construction from the supposed incommensurability of the diameter and circumference of a circle, the effect of this incommensurability would be to prevent the curve from having a regular and uniform flexure when the points thus found were carefully joined: but this objection may be deemed of little importance, since, as will immediately appear, the application of the quadratrix to the solution of the problem of the quadrature of the circle depends solely upon the accuracy with which the length of its base, or the proportion which that bears to the radius of the quadrant, is determined. We shall, therefore, now proceed to show how the quadrature of the circle may be accomplished by means of this curve.

It is demonstrated by Pappus, in his Mathematical Collections, that "the quadrant circumference, the side, and the base, of the corresponding quadratrix are continually proportional." It follows from this proposition as a manifest corollary, that the side of the quadratrix, as D A (Fig. 1), is equal to that quadrantal arch whose radius is the base of the quadratrix, as A E. Because by the proposition D B : D A :: D A : A E, and D A : A E, as the quadrants of which they are respectively radii; whence the quadrant whose radius is A E must be just equal to D A, as D B is just the quadrant whose radius is D A. It follows, also, that if there be two right lines in the proportion of D A : A E, and the lesser be made the radius of any circle, the greater will be equal to the quadrant of the same circle.

From these premises we easily deduce the following method of

squaring the circle. It is proved by Archimedes that the rectangle under the radius and semi-perimeter of a circle is equal to its area. But the radius of a circle is to its quadrant as the base of the quadratrix is to its side. If, therefore, we find a fourth proportional to the base of that quadratrix, its side, and the radical of the given circle, it will be equal to the quadrantal arch of that circle; and the rectangle under twice this line and the radius will be equal to the area of the given circle, and may be converted into a square by the 14th prop. of Euclid, book ii.

Thus it appears that to find a square equal to a given circle, we have only to determine the proportion between the base and side of the quadratrix with accuracy in a single case; for this being determined, in any one case, we can, without any further assistance from this curve; proceed to the rectification of the circular perimeter. On this account we shall subjoin to the above method of describing the quadratrix another method, which Clavius has given, for determining the length of its base, and at the same time the position of several of its points, which is at once possessed of much simplicity and geometrical accuracy.

Let D B be a quadrant whose radii are A D, A B, to which it is required to find the corresponding quadratrix. (Fig. 2.) Draw the chord D B, and bisect it by the radius A Q C, which will cut it at right angles. (Eu. iii. 27.) From A B cut off A E equal to A Q. Join Q E, and bisect it by the radius A F I, which will manifestly be perpendicular to it, on account of the equal and corresponding triangles A F Q, A F E. Again, from A E cut off A G equal to A F. Join F G, and bisect it by the radius A H K, which, on similar principles, will also cut it at right angles. And this operation may be renewed till we have attained the requisite degree of accuracy. Then all the points, Q, F, H, &c. lie in the curve of the quadratrix, as may be thus shown. Draw through these points the lines Q L, F M, H N, parallel to A B, and cutting D B in O and P. Then, on account of parallel lines, we have D Q : Q B :: D L : L A (Eu. vi. 2); and consequently as D Q is equal to Q B, D L also is equal to L A, and is a point in the quadratrix, because the quadrant D B is bisected in C. Again, on account of parallel lines, as Q E is bisected in F, Q B also is bisected in O, and L A is bisected in M; and since the arch C B is bisected in I, F also is a point in the quadratrix. By similar reasoning, H is proved to be a point in the quadratrix, as well as all the other points that are found by this construction. And thus we may determine, to any required accuracy, the point where the base of the quadratrix cuts the radius A B.

Fig. 3 exhibits the quadratrix drawn by the first of the above methods to a considerable degree of accuracy, the quadrantal arch being divided into 45 equal parts or divisions of two degrees each, by means of a semicircular divided protractor, and the radius being geometrically divided into a like number of equal parts. By means of the other figures which follow, we shall be able to find the side

of a square equal to a given circle, or the radius of a circle equal to a given square, by a very short and accurate process, which is as follows:—

In Fig. 4, A D is the side of the quadratrix, and A E its base, as determined in Fig. 3. Let D E be joined, and apply the radius of the circle that is to be squared to the base A E. Then if it be precisely equal to A E, A D is equal to a quadrant of the given circle. But if it be less, as A B, through B draw B C parallel to the chord D E. Then is A C equal to a quadrant of the given circle; since, on account of parallel lines, A E : A B :: A D : A C. If the radius of the circle be greater than A E, we have only to produce A E to the requisite length, and draw a parallel as before; and thus we shall ascertain the sides of a rectangle equal to the given circle. To turn this rectangle into a square, let the line A H (Fig. 5) be double of A D, the side of the quadratrix, and A E equal to its base. Find a mean proportional to H A and A E by describing a semicircle on H E, and at the point A drawing A L perpendicular to H E; then shall A L be the side of a square equal to the rectangle under H A and A E, or equal to the circle whose radius is A E. Hence to find a square equal to any given circle, we have to join H L and L E, and to cut off from A E, or from A E produced, a line, A B, equal to the radius of the given circle. Then through B draw B N parallel to L E, and through N draw N K parallel to L H; when, on account of parallel lines, it is evident that H A : A E :: K A : A B; and that A N is a mean proportional between K A and A B; and consequently is the side of a square equal to the circle whose radius is A B, which square and circle are accordingly drawn in Fig. 6.

If it is required to find a circle equal to a given square, the above process may readily be reversed, as follows:—Let A N (Fig. 5 and 6) be the side of the given square, which is to be applied to A L, the perpendicular at A, or to A L produced. Through N draw N B parallel to L E, and A B shall be the radius of a circle equal to the given square. For L A : A E :: N A : A B; and A E is the radius of a circle equal to a square whose side is L A; consequently A B is the radius of a circle equal to a square whose side is N A, that is equal to the given square.

By means of the quadratrix, various other interesting problems analogous to the quadrature of the circle may be very readily resolved; such as the finding a circle of a circumference equal to a given right line; or the cutting off from a given circle a circumference equal to a given right line. Also the finding a right line equal to any given arch of a circle, the dividing a given angle or given arch of a circle in any assigned proportion, and the description of an isosceles triangle of which the angle at the base shall have any assigned proportion to the vertical angle. But we are prevented by the narrowness of our limits from entering into any further detail of the particulars.

## ARTICLE IV.

### *On the Divisions of Fahrenheit's Scale.*

(To Dr. Thomson.)

SIR, *March* 27, 1816.

HAVING been always in the habit of considering Fahrenheit's scale as perfectly arbitrary, my attention was arrested by a passage in p. 232, vol. vi., of the *Annals*, in which your Correspondent R. W. lays down a different doctrine. This has induced me to examine the subject again with some attention; and as I cannot adopt the same opinions as are stated in the place referred to, I take the liberty of offering you some remarks upon them.

Sir I. Newton was the first* who suggested the philosophical idea of forming the divisions of the thermometrical scale from the proportional parts, by which a given mass would be expanded by a certain addition of heat. Reaumur † also framed his scale upon the same principle. It is wrong, therefore, to confine this excellence to Fahrenheit's scale; and I believe it not even to have entered into his plan. The claims of Newton and Reaumur are not all which might be set up on this occasion; but the two volumes of the Philosophical Transactions and Memoirs of the Academy of Paris, to which I have referred, are sufficient to set side part of the credit attributed (and it seems to me too hastily attributed) to Fahrenheit. I wish I could as shortly dismiss the other part of the question; but if we enter a little more at large into it, I think we may collect with great probability that Fahrenheit was guided by no idea similar to that which I have just been mentioning.

I am uncertain whether R. W. derives his statement from Dr. Martine, ‡ or immediately from Boerhaave; § but from whichever of these writers he draws his information, I am ready to acknowledge that there is something in their method of expression which, if not compared with other parts of their remarks, might suggest the notion adopted by your Correspondent. Deluc, ‖ indeed (no mean authority on such a subject), appears to have understood them in some such sense; but still I am prepared to contend that a more careful investigation will prove that the truth lies on the other side.

If I wished merely to rest upon authorities, I might quote Dr. Martine himself, who says, in a subsequent passage, ** that the division was originally quite arbitrary, and Van Swinden, who

* Philosophical Transactions, 1701.
† Memoires de l'Academie des Sciences, 1730.
‡ Essay on the Construction and Graduation of Thermometers, sect. 24.
§ Chemistry, Shaw's Translation, 1741, vol. i. p. 235.
‖ Modification de l'Atmosphere, ii. partie, chap. ii., 430 b., 432 h.
** Sect. 27.

in his valuable treatise,* precisely says, "J'ignore ce qui peut avoir engagé Fahrenheit a diviser son echelle en 96 degrés," which we shall see hereafter to have been the origin of his scale. This, however, might leave the question in an unpleasant state of doubt, from the apparent contradiction of different testimonies. I prefer, therefore, going more fully into it, especially as the points connected with the history of this curious instrument cannot be without their interest.

In the first place, then, let us see what F. himself† says of his method: "Duo potissimum genera thermometrorum a me conficiuntur, quorum unum spiritu vini, alterum argento vivo repletur: longitudo varia est, pro usu cui inservire debent omnia autem in eo conveniunt, quod in omnibus scalæ gradibus concordent interque limites fixas variationes suas absolvant. Thermometrorum scala quæ meteorologicis observationibus solummodo inserviunt infra a zero incipit et 96$^{to}$ gradu finitur. Hujus scalæ divisio tribus nititur terminis fixis, quorum primus in infima scalæ parte reperitur et commixtione glaciei aquæ et salis armoniaci vel etiam maritimi acquiritur; huic mixturæ si thermometron immergatur fluidum ejus usque ad zero descendit.....Secundus terminus obtinetur, si aqua et glacies absque memoratis salibus commisceantur; immerso thermometro huic mixturæ fluidum ejus 32$^{um}$ occupat gradum..... Terminus tertius in 96$^{to}$ gradu reperitur et spiritus usque ad hunc gradum dilatatur si thermometrum in ore vel sub axillis sani hominis teneatur donec perfectissimi calorem corporis acquisiverit. ..... Thermometrorum scala, quorum ope ebullientium liquorum gradus caloris investigatur etiam a zero incipit et 600 continet gradus: hoc enim circiter gradu mercurius ipse (quo thermometrum repletum est) incipit ebullire."

From this extract we learn several curious points: 1. It appears that Fahrenheit originally graduated from three fixed points, the highest of which was very much below the temperature of boiling water. 2. That he precisely calls the fluid in his shorter thermometer "spiritus," whereas he calls it "mercurius" when he talks of his longer scale for ascertaining the points at which different fluids boil: and, 3. That he used the same divisions for both these thermometers. Indeed, Van Swinden enlarges upon all these points, and adds some further particulars, which are well worth detailing. He says,‡ "Fahrenheit.....dit dans une dissertation publiée en 1724.....qu' ayant lu il y avoit dix ans les experiences par les quelles Amontons a trouvé que la chaleur de l'eau bouillante est constante, il avoit pensé a construire un thermometre au moyen duquel il put suivre cette decouverte: qu'il en avoit essayé quelques uns, mais que ses efforts avoient eté inutiles; que des occupations

* P. 44. It may be right to add that Van Swinden's acknowledgment of ignorance is not to be attributed to any neglect of Boerhaave, since at p. 54 is the very passage on which the foundation of R. W. rests.

† Phil. Trans. vol. xxxiii. Old abridgment, vol. vii. p. 52.

‡ P. 47.

nombreuses l'avoient obligé de remettre ces recherches à une autre occasion et qu' enfin il etoit tombé sur l'idée d'un thermometre de mercure." From this it is clear, as Van Swinden remarks, and confirms by other reasons,* that Fahrenheit did not use boiling water in the original method of graduating his thermometer; and he thinks † it highly probable that F. first used mercury for filling his tubes about the year 1720; but he used the scale with his shorter thermometers as early as 1714. ‡ It is evident, therefore, that the 212 degrees are not to be derived from the effects of boiling water, nor from the relative expansion of mercury under any change of temperature. Indeed, if he had been guided by any such idea, it would be difficult to assign any reason for his assuming an inconvenient number, as Dr. Martine § justly calls it, like 11124, for the parts into which the fluid should be supposed to be divided at the temperature of 0. There is nothing certainly to lead to it in the simplicity which may be obtained from it for calculation. Dr. Martine likewise adds ‖ that Boerhaave is not consistent with himself, but that in other places he supposes a division of the mass into 10782 and 11520 parts, of which in each case 212 is the quantity increased between freezing and boiling water. The latter number has escaped me in my search, but I have found the former; ** and I have made several trials to reduce it to the first-mentioned division, but all without effect. Van Swinden, indeed, acknowledges †† himself unable to account for this variation; and the only cause to which I can attribute it is to some change in the estimate which Boerhaave made of the expansion of mercury; for this certainly entered into his, though not into Fahrenheit's, calculation. Indeed, there is no other way of understanding his meaning, but that mercury will expand $\frac{212}{11124}$, if the heat be increased from the temperature of F.'s freezing mixture to that of boiling water; or $\frac{96}{11124}$ from the same zero to the heat of a healthy man's body. Now when speaking of the expansion of spirits, ‡‡ he says, if we take a glass vessel terminating in a narrow tube carefully made, which contains 96 parts, whereof the lower vessel contains 1933, pure spirits of wine, which filled the lower vessel at the temperature of zero, would expand so as to fill the tube also when the heat of a healthy man was applied to it. In this case we have the expansion expressed by the fraction $\frac{96}{1933}$. Hence we see that Boerhaave wished in both instances to express a ratio; and if the scale had been formed upon any assumed division of the original mass, the number which expressed that division would have remained the same in both instances; whereas he retains the same numerator, and varies the denominator, which is to express the number of these parts: from whence I think we are fully justified in concluding that these numbers were not the foundation on which the scale was

* P. 50.
§ Sect. 27.
†† P. 54.
† P. 47.
‖ Sect. 25.
‡‡ Vol. i. p. 231.
‡ Van Swinden, p. 42.
** Vol. i. p. 231.

formed, but that the scale having previously been fixed, these numbers were accommodated to them.

But your Correspondent sees an advantage in this method of considering the subject; because it will make the thermometer denote, not only the temperature, but the expansion. No scale divided into equal parts can do this exactly; but still the principle was probably in Boerhaave's mind, although his numbers do not express it with any thing like accuracy. This, however, was by no means the case with Fahrenheit; nor does it give any peculiar excellence to his scale. For example, let us consider the centigrade thermometer: Sir G. Shuckburgh Evelyn says,* that "if quicksilver at freezing be supposed to be divided into 13119 parts, the increase of volume by the heat of boiling water under a pressure of 30 inches is 208 of those parts." Now 208 : 13119 :: 100 : 6307. Therefore if we conceive the mercury at the temperature of melting ice to be divided into 6307, the centigrade thermometer will then point out that this volume will be increased to 6407 of those parts by an increase of heat up to that of boiling water, and thus will be made to denote the expansion, as well as the temperature. Now I know no reason why we should conceive the original mass divided into 11156 rather than into 6307 parts. It is erroneous, therefore, to assert that "this indication of expansion is an advantage which no other thermometrical scale" but F.'s "possesses."

Still, however, it may be asked if all which is here laid down be true, what reason could Boerhaave have for giving these numbers? And to this a very plain answer may be given. It will be seen, by barely looking at Fig. 2 and 3 of B.'s Plate V., that he not only gives the same scale to what he calls "F.'s first or spirit thermometer," as he does to what he denominates his "second or mercurial thermometer," but he also places the zero of the scale in both cases at the insertion of the tube into the cylinder. Now by examining the passages where he supposes the mercury divided into 11124, and the spirits into 1933 parts, it will be seen that he does not take any given mass to be divided into these numbers of parts, but only that which will exactly fill his bulb or cylinder. Hence I conclude that it was not his intention to enter into any philosophical discussion of the scale, but merely to give practical rules for graduating particular instruments; so that the size of the bulb and the relative dimension of the tube being known, it might be easily ascertained at any temperature what quantity of the fluid must be introduced in order to have the lowest point of the scale at the very bottom of the tube. This seems to have been likewise the sense in which Dr. Martine † understood him; and it is further confirmed by the following passage from Muschoenbroek: ‡ "Cognoscitur quantum mercurius in tubo cuicumque gradui scalæ respondens dilatetur quia uti nunc est scala Fahrenheytii 600 graduum, capacitas ventris est

* Phil. Trans. vol. lxvii. p. 566.

† Sect. 25.

‡ Elementa Physices, Lugd. Bat. 1762, p. 630, sect. 1569.

ad eam tubi uti 11124 ad 600, et si scala inciperet ubi nunc 30 gradus sub 0, oportet ut ventris amplitudo sit ad eam tubi uti 11094 ad 600; vel uti 11124 ad 630." The numbers, likewise, might have been intended to have the further advantage of affording the means of deducing the relative expansion of the fluid from the different degrees on the scale, although it certainly does not derive the number of those degrees from the relative expansion.

Upon the whole, then, I must confess that I see no reason to think that F. derived his scale from any property of the expansion of either mercury or spirit of wine. If the origin of it is now to be detected, it must be looked for in some cause perfectly independent of either, and there are some circumstances which will probably lead to it.

It has been seen from his own account that his first short scale extended only from 0 to 96, and Van Swinden mentions * that he at first divided these into only 24 parts; but that, finding his degrees by this means too few, he afterwards increased them into four times that number. Now this seems to suggest a probable conjecture when it is combined with what V. S. † quotes from the Acta Erud. Lips. for 1714, p. 380. It is there stated that F. made two thermometers for Professor Wolf, and that the tube of each was divided into 26 degrees, and that each degree was divided into four subdivisions. The second of these degrees was marked "very great cold," and from thence to the top of the scale were 24 degrees. These thermometers were graduated to two degrees below 0, and names were affixed in the following order:—

0, very great cold,
4, great cold,
8, cold air,
12, temperate,
16, hot,
20, very hot,
24, insupportable heat.

This last is the same degree of heat with that of the human body; but the names here are only derived from the sensations produced by the weather; and although the frame of man can resist an heat of 96°, still, with reference to all European ideas of comfort, it may, with no great violence to language, be called "insupportable:" besides, in the early part of the last century there was little precise knowledge of what heat could be supported; and as Boerhaave ‡ fancied that the limit of natural cold was the zero of Fahrenheit, to which the mercury descended in 1709, § so it might have appeared to be impossible to bear a greater heat than that which was produced by the economy of the human body itself. Be this, however, as it may, it must strike every one, from the manner in which I have arranged the numbers, that they all go on in arith-

* P. 41, 43.
† P. 42, 43.
‡ Vol. i. p. 229.
§ Boerhaave, vol. i. p. 227.

metical progression with a common difference of 4. I think, therefore, that it is by no means improbable that F. had framed to himself an idea of six equal gradations of temperature from his point of extreme cold to that of insupportable heat; but six were too few for precision in observation, and he therefore was induced to divide them into quarters, which gave 24 degrees in the same manner as he afterwards introduced four subdivisions into each degree, which gives the numbers of our present scale, which answer to those above, and which are 0, 16, 32, 48, 64, 80, 96.

I have possibly made this already too long, and yet I am tempted to add one more remark on the subject in question. I turned to the article *Thermometer*, in the Encyclopædia Britannica, in order to try whether I could find any remarks which would serve me in the present inquiry; I found nothing: but when treating * of the division into 180° between the freezing and the boiling points, it is approved of as giving a sufficiently large number of degrees, but objected to on account of its quotients, when it is repeatedly divided, soon becoming an odd number. Now from what we have seen above, this objection applies only to the extension of the scale beyond what it was originally intended for, and few numbers could have been fixed upon which were better than 96. It must be fresh in every one's recollection that this is the very number into which Bird recommended to divide, and actually did divide, the quadrant.

S.

P. S. Plate 534 of the Enc. Brit. is a copy of Martine. It will be found also in Desagulier's Lectures, Hutton's Dict. and many other publications. As I see your name among those who are to contribute to the supplementary volumes of the Enc. Brit., let me suggest the improvement of giving Van Swinden's much superior plate of comparative scales. I looked for Musschoenbroek, and his name is not mentioned. The Editors, likewise, should be aware that Wallis's account of the Helmdon mantle-tree, to which a reference is made in the article *Arithmetic*, is supposed to be erroneous. On this subject they may consult the Archæologia, vol. xiii.

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## Article V.

*Remarks on the Wire-Gauze Lamp lately constructed by Sir H. Davy.* By Mr. John B. Longmire.

Sir Humphry Davy's lamp being now fully before the public, its merits and demerits may be freely discussed. Before entering upon the subject of this paper, I beg leave to remark that sometimes there are not wanting those who rather stigmatize the person,

* P. 403.

than refute the argument, of a controversialist: but although such conduct is too often resorted to by persons whose acquirements and situations in life ought to inspire them with more liberal sentiments, yet I do expect, from the known liberality of the person whose work I am now to animadvert upon, that any remark, either in the praise or dispraise of this lamp, will be received by every body interested with the same good will that it is delivered.

Sir Humphry Davy's lamp consists of a cylinder of wire-gauze, about six inches long, and two inches diameter, which is covered at the top by two layers of wire at about half an inch asunder, and which is fastened at the bottom to a brass ring that screws on to the body of the lamp containing the wick and oil.

By this contrivance, the air which passes to the inflamed wick passes only through the apertures of the wire-gauze; and when a mixture of air and inflammable gas enters, it either burns in the inside of the wire-gauze (but then the flame cannot escape through the apertures, nor the mixture which surrounds the lamp be set on fire); or, as when the quantity of gas is great, it extinguishes the light. The miner, therefore, may venture to travel through every part of the mine, whether foul or pure: for if he enters a mixture, and sees the inflammable air burning in the lamp, he may travel on, and perhaps pass through the foul part, if the inflammable air do not put out his light; and even then he can return in safety. A lamp so contrived is sure to give the collier *absolute* or *perfect security* against his most destructive foe—the inflammable air.

But this much desired, this perfect security, is to colliers what perpetual motion is to mechanics—a want! never to be obtained by human intellect. *Very few*, if any, practical miners, will come before the public, *and pledge their credit* on the unrestricted assertion that Sir Humphry Davy's lamp will give the perfect security so much boasted of. That the lamp when newly made, and constructed according to the rules laid down, will not set fire to the exterior inflammable air, must be admitted by all: but when its materials have begun to wear, an ample source of accident is opened; the fatal effects of which will be very much increased by the precarious tenor of the mine, and the uncertainty of human agency.

It would appear that even the construction of the lamp, containing in itself the seeds of future uncertainty, is a conclusion which may be drawn from Sir Humphry Davy's own words. He says, "Persons *appointed by the viewers* should *daily* inspect the lamps, and supply them with oil; and *to prevent* the possibility of *accidents* from the removal of the gauze cylinders, they may be fastened to the lamp by *small padlocks*; though, as *the imminent danger arising from such a circumstance* is obvious, the precautions, *it may be hoped*, will be unnecessary." Such precautions as these are entirely unnecessary were the lamp constructed to perfection. The man who inspects the lamp has a most arduous task to perform. He is to see that the apertures in every lamp (and there are from 14,000

to 16,000 of them,) are not more than $\frac{1}{30}$, or at most $\frac{1}{20}$, of an inch on the side, or the $\frac{1}{400}$ part of an inch is to be the greatest area. He must take care that the space between the parts of the screw is in no place more than the side of an aperture; and he has all this to do, besides trimming and oiling the lamps. He must have very keen eyes to check the apertures; but how he is to manage the screw is what I cannot advise him, *to a certainty*, and even Sir Humphry himself will be puzzled to do it. Sir Humphry Davy dare not trust the oiling and trimming of his lamp to the person who works with it; yet to be perfect, it ought to be safe in every person's hands; and the restraint thus laid upon its use militates against its great practical utility. If the lamps are to be padlocked by one person, when a collier loses his light he has to travel in the dark till he find the man with the key; that is to say, he is to travel, if he can find his way in the dark! If they are not padlocked, and every miner has the management of his own lamp, it is not to be supposed that every one will always go to the place fixed on to light the lamp: when they are in separate workings, and alone, they will light them in less distant places, which they think suitable, but which will not be *always safe*. The lamp, therefore, as to construction, neither yields a desirable degree of convenience nor absolute security.

In practice the wire-gauze lamp will be liable to accidents, and of course insecure. If we suppose the lamps to be always constructed properly, which might admit of a doubt, on their introduction into the mine, the gauze rusts much faster than in common air, by the alternate action of heat and cold, and air and water, which last, in coal-mines, corrodes iron very fast; so that in a few months the rusted parts would fall off in flakes, or be detached by a slight blow against any hard substance; when the rust begins to fall off, the apertures enlarge, uncertainty commences, and may end in misfortune. The apertures will be increased in size from other causes. If the lamp strike sideways against any sharp pointed instrument, this penetrates the lamp, and increases the size of an aperture; or if the lamp scrape at the projecting part of the cone, or stone wall, the arrangement of the apertures will be disturbed, some of them being enlarged, and others lessened. In both cases it is highly improbable that the change can always be observed before some fatal accident has resulted from it. That the lamp is not perfectly secure, either in construction or practice, will now, I think, appear sufficiently evident.

Did we for a moment take it for granted that the wire-gauze lamp was infallible, still coal-miners are not altogether secure from the destructive effects of inflammable air. It may be set on fire by the ventilating furnace. The chance of its being so might indeed, as has been some time suggested, be much lessened by using charcoal well burnt, instead of coal. But suppose an explosion take place, whether would the steel mill or wire-gauze lamp be the more useful mode of getting light? The steel mill, most undoubtedly! The

light of the wire-gauze lamp would soon be put out by the noxious gases of combustion, but the steel mill would continue to give light. If by accident the light in the lamp of a person going to find the burned miners is put out, the object of the journey is at least retarded; and a delay under such circumstances is in the highest degree an injury. In a common working, where the air is only slightly mixed, the wire-gauze lamp may be preferred, as being less expensive, and less troublesome; but when the lamp goes out, the mill may be used. The steel mill has always been, and perhaps ever will be, the miner's last resource. It can be used with safety where neither candle nor lamp can burn, and it is in all situations much safer than any other means of giving light.

To conclude, I am of opinion that the wire-gauze lamp at present enjoys a greater reputation than it will retain. It has been hitherto contemplated through the beauty and novelty of the experiment; while the great obstacles it has to meet with in the mine have not been taken into the question so much as they ought. A few years' experience will give it that place in the estimation of miners which *only its real utility* will obtain for it.

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## ARTICLE VI.

*On the astonishing Power which Spiders have of conveying their Threads from one Point to another at a considerable Distance from it, without any intermediate Communication.* By M. W. Carolan.

(To Dr. Thomson.)

SIR,

I HAVE frequently remarked, with some surprise, the deficiency of information amongst naturalists with respect to the method by which the weaving spiders fix their threads between two points, distant from each other perhaps several feet, or yards, without any visible means by which they can accomplish it. The writers of some date who treat of the natural history of spiders were not only ignorant how this was done, but did not even perceive that they needed to be informed respecting it. They generally seem to have supposed that the thread which the spiders left behind them bore them up. This every one knows is a great mistake; for if a spider is suspended by the end of its thread, however long the thread may be, it drops down. Those who have written more recently on this subject, both observe the curious fact, and confess that it yet remains to be satisfactorily explained.

Mr. Kirby, in his excellent Introduction to Entomology, endeavours partly to account for this circumstance, by supposing that when the geometrical spider has climbed up the side of any thing standing nearly perpendicular, it fixes its thread, goes down again,

and cuts the thread which it has let out at the bottom of the ascent; and the line being extremely buoyant, immediately floats in the air, and catching hold of whatever is within its length, the spider in this way makes a conveyance to go across. But, as Mr. Kirby observes, this cannot be the case where the threads are drawn several feet long between two stalks not a foot higher than the ground, as the threads formed by the process above-mentioned can never be longer than the height of the ascent. Neither can it account for the extension of long lines across pools of water fixed to the tops of short bulrushes; nor when threads are drawn between the outermost branches of trees.

Mr. Kirby also gives us the opinion of another person, who informs us that he has seen spiders standing and shooting out threads in any direction they pleased, and to any length, which, by some power unknown to us, are pointed to any particular spot to which they wish them to fasten. But although some species of spiders do possess this power, it cannot sufficiently account for the regular way in which the long threads are placed, which support the nets of the geometrical spider, as the most gentle zephyr in this case would easily destroy that uniformity and tenseness which the threads are always observed to hold, lying in general quite parallel to each other. A spider's thread, when very long, does not always keep stretched out while flying in the air, and consequently, although the end did fasten to something, we would often find it hanging in a curved form; but we seldom or never meet with any of these lines, but quite straight and tight.

Mr. Kirby remarks that Pliny thought it much to the discredit of the naturalists of his time that, while they were disputing about their mythological statues, they were not able to tell whether the queen bee had a sting or not. And he thinks it not less surprising that the entomologists of the present day should not be able to inform us how the geometrical spider fixes the long lines that support her net.

Having often formed various conjectures respecting the method by which there was any probability of these spiders performing this, I at length concluded, from strong presumptive evidence, that it would be almost impossible to account for it in any other way than by supposing the spiders to have the power of flying; but this appeared so contrary to the general economy of nature (as we have no instance of any creature flying without wings of some description), that although I could hardly help believing it, yet it required actual observation to render it an unquestionable fact. All doubt with me, however, was at last removed by the following incident. Some years ago, while sitting by the side of a plantation, I observed a pretty large spider crawl up to the top of one of the highest stalks of a small bush of rushes, and after resting a short time, it took its flight almost straight upwards, and alighted on a leaf, at the end of one of the branches of a tree about nine or ten feet above the place where it set off. I was not so much surprised at this, as I was

partly prepared for something of the kind, as highly gratified at the confirmation of my former suppositions. There was no thread previously drawn between the two places, neither was it wafted up by any wind, the weather being quite calm, a clear sunshine, and the situation well sheltered by trees, as far as I can recollect. It also moved a great deal quicker, and more smoothly, than if it had ascended by a thread, its flight exactly resembling that of a common fly. If from this single instance we might ascribe this power to all the spiders of the weaving class that spin long threads, it would completely account for this surprising phenomenon. At any rate, it may lead to experiments which may more fully establish the fact. I never was so fortunate as to observe another fly.

The idea of the geometrical spiders possessing this property in general, will appear quite probable if we take into view the following considerations. They are, for the most part, very light, in comparison to their size. Their legs also, from their length, might assist them in part to fly, or to direct their course in the air. Those at the sides, if moved in a vibratory way, might act, either in raising them, or, like oars, help to push them forward; while those behind, when stretched out, might serve as a tail, as in some kinds of flies. The power they have of springing pretty high might facilitate their offset; and the thread which they let out having a tendency rather to rise in the air than fall, might contribute still further to aid them. Besides, as they fly very fast, it is possible that the lines sent quickly from their spinners might act in some small degree like the tail of a rocket, which, when ignited, makes the rocket fly in any direction to which it is pointed. But it is very possible that the principal means by which these spiders may be enabled to fly with ease might be in consequence of having a vesicle for containing air in some part of their body, resembling the swim of a fish, by which it is enabled to rise from the bottom to the surface of the water. Supposing the geometrical spiders, &c. to have a vesicle of this description, with the power of filling it with a gas lighter than the atmospheric air, they would easily, with the concurrence of the circumstances before mentioned, move through the air to any distance. Naturalists generally believe that birds have small air vesicles in some parts of their body, which are filled, when they wish to rise, with a decomposed light air, rendering them more buoyant. Carry this idea just a little further, and we can easily conceive a spider to fly, and direct its course to any point it inclined. I have sometimes dissected one or two, but could never discover a vesicle of this kind; but it is evident, if there is any, it must be extremely difficult to perceive it, except when inflated; and this is not likely to be the case when the creature is dead. It is likely that the faculty of rendering themselves lighter may assist in enabling them to stop suddenly when descending rapidly from a height through the air, by letting out their line; for if you give the thread, after they have stopped, a small tug, they are obliged to descend further, apparently against their will; so that

it would appear they have not the power of closing their spinners so as to prevent the possibility of the threads being drawn out by a small additional impetus. They, indeed, appear always to be letting out their thread whenever they crawl, as a kind of security. This is particularly observable if we look over a ploughed field in the autumnal months, while the dew is lying. The whole field often appears quite covered with spiders' threads between the ridges. The geometrical spiders seem to take great pleasure in drawing many long threads between the trunks of trees, where they are plentiful, especially in autumn. They have no end seemingly in view while doing this, as there are no nets attached to them. In passing through some woods we may see thousands of these threads in every direction between the trees, often several yards long. And every one must have felt in such a situation the necessity of putting their hands frequently to their faces to rub off the lines which constantly come across them as they walk along, producing frequently a very tickling sensation.

M. W. Carolan.

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## Article VII.

### *Experiments on Prussic Acid.* By M. Gay-Lussac.

(*Continued from* Vol. VII. p. 364.)

### II. *Of Cyanogen, or the Radical of Prussic Acid.*

I discovered the peculiar gas called cyanogen by decomposing the cyanuret of mercury by heat. But as the cyanuret of mercury varies in its composition, and in that case does not furnish the same products, I shall begin by describing how it ought to be prepared.

By digesting red oxide of mercury with Prussian blue, we obtain a cyanuret perfectly neutral, which crystallizes in long four-sided prisms truncated obliquely. We may, by repeated evaporations and crystallizations, free it from the iron which it contains; but I think it better to boil it, as M. Proust has prescribed, with peroxide of mercury, which completely precipitates the oxide of iron, and I then saturate the excess of oxide of mercury with a little hydrocyanic acid, or even with muriatic acid. It is cyanuret thus prepared that I decompose by heat, in order to obtain the radical; but for common experiments we may dispense with these precautions.

When this cyanuret is boiled with peroxide of mercury, it dissolves a considerable quantity of the oxide, becomes alkaline, crystallizes no more in prisms, but in small scales, and its solubility appears a little increased. When evaporated to dryness, it is very easily charred, which obliges us to employ only the heat of the water bath. This compound, which might have been distinguished by the name of sub-prussiate, was observed by M. Proust. (*Ann. de*

*Chim.* vol. lx. p. 228.) When decomposed by heat, it gives abundance of cyanogen, but mixed with carbonic acid gas and azote. M. Proust says that we obtain ammonia, oil in considerable abundance, carbonic acid, azote, and oxide of carbon. For my part, though I endeavoured to discover the ammonia and oil, I was not able to perceive the least trace of them.

The accuracy of M. Proust was too well known to me to lead me to doubt of the results which he had obtained. I was, therefore, led to suspect that the cyanuret which he had employed was very different from mine. But at last, after some examination, I have discovered the cause of the difference in our results. M. Proust employed a moist cyanuret, while mine was very dry; and had not water been present, the discovery of cyanogen could hardly have escaped him. The cyanuret of mercury, when neutral and quite dry, gives nothing but cyanogen; but if it be moist, it furnishes only carbonic acid, ammonia, and a great deal of hydro-cyanic vapour. When we employ the alkaline cyanuret moist, we obtain the same products, but in different proportions, and likewise azote and a brown liquid, which M. Proust has taken for an oil, though it be not so in reality.

Therefore to obtain pure cyanogen, it will be sufficient to employ a neutral cyanuret of mercury in a state of perfect dryness. If I had been aware at first of the influence of moisture, the analysis of cyanogen would have cost me much less time; for then the variable proportions of hydrogen which it contained, and the anomalies resulting from them, would not have so much embarrassed me.

As I think I have demonstrated that the neutral combination known by the name of prussiate of mercury is a cyanuret, it would appear that the alkaline combination of which I have just spoken is a sub-cyanuret. But the case is not so. It is a compound of oxide of mercury and cyanogen, analogous to many other compounds of this kind, which have not yet been sufficiently observed, and to which I draw the attention of chemists. Thus when the deuto-chloruret of mercury is decomposed by potash, we obtain, provided the alkali be not in excess, a brick-coloured precipitate, which is a triple compound of chlorine, oxygen, and mercury, or a binary compound of oxide of mercury with the chloride of that metal. It is obvious that we cannot call it a sub-chloride of mercury. The proper name would be *oxychloride of mercury*. The sulphureted oxide of antimony, which ought likewise to be distinguished by the name of *oxysulphuret of antimony*, and many other combinations are in the same case. I shall remark, on this occasion, that the complex combinations resulting from the union of two binary compounds require, in general, that there should be an element common to each compound. The salts furnish a great number of examples of this, especially the triple salts, which are constantly formed from two salts of the same genus, and it would be difficult to point out combinations between a chloride or a sulphuret, and a salt properly so called. I now return to the method of preparing cyanogen.

When cyanuret of mercury is exposed to heat in a small retort or tube shut at one extremity, it soon begins to blacken. It then appears to melt like an animal matter, and then the cyanogen is disengaged in abundance. This gas is pure from the beginning of the process to the end, provided always that the heat be not very high; for if it were sufficiently intense to melt the glass, a little azote would be disengaged. Mercury is volatilized with a considerable quantity of cyanuret, and there remains a charry matter of the colour of soot, and as light as lamp-black. I shall give an account of it afterwards. The cyanuret of silver likewise gives out cyanogen when heated; but the cyanuret of mercury is preferable to every other.

Cyanogen is a permanently elastic fluid. Its smell, which it is impossible to describe, is very strong and penetrating. Its solution in water has a very sharp taste. It burns with a bluish flame, mixed with purple. Its specific gravity compared to that of air is 1·8064. I obtained it by weighing at the same temperature, and under the same pressure, a balloon of about 2½ litres (152·56 cubic inches) of capacity, in which the vacuum was made to the same degree, and alternately full of air and cyanogen. The data of the experiment are as follow:—

| | | |
|---|---|---|
| Weight of the balloon empty ..... | A + | 0·086 gramme |
| ——————— full of air ..... | A + | 2·824 |
| ——————— cyanogen.. | A + | 5·032 |

On dividing the weight of the cyanogen by that of the air, we obtain the number 1·8064. I have neglected the effect of humidity, because, not knowing it exactly, the correction would have been uncertain. Besides, it is so small that it may be neglected.

Cyanogen is capable of bearing a pretty high temperature without being decomposed. Water, with which I agitated it for some minutes, at the temperature of 68°, absorbed almost 4½ times its volume. Pure alcohol absorbs 23 times its volume. Sulphuric ether and oil of turpentine dissolve at least as much as water; but I did not attempt to ascertain the quantity exactly.

Tincture of litmus is reddened by cyanogen. On heating the solution, the gas is disengaged, mixed with a little carbonic acid, and the blue colour of the litmus is restored. The carbonic acid is no doubt owing to the decomposition of a small quantity of cyanogen and of water. It deprives the red sulphate of manganese of its colour, a property which hydro-cyanic acid does not possess. This is a proof that its elements have more mobility than those of the acid. By the dry way it separates their acid from the carbonates.

Among the simple bodies which I placed in contact with cyanogen, in a temperature produced by a spirit lamp, which is incapable of melting glass, I found that phosphorus, sulphur, and iodine, might be volatilized in it without undergoing any change. Its mixture with hydrogen was not altered by the same temperature, nor by passing electrical sparks through it. Copper and gold do

not combine with it; but iron, when heated almost to whiteness, decomposes it in part. It is covered with a slight coating of charcoal, and becomes brittle. The undecomposed portion of gas is mixed with azote. In one trial the azote constituted 0·44 of the mixture, but in general it was less. Platinum, which had been placed beside the iron, did not undergo any alteration. Neither its surface, nor that of the tube, was covered with charcoal, like the iron.

In the cold, potassium acts only slowly on cyanogen, because a crust is formed on its surface, which presents an obstacle to the mutual action. On applying the spirit-lamp, the potassium becomes speedily incandescent; the absorption of the gas begins, the inflamed disc gradually diminishes, and when it disappears entirely, which takes place in a few seconds, the absorption is likewise at an end. Supposing we employ a quantity of potassium that would disengage 50 parts of hydrogen from water, we find that from 48 to 50 parts of gas have disappeared. On treating the residue with potash, there usually remains four or five parts of hydrogen; sometimes 10 or 12. I have made a great number of experiments to discover the origin of this gas. I think that I have at last succeeded. It is derived from the water which the cyanuret of mercury contains when it has not been sufficiently dried. Hydro-cyanic vapour is then produced, which, when decomposed by the potassium, leaves half of its volume of hydrogen. Before I was aware of this cause, I concluded, from the variation in the quantity of hydrogen, that it did not proceed from the cyanogen. But it is much more satisfactory to know to what source it is to be ascribed. From this experiment, I shall draw as a consequence that potassium absorbs a volume of cyanogen equal to that of the hydrogen which it would disengage from water.

The compound of cyanogen and potassium is yellowish. It dissolves in water without effervescence, and the solution is strongly alkaline. Its taste is the same as that of hydro-cyanate of potash, of which it possesses all the properties.

This experiment is doubtless very instructive; but it is not sufficient to make us acquainted with the true nature of cyanogen. This gas being very inflammable, I detonated it in Volta's eudiometer with about 2½ times its volume of oxygen. The detonation is very strong; the flame is bluish, like that of sulphur burning in oxygen.

Supposing us to operate on 100 parts of cyanogen, we find, after the detonation, a diminution of volume, which amounts from about four to nine parts. When the residue is treated with potash or barytes, it diminishes from 195 to 200 parts, which are carbonic acid gas. The new residue, analyzed over water by hydrogen, gives from 94 to 98 parts of azote, and the oxygen which it contains, added to that in the carbonic acid, is equal (within four or five per cent.) to that which has been employed.

Neglecting the small differences which prevent these numbers

from having simple ratios to each other, and which, like the presence of hydrogen, depend upon the presence of a variable portion of hydro-cyanic vapour in the cyanogen employed, proceeding from the water left in the cyanuret of mercury, we may admit that cyanogen contains a sufficient quantity of carbon to produce twice its volume of carbonic acid gas; that is to say, two volumes of the vapour of carbon and one volume of azote, condensed into a single volume. If that supposition is exact, the density of the radical derived from it ought to be equal to the density derived from experience; but supposing the density of air to be one, twice that of the

| | |
|---|---|
| vapour of carbon is ..... | 0·8320 |
| That of azote..... | 0·9691 |
| Total ..... | 1·8011 |

The density of cyanogen, then, calculated from the preceding analysis, would be 1·8011; and as I found by experiment 1·8064, we are entitled to conclude, from the agreement of these numbers, that this analysis is correct, and that we may neglect the slight differences observed, the true cause of which appears known.*

On comparing the analysis of cyanogen with that of hydro-cyanic acid, it will be seen that, by adding a volume of hydrogen to a volume of cyanogen, we obtain exactly two volumes of hydro-cyanic vapour. Hence it follows that the density of this last is equal to half the sum of that of cyanogen and of hydrogen. This result is analogous to that which chlorine and iodine give us; for each of them combines with its own volume of hydrogen to produce two volumes of muriatic and hydriodic gases.

It is now easy to ascertain that the action of potassium on cyanogen agrees with its action on hydro-cyanic acid. We have seen that it absorbs 50 parts of the first, and likewise that it absorbs 100 parts of the second, from which it separates 50 parts of hydrogen. But 100 parts of hydro-cyanic vapour minus 50 parts of hydrogen amount exactly to 50 parts of cyanogen. Hence the two results agree perfectly, and the two compounds obtained ought to be identic, which agrees perfectly with experiment.

The analysis of cyanogen appearing to me of the greatest importance, I have attempted it likewise by other methods. Having put cyanuret of mercury into the bottom of a glass tube, I covered

* As I was not acquainted with the influence of water till after having taken the specific gravity of cyanogen, it is probable that this gas contained a small quantity of hydro-cyanic vapour, which ought to have diminished its density a little.

It is easy to explain the diminution of volume which we observe after the detonation of cyanogen and oxygen, as well as the deficit of carbonic acid, azote, and oxygen, by the presence of a little hydro-cyanic vapour. When this is detonated with oxygen, a diminution takes place equal to $\frac{3}{4}$ of the vapour. It produces but one volume of carbonic acid; while cyanogen produces two. It gives only half a volume of azote; and it contains hydrogen, which causes $\frac{1}{4}$ of the oxygen to disappear. On detonating cyanogen, I have not observed nitrous acid; but the formation of that acid does not depend solely on the presence of azote in the combination; it depends likewise on the mode in which it exists in it.

it with brown oxide of copper, and then raised the heat to a dull red. On heating gradually the part of the tube containing the cyanuret, the cyanogen was slowly disengaged, and passed through the oxide, which it reduced completely to the metallic state. On washing the gaseous products with potash, at different parts of the process, I obtained only from 0·19 to 0·30 of azote, instead of 0·33, which ought to have remained, according to my analysis. Presuming that some nitrous compound had been formed, I repeated the experiment, covering the oxide with a column of copper filings, which I kept at the same temperature as the oxide. With this new arrangement the results were very singular; for the smallest quantity of azote which I obtained during the whole course of the experiment was 32·7 in 100 of the gas, and the greatest was 34·4. The mean of all the trials was—

| | |
|---|---|
| Azote | 33·6 |
| Carbonic acid | 66·4 |

a result which shows most clearly that cyanogen contains two volumes of the vapour of carbon and one volume of azote.

In another experiment, instead of passing the cyanogen through the oxide of copper, I made a mixture of one part of cyanuret of mercury and 10 of the oxide; and after introducing it into a glass tube, close at one end, I covered it with copper filings, which I began by raising to a red heat. On heating the mixture successively, the decomposition went on with the greatest facility. The proportions of the gaseous mixture were less regular than in the preceding experiment. Their mean was—

| | | |
|---|---|---|
| Azote | 34·6 | instead of 33·3 |
| Carbonic acid | 65·4 | 66·6 |

In another experiment I obtained

| | |
|---|---|
| Azote | 32·2 |
| Carbonic acid | 67·8 |

Now if we take the mean of these results, we get

| | |
|---|---|
| Azote | 33·4 |
| Carbonic acid | 66·6 |

I paid attention to the water which might have been formed during these analyses; but no sensible quantity of it could be discovered. This shows still further that what has been called prussiate of mercury is a cyanuret of that metal. It appears demonstrated by these experiments that in this last compound the carbon is to the azote in the same proportion as in cyanogen. Yet if this be the case, how comes a charry matter to remain when the cyanuret is decomposed by heat? This difficulty embarrassed me for some time. But I conceive that I have at last succeeded in solving it. I have observed that when the cyanuret of mercury is exposed to too high a temperature, the cyanogen towards the end of the process was mixed with from seven to eight per cent. of azote. It remained

only, therefore, to analyse the charry residue, to see whether it contained azote, and in what proportion. Its weight for a quantity of cyanuret, which furnished three litres (183·084 cubic inches), is about 0·25 gramme (3·81 grains). A portion of this matter, calcined with red oxide of mercury, left no residue. Another portion was mixed with a great excess of oxide of copper, and heated in a glass tube. As I had not put copper over the mixture, the gas which was disengaged had a nitrous smell, and became sensibly red when mixed with air, though the proportion of nitrous gas did not exceed five or six per cent. 100 parts of the gaseous mixture, washed with a solution of potash, left a residue of 32 parts, which still retained a slight nitrous smell, so that the proportion of azote ought to be somewhat less. But if to this quantity of azote were added that which is disengaged at the end of the distillation of the cyanuret of mercury, its proportion to that of the quantity of carbonic acid would approach very closely to that of one to two, which the other analyses have given. I regret, however, that I have not determined more exactly the proportion of these different products.

Now that we know the nature of cyanogen, let us examine how it is affected by the alkaline bases.

When a pure solution of potash is introduced into this gas, the absorption is rapid. If the alkali be not too concentrated, and be not quite saturated, it is scarcely tinged of a lemon-yellow colour. But if the cyanogen be in excess, we obtain a brown solution, as it were, carbonaceous. On pouring potash combined with cyanogen into a solution of black oxide of iron, and adding an acid, we obtain Prussian blue. It would appear from this phenomenon that the cyanogen is decomposed the instant it combines with potash; but this conclusion would be premature. I shall show that when this body is decomposed by means of an alkaline solution, carbonic acid is always produced, together with hydro-cyanic acid and ammonia. But on pouring barytes into a solution of cyanogen in potash, no precipitate takes place, which shows that no carbonic acid is present. On adding an excess of quick-lime, no trace of ammonia is perceptible. Since, then, no carbonic acid and ammonia have been formed, water has not been decomposed, and consequently no hydro-cyanic acid evolved. How, then, comes the solution of cyanogen in potash to produce Prussian blue, with a solution of iron and an acid? The following is the solution of this difficulty.

The instant an acid is poured into the solution of cyanogen in potash, a strong effervescence of carbonic acid is produced, and at the same time a strong smell of hydro-cyanic acid becomes perceptible. Ammonia is likewise formed, which remains combined with the acid employed, and which may be rendered very sensible to the smell by the addition of quick lime. Since, therefore, we are obliged to add an acid in order to form Prussian blue, its formation occasions no further difficulty.

Soda, barytes, and strontian, produce the same effect as potash.

We must, therefore, admit that cyanogen forms particular combinations with the alkalies, which are permanent till some circumstance determines the formation of new products. These combinations are true salts, which I consider as analogous to those formed by acids. In fact, cyanogen possesses acid characters. It contains two elements, azote and carbon, the first of which is strongly acidifying. It reddens the tincture of litmus, and neutralizes the bases. On the other hand, it acts as a simple body when it combines with hydrogen; and it is this double function of a simple and compound body which renders its nomenclature so embarrassing.

Be that as it may, the compounds of cyanogen and the alkalies, which I shall distinguish by the generic name of *cyanurets*, do not separate in water, like the alkaline chlorides, which produce chlorates and muriates. But when an acid is added, there is formed, 1. Carbonic acid, which corresponds to the chloric acid. 2. Ammonia and hydro-cyanic acid, which correspond to the muriatic acid.

As we may obtain colourless cyanurets, it was necessary to examine in what proportions the carbonic acid, ammonia, and hydrocyanic acid, are formed, when the cyanurets are decomposed by an acid. But the absorption of the carbonic acid by the mixture of acid and potash which we are obliged to employ, rendering this inquiry somewhat uncertain, I got over the difficulty in the following manner:—I procured two small glass measures, one for the alkali, and the other for the acid; so that, when the measures of acid and alkali were mixed, the whole of the acid was not neutralized. After this disposition, I put into a graduated tube 149 parts of carbonic acid, which I absorbed by a measure of potash. Then I introduced into the tube one measure of muriatic acid. Only 140 parts of the gas were disengaged; consequently nine parts remained dissolved in the acid muriate of potash.

I then took 147 parts of cyanogen. I absorbed them by one measure of potash, and then added one measure of muriatic acid. I obtained 141 parts of carbonic acid gas. But as I knew that it contained a little hydro-cyanic vapour, I placed it in contact with the red oxide of mercury. The 141 parts were reduced to 137. This number differs so little from 138, which I ought to have obtained according to the first experiment, that we may admit, without hesitation, that when the cyanuret of potash is decomposed by an acid, there is produced a volume of carbonic acid gas just equal to that of the cyanogen employed. It remains, then, for us to ascertain what becomes of the other volume of the vapour of carbon; for the cyanogen contains two, and likewise one volume of azote.

Since there is produced at the expense of the oxygen of the water a volume of carbonic acid, which represents one volume of oxygen, two volumes of hydrogen must likewise have been produced. Therefore, neglecting the carbonic acid, there remains

1 volume vapour of carbon,
1 volume of azote,
2 volumes of hydrogen.

and we must make these three elements combine in totality, so as to produce only hydro-cyanic acid and ammonia.

But the volume of vapour of carbon with half a volume of azote and half a volume of hydrogen produces exactly one volume of hydro-cyanic acid, while the volume and a half of hydrogen, and the half volume of azote remaining, produce one volume of ammoniacal gas; for it will be recollected that this substance is formed of three volumes of hydrogen and one of azote condensed into two volumes.

A given volume of cyanogen, then, combined first with an alkali, and then treated with an acid, produces exactly

1 volume of carbonic acid gas,
1 volume of hydro-cyanic vapour,
1 volume of ammoniacal gas.

It is very remarkable to see an experiment, apparently very complicated, give so simple a result.

The metallic oxides do not appear capable of producing the same changes on cyanogen as the alkalies. Having precipitated proto-sulphate of iron by an alkali, so that no free alkali remained, I caused the oxide of iron (mixed necessarily with much water) to absorb cyanogen, and then added muriatic acid. But I did not obtain the slighest trace of Prussian blue; though the same oxide, to which I had added a little potash before adding the acid, produced it in abundance.

I should from this result be induced to believe that oxide of iron does not combine with cyanogen; and so much the more, because water impregnated with this gas never produces Prussian blue with solutions of iron, unless we begin by adding an alkali. The peroxide of manganese, that of mercury, and the deutoxide of lead, absorb cyanogen, but very slowly. If we add water, the combination is much more rapid. With the peroxide of mercury, we obtain a greyish-white compound, somewhat soluble in water. I have not sufficiently examined what takes place in these different circumstances.

Cyanogen rapidly decomposes the carbonates at a dull red heat, and cyanurets of the oxides are formed. When passed through sulphuret of barytes, it combines without disengaging the sulphur, and renders it very fusible, and of a brownish-black colour. When put into water, we obtain a colourless solution, but which gives a deep brown maroon colour to muriate of iron. What does not dissolve contains a good deal of sulphate, which is doubtless formed during the preparation of the sulphuret of barytes.

On dissolving cyanogen in the sulphureted hydro-sulphuret of barytes, sulphur is precipitated, which is again dissolved when the liquid is saturated with cyanogen, and we obtain a solution having a

very deep brown-maroon colour. This gas does not decompose sulphuret of silver nor of potash.

Cyanogen and sulphureted hydrogen gas combine slowly with each other. A yellow substance is obtained in fine needles, which dissolves in water, does not precipitate nitrate of lead, produces no Prussian blue, and is composed of one volume of cyanogen and $1\frac{1}{2}$ volume of sulphureted hydrogen. *

Ammoniacal gas and cyanogen begin to act on each other whenever they come in contact; but some hours are requisite to render the effect complete. We perceive at first a white thick vapour, which soon disappears. The diminution of volume is considerable, and the glass in which the mixture is made becomes opake, its inside being covered with a solid brown matter. On mixing 90 parts cyanogen and 227 ammonia, they combined nearly in the proportion of 1 to $1\frac{1}{2}$.

This compound gives a dark orange-brown colour to water, but dissolves only in a very small proportion. The liquid produces no Prussian blue with the salts of iron. I have not subjected it to other trials.

In describing the properties of hydro-cyanic acid, I have not spoken of the way in which the galvanic battery acts on it, because I could not then have explained the products obtained. On subjecting this acid to the action of a battery of 20 pair of plates, much hydrogen gas is disengaged at the negative pole, while nothing appears at the positive pole. The reason is, that cyanogen is evolved at that pole which remains dissolved in the acid. We may in this manner attempt the combination of metals with cyanogen, placing them at the positive pole.

It is easy now to determine what is formed when an animal matter is calcined with potash or its carbonate. A cyanuret of potash is formed, as I shall now show.

I have proved that by heat potash separates the hydrogen of the hydro-cyanic acid. We cannot, then, suppose that this acid is formed while a mixture of potash and animal matters is exposed to a high temperature. I say, further, that we obtain a cyanuret of potash, and not of potassium; for this last, when dissolved in water, gives only hydro-cyanate of potash, which is decomposed by the acids without producing ammonia and carbonic acid; while the cyanuret of potash dissolves in water, without being altered, and does not give ammonia, carbonic acid, and hydro-cyanic vapour, unless an acid be added. This is the character which distinguishes

* This is obviously the sulphureted chyazic acid of Mr. Porrett, of which it possesses all the properties. According to the preceding statement, it would be a compound of nearly

| | |
|---|---|
| Hydrogen | 1 atom |
| Sulphur | 1 |
| Carbon | 2 |
| Azote | 1 |

But probably there is some inaccuracy.—T.

a metalline cyanuret from that of an oxide. M. Berthollet remarks, however, in his Statique Chimique, vol. ii. p. 268, that when, after calcining potash with an animal substance, it is thrown into water, ammoniacal odours are instantly exhaled. Several other chemists are of the same opinion; and as it is contrary to the results which I have obtained, it is of considerable importance to know the cause of this difference.

I calcined potash with animal matter in a well luted crucible, and allowed the product to cool. I then dissolved a portion of it in water, and could not perceive the smallest trace of ammonia on putting an excess of lime into the solution, which likewise furnished abundance of Prussian blue. I heated the other portion to a degree far below that of redness; and on throwing a little of it into water, instantly copious vapours of carbonate of ammonia were exhaled. Hence it is obvious that the ammonia is produced in Berthollet's experiment by being put into water at a high temperature; but at the common temperature not the smallest quantity is produced. The presence of an acid is absolutely necessary, as I have already said, and as I again satisfied myself of, with potash calcined with animal substances.*

Thus there is formed in reality, when alkali and animal matters are calcined together, a cyanuret, as Berthollet and Curaudau had affirmed. But their opinion was founded on two inaccurate facts; namely, the destruction of prussiate of potash at a high temperature, and the formation of ammonia when the cyanuret is dissolved in water.

### III. *Of the Substance formed when Hydro-Cyanic Acid is treated with Chlorine.*

M. Berthollet has discovered that when hydro-cyanic acid is mixed with chlorine, it acquires new properties. Its odour is much increased. It no longer forms Prussian blue with solutions of iron, but a green precipitate, which becomes blue by the addition of sulphurous acid. Hydro-cyanic acid thus altered had received the name of *oxyprussic acid*, because it was supposed to have acquired oxygen. The preceding experiment having induced me to examine its nature, I discovered that it is a compound of equal volumes of chlorine and cyanogen.

To prepare this compound, which I propose to distinguish by the name of *chloro-cyanic acid*, I passed a current of chlorine into a solution of hydro-cyanic acid, till it destroyed the colour of indigo dissolved in sulphuric acid, and I deprived it of its excess of chlorine by agitating it with mercury. By distilling it afterwards at a moderate heat, an elastic fluid is disengaged, which possesses the properties ascribed to oxyprussic acid. However, it is not pure

* Is it not possible that when the cyanuret of potash is decomposed by an acid, the heat disengaged by the combination is the principal cause of the mutual decomposition of the cyanogen and water?

chloro-cyanic acid; for this last is not a permanent gas, and cannot exist alone under the pressure of the atmosphere at the temperature of 60° or 65°. It is a mixture of carbonic acid and chloro-cyanic acid in variable proportions, which it is very difficult to determine exactly. I determined the point in the following manner:—

When hydro-cyanic acid is supersaturated with chlorine, and the excess of this last is removed by mercury, the liquid contains chloro-cyanic acid and muriatic acid. On saturating it with barytes, no precipitate falls; and on adding an excess of quick-lime, no smell of ammonia is perceptible. But if this last trial be made after the distillation, abundance of alkali is obtained, and the elastic fluid obtained produces a precipitate in the solution of barytes.

Convinced by these experiments that heat decomposes chloro-cyanic acid, I endeavoured to separate it from its solution by another method. Having put mercury into a glass jar till it was $\frac{3}{4}$ths full, I filled it completely with a concentrated solution of chloro-cyanic acid and muriatic acid, and reversed it in a vessel of mercury. On exhausting the receiver of an air-pump containing this vessel, the mercury sunk in the jar, in consequence of the elastic fluid disengaged. By degrees the liquid itself was entirely expelled, and swam on the mercury on the outside. On admitting the air, the liquid could not enter the tube, but only the mercury, and the whole elastic fluid condensed, except a small bubble. Hence I concluded that chloro-cyanic acid is not a permanent gas, and that, in order to remain gaseous under the pressure of the air, it must be mixed with another gaseous substance. In my experiments it was mixed with carbonic acid gas. It would have been more advantageous if, instead of this gas, an insoluble gas had been present; but after finishing my analysis of the mixture of chloro-cyanic acid and carbonic acid, I did not think that I should add to its accuracy by repeating it with another mixture. The properties of chloro-cyanic acid are as follows:—

It is colourless. Its smell is very strong. A very small quantity of it irritates the pituitory membrane, and occasions tears. It reddens litmus, is not inflammable, and does not detonate when mixed with twice its bulk of oxygen or hydrogen. Its density, determined by calculation, is 2·111. Its aqueous solution does not precipitate nitrate of silver, nor barytes water. The alkalies absorb it rapidly, but an excess of them is necessary to destroy its odour. If we then add an acid, a strong effervescence of carbonic acid is produced, and the odour of chloro-cyanic acid is no longer perceived. If we add an excess of lime to the acid solution, ammonia is disengaged in abundance. This decomposition of chloro-cyanic acid into carbonic acid, and ammonia was observed by Berthollet; but, according to him, it takes place the instant it is mixed with an alkali, while I cannot perceive the presence of carbonic acid and ammonia till an acid has been added. We may easily satisfy ourselves that the aqueous solution of chloro-cyanic acid is not precipitated by barytes before distillation, and that lime does not dis-

engage ammonia. We may likewise ascertain that its solution in potash does not exhale ammonia. But as the formation of that alkali and of the carbonic acid are necessarily simultaneous, it must follow that no carbonic acid is formed when the alkali is added to the chloro-cyanic acid. However, if the elements of this acid do not separate at the instant that the alkali is added, they experience such a modification that we can no longer obtain a green precipitate with solution of black oxide of iron. I have satisfied myself by repeated experiments that, in order to obtain the green precipitate, we must begin by mixing chloro-cyanic acid with the solution of iron. We then add a little potash, and at last a little acid. If we add the alkali before the iron, we obtain no green precipitate.

I have already said that we obtain no precipitate when we pour nitrate of silver into the aqueous solution of chloro-cyanic acid. If we try the same experiment, after having added potash to this last, and then nitric acid, chloride of silver immediately precipitates in considerable quantity. Hence chloro-cyanic acid evidently contains chlorine; and since from the experiments of Berthollet it contains likewise azote and carbon, we ought to admit these three bodies among its elements. But in what proportion are they combined, and does it contain no others?

Its analysis by oxygen did not give me such satisfactory results as I could have wished. Its combustion with oxygen takes place only when a little hydrogen is added. It is very lively; the flame is bluish-white, and accompanied by a white vapour, exceedingly dense, the odour of which has something nitrous, while its taste is mercurial. The mercury adheres to the sides of the eudiometer, as when it is agitated with chlorine. From several experiments, I found,

1. That any quantity of chloro-cyanic acid mixed with carbonic acid produces by its combustion an equal bulk of this last gas. Hence we must conclude that pure chloro-cyanic acid produces its own bulk of carbonic acid:

2. That the oxygen employed is found within two or three per cents. in the carbonic acid and water formed; which shows that chloro-cyanic acid contains no hydrogen:

3. That the volume of azote obtained is nearly equal to half the volume of the chloro-cyanic acid employed. The difference is sufficiently small to enable us to conclude that this body contains

1 volume of vapour of carbon,
$\frac{1}{2}$ volume of azote.

As we know that chloro-cyanic acid contains, likewise, chlorine, let us endeavour to determine its proportion. It is not easy to determine this question directly. But I have succeeded by a particular consideration.

When chloro-cyanic acid is treated by potash, and then by muriatic acid, we obtain a volume of carbonic acid equal to that of the chloro-cyanic acid employed. This result is obviously independent

of the quantity of carbonic acid previously mixed with the chloro-cyanic acid; and as the analysis by the eudiometer has shown us that this last produces, when burnt, an equal volume of carbonic acid gas, it follows that no hydro-cyanic acid is produced when chloro-cyanic acid is decomposed by the successive action of an alkali and an acid. This is confirmed by experience, and Berthollet ascertained it long ago. Finally, the chlorine. when it separates from the azote and the carbon, produces, when it unites with the alkali, either a muriate or a chloride; it is indifferent which; but I shall suppose here a muriate. If, then, we obtain only carbonic acid, ammonia, and muriatic acid, I say that chloro-cyanic acid contains half its volume of chlorine. Since on decomposing the water by the successive action of an alkali and an acid, a volume of carbonic acid is produced, two volumes of hydrogen must be disengaged. We have seen that it contains $\frac{1}{2}$ a volume of azote, which, when changed into ammonia, takes $\frac{3}{2}$ volumes of hydrogen. Half a volume of hydrogen remains, which, in order to be changed into muriatic acid, requires half a volume of chlorine. Chloro-cyanic acid, then, is composed of

1 volume of the vapour of carbon,
$\frac{1}{2}$ volume of azote,
$\frac{1}{2}$ volume of chlorine;

and when decomposed by the successive action of an alkali and an acid, it produces

1 volume of muriatic acid gas,
1 volume of carbonic acid,
1 volume of ammonia.

It remains to determine the condensations which these three elements undergo when they combine; and I shall now show, by another mode of analysis, that it is the half of the sum of their volumes.

When chloro-cyanic acid is heated with antimony in a small glass vessel, by means of a spirit lamp, the bulk of the gas gradually diminishes. At the same time vapours of chloride of antimony are produced, which crystallize as they condense. When the action is terminated, the gas is still, as it was before, entirely absorbable by alkalies. Its odour and properties are those of cyanogen. The chloro-cyanic acid which I employed diminished 0·344 by the action of the antimony. It contained carbonic acid, as I have already said, so that its diminution would have been more considerable if it had been pure, and would doubtless have amounted to half its volume. If we admit this supposition, it is easy to determine the quantity of chloro-cyanic acid mixed with the carbonic acid. It would be obviously double the diminution observed; that is to say, 0·688; and that of the carbonic acid, 0·312. But if we calculate from this result, the quantity of chloro-cyanic acid from its analysis by means of oxygen, and that of the cyanogen in the analysis of the residue

left by treating chloro-cyanic acid with antimony, we find a quantity of azote equal to that which experiment gives. Therefore the supposition that this acid is reduced to half its bulk when deprived of its chlorine is well founded. Let us admit, then, that it is composed of

1 volume of vapour of carbon,
$\frac{1}{2}$ volume of azote,
$\frac{1}{2}$ volume of chlorine;

and that the condensation which these three elements undergo is half their total volume; or otherwise, that a volume of chlorine and a volume of cyanogen, when they unite, produce two volumes of chloro-cyanic acid. This is the same result as for hydro-cyanic acid, which is produced likewise by the combination of equal volumes of hydrogen and cyanogen without any condensation; so that the chlorine in the chloro-cyanic acid comes in place of the hydrogen in the hydro-cyanic. It is remarkable that two bodies with such different properties play the same part in their union with cyanogen.

Since one volume of chlorine and one volume of cyanogen produce two volumes of chloro-cyanic acid, the density of this last ought to be the mean of the sum of the densities of its two constituents:—

| | |
|---|---|
| Density of chlorine ..... | 2·421 |
| Density of cyanogen ..... | 1·801 |

Half the sum of which is 2·111, as I have already stated.

Chloro-cyanic acid exhibits with potassium almost the same phenomena as cyanogen. The inflammation is equally slow, and the gas diminishes as much in volume. If we employ a quantity of potassium which would disengage 50 parts of hydrogen from water, the diminution of volume is about 50 parts, and the gaseous residue, washed with potash, gives from 10 to 12 parts of carbonic oxide. The solid matter has a dirty yellow colour; when dissolved in water, it forms an alkaline solution, which precipitates nitrate of silver after having been neutralized by nitric acid, and which gives Prussian blue with solutions of iron. The presence of carbonic acid in the chloro-cyanic acid rendering these results somewhat uncertain, I did not proceed further with this experiment.

I have not made other experiments with chloro-cyanic acid; I have, however, attempted to procure it by other methods; but those above described still appear to me the best.

When hydro-cyanic acid containing but little water is put into chlorine, there is instantly produced a great quantity of muriatic acid gas, and of chloro-cyanic acid, which is partly deposited on the sides of the vessel in the form of small oily drops. If we make a mixture of hydro-cyanic vapour and air, and add chlorine to it, no change takes place in the dark, provided the gases do not contain

moisture; but when exposed to sun-shine, there is quickly produced, without detonation, a thick vapour, which condenses itself in part upon the sides of the vessel, as in the preceding experiment. When the gaseous mixture is agitated with mercury, the oily drops disappear, and the gaseous residue, containing evidently chloro-cyanic acid, preserves an evident odour of chlorine, though it does not destroy the colour of litmus.

I likewise kept in the shade for some days chlorine, with well dried cyanuret of mercury; but no action between them became visible. In the light of the sun, on the contrary, the colour of the chlorine disappeared completely in 12 hours, and the cyanuret of mercury adhered to the sides of the vessel, as if it had been moist. On opening the vessel over mercury, that metal filled about half its capacity, and the remaining gas, which was a mixture of air and chloro-cyanic acid, had, however, a decided smell of chlorine, though it did not discolour litmus. Having filled the vessel entirely with mercury, I introduced a determinate volume of air; and on measuring it anew, I found that it had increased nine per cent. But though these different experiments prove that chloro-cyanic acid is not a permanently elastic fluid, they do not give us any convenient mode of obtaining it. I likewise tried to obtain the acid by mixing together equal volumes of chlorine and cyanogen. As, after several days exposure to a weak light, no change took place in it, I exposed it to sun-shine. A white vapour appeared, and I perceived a great number of oily drops. Yet after agitating the gases with mercury, nothing remained but cyanogen. Is it not probable that in this case a peculiar substance is produced, which is destroyed by agitating it with mercury?

*(To be continued.)*

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## ARTICLE VIII.

### *Astronomical Observations.* By Col. Beaufoy.

Lat. 51° 37′ 42″ N. Long. W. from Greenwich in time 1′ 20½″.

*June* 9.—Moon eclipsed at Bushey.

| | Calculated Time. | | | Observed Time. | | |
|---|---|---|---|---|---|---|
| Beginning of the eclipse .... | 11h | 29′ | 28″ | 11h | 30′ | 30″ |
| Beginning of total darkness .. | 12 | 40 | 19 | 12 | 49 | 22 |
| Middle ..... | 13 | 14 | 02 | — | — | — |
| Ecliptic ☍ ..... | 13 | 17 | 44 | — | — | — |
| End of total darkness ..... | 13 | 47 | 45 | 13 | 43 | 50 |
| End of the eclipse ..... | 14 | 58 | 36 | 14 | 58 | 44 |

The calculations and observations are made according to apparent

time, and the calculated duration of the total darkness exceeds that deduced from observation by 12′ 58″.*

*May* 17.—Emersion of Jupiter's second satellite, $12^h$ 38′ 53″ mean time; or $12^h$ 40′ 14″ mean time at Greenwich.

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## Article IX.

*Account of Bruce's Portable Boat.* Communicated to Dr. Thomson by Robert Stevenson, Civil Engineer.

(With a Plate.)

*April*, 1816.

When I had the pleasure of seeing you in London last summer I mentioned some experiments which I was then engaged in making upon the waters of the Thames, suggested by former experiments which I had made upon the River Dee, at Aberdeen, in the month of May, 1812; and I intended before now to have communicated some information to you upon that subject. A desire, however, to extend those observations to several of our principal rivers has hitherto prevented me from writing to you; but in the mean time I venture to send you an account of a most useful improvement upon boats and boat sailing, which has within these few years been introduced upon the coast by James Bruce, Esq. of the Naval Yard, Leith; and is found to be of great utility in the Frith of Forth, particularly on the Ferry of Kinghorn.

The boat to which I allude may be built of any convenient size; but being chiefly carried as the launch of a larger boat, and used for the purpose of landing upon shores where there is little depth of water, it is usually built on a light construction, calculated to carry five or six persons upon a very easy draught of water. Its dimensions are, therefore, generally about 15 feet in length, and its extreme breadth measures five feet. The peculiarity of this boat consists in its being so constructed that it may either be used as *one boat*, or separately as *two boats*. It is thereby rendered much more generally useful and portable by the one half being stowed within the other, when they are taken on board of a larger boat. This *portable boat* is constructed in all respects like a common boat or ship's yaul; but when almost finished, two division boards are introduced, and fixed transversely nearly *amidships*, and within a *saw draft* of each other, in such a manner as to answer the purpose of the *stern boards* of two boats when used separately. These *stern boards* being fixed in their places, the boat is cut transversely, when two entire and complete boats are produced, which may be connected at pleasure

* In this eclipse the body of the moon completely disappeared, a circumstance which has been thought improbable, but the same phenomenon is recorded three times between the years 1600 and 1650.

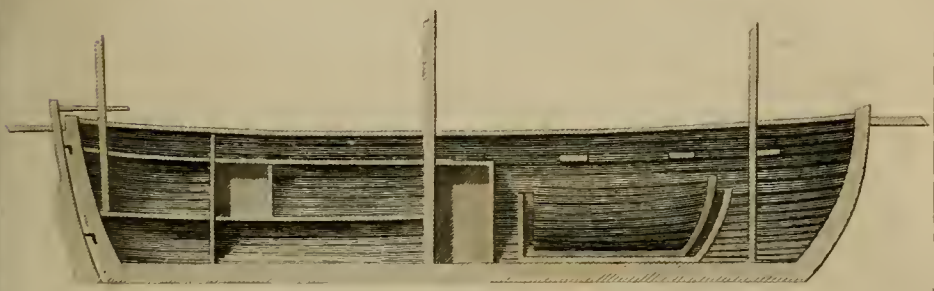
by a *slip bolt* placed near the keel, like the lower *rudder chatter* of a common boat. Besides this bolt, and near the top of the *stern sheets* there are two screw bolts with *finger nuts*. But this, and other particulars, will be more readily understood by inspecting Plate LII. in which are exhibited the sections of the boat, together with the view of the two halves when afloat.

The advantages of employing *Bruce's portable boat* as the *launch or long boat* of larger boats, and *small craft* in general, are very obvious; but this invention is more particularly adapted to become extensively useful for large fishing or pleasure boats without a deck, which in most cases require as great, or even a greater, depth of water to float them than common trading sloops of 20 or 30 tons burthen. Now to all who have seen or experienced the inconveniency, and even hazard, which often attends the approaching of a lee shore with a large boat of this description, without the aid of a smaller boat, and who have of course felt the mortification of either waiting the return of the tide, or perhaps been disappointed in their object altogether, will at once be enabled to appreciate the value of this simple contrivance, by which the perplexities that daily occur in navigating large open boats may be effectually avoided. I have, indeed, often wondered that so complete a remedy for much inconvenience has not reached the noted towns of Deal and Dover, where large boats, managed with much spirit and activity, are still greatly lumbered, and their passengers much hampered, with the unavoidable size of a small boat made in the common way, which they carry on board. They often, indeed, *tow* it astern, but this greatly retards their speed in sailing. Before the common small boat can be launched into the water from a larger boat, for the purpose of *making a landing*, much trouble and confusion usually ensue, even in the best managed boats; whereas the boat I am now describing is commodiously stowed, *the one half within the other*, before the mainmast of a boat measuring 26 feet in length over the stem and stern, which gives great additional accommodation to passengers. So complete is this contrivance, that at all times one man may hand these two small boats over the large boat's *gunwhale*; or if convenient, two men may assist each other. When afloat, the *two halves* may either be used separately, or connected by the stern-bolts as one boat, according to the purpose in view; and it is satisfactory to know that the *two halves* are nearly as speedily put together, on the water, while afloat, as a boat's rudder may be *shipped:* and indeed the connecting of them by the catch-bolt or chatter is an operation not unlike the hanging of a rudder, while the screw bolts are secured with the greatest ease and expedition. If we reflect for a moment on the advantage of this double boat in point of buoyancy, compared with the small *single boat*, which can only sometimes be carried on board of large boats, on account of its inconvenient bulk in stowage, we at once see the value and important use of this boat as applicable to many purposes on all the shores of the kingdom.

The Frith of Forth has already been mentioned; but it is no less

# Bruce's Portable Boat.

Stern View of the two half boats.



Section of a large boat with the two half boats on board.

J. Steedman del. J. Shury Sc.

The two half boats afloat.

Engraved for Dr Thomsons Annals. for Baldwin Cradock & Joy. Paternoster Row July 1st 1818

NATURAL HISTORY

applicable to the ferry between Caithness and Orkney, where the post boat crosses the Pentland Frith. Such a boat would be highly conducive to the safety of the mail, and to the comfort of the numerous passengers who land on the shelving and unprotected shores on either side of this impetuous Frith, where as yet no harbour has been made for the reception of the *post boat*. There are many other situations, both upon the British and Irish coasts, where the introduction of Mr. Bruce's boat might be attended with great advantage to the public. In recommending this boat to notice, I am extremely happy in being able to state that it is highly approved of, and has already been tried pretty extensively, Mr. Menzies, carpenter, of Leith, having built no less than 12 of these boats for various parts of the coast. To show the facility with which they may be used, I wish particularly to mention that for the last three years it has been employed with great success in the *pinnaces*, or open boats, belonging to the ferry between Leith and Kinghorn, which is under the management of trustees appointed by Act of Parliament; and it is also in two of the *boat tenders* in the service of the Commissioners of the Northern Light-houses.

Besides being applicable to ferry boats, and as a launch for all boats not of less dimensions than 22 feet of keel, this boat, from its portable form, will be found to be very generally useful. It deserves a place in every ship of war, and is well adapted to become an appendage to all ships of discovery sent by Government to explore unfrequented coasts, and has already met with the approbation of many Captains and Commanders of the Navy, as highly useful in watering parties, and even suitable for landing expeditions upon an enemy's coast. The Hon. Col. Sir R. Dundas has had a model of this boat laid before the Board of Ordnance, with a view to its application in many situations for an army; as it may be made so light as to form the bodies of two *baggage carts*, and, when joined, to become useful as a boat for crossing rivers or arms of the sea. In Spain, in France, and in Flanders, such boats would have been found much more useful than the common *pontoon*, carried in the engineer's train during the late Wellington wars.

There is still another use which may be mentioned to which this boat seems to be peculiarly well adapted. Your readers already know that Mr. Scoresby, of Whitby, has supposed it practicable to make a journey to the North Pole, which indeed has already been attempted by Alexia Markoff, a Cossack, who set off from the coast of Siberia in the spring of 1715, in a sledge drawn by dogs, but who, owing to the want of provisions, was obliged to abandon the enterprise after he had proceeded about 400 miles. For such a purpose nothing could be better adapted than Mr. Bruce's boat, made of thin sheet copper, or built of light *willow timber*, which would answer for the bodies of two sledges, and these with an *awning* or covering, might readily be converted into two small huts, carrying fuel and every necessary; and when any of the intervening

pools occur, which, Mr. Scoresby alludes to in his paper inthe second volume of the Transactions of the Wernerian Society, by joining the *two halves* a boat of great buoyancy would readily be obtained for crossing them.

In stating the obligations the public owe to Mr. Bruce for this boat, I might also mention other improvements in boat sailing which that Gentleman has introduced upon the coast of the Frith of Forth, particularly the use of the *lateen sail*, which is found to be more safe, and in every respect better, than the common *lug and sprit* sails formerly in use in the Kinghorn passage boats.

*Edinburgh, April* 21, 1816.

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## ARTICLE X.

### *On the Physical Properties of Gases.* By Mr. I. Herapath.

(To Dr. Thomson.)

SIR, *Bristol, Guinea-street, April* 11, 1816.

IN my letter to you respecting a paper which I wished to have published in your *Annals of Philosophy*, I did not, I apprehend, state that it was my intention to give the solution to the problem it was likely to contain. The purport of that letter was, I believe, that having reason to think, from many concurring circumstances, I could give a mathematical explication of the cause of heat, light, gravity, cohesion, &c., I had composed a problem, which I had thoughts of proposing to mathematicians and philosophers, for the sake of discovering whether my ideas were known to others; by which means I hoped to avoid controversy, in case I should be induced to make my sentiments public. And as I was desirous this problem should have such an introduction to the world as would ensure it attention, I wrote to you, requesting to be informed whether it was consistent with your plan to allow a paper of this mixed problematic kind a place in your *Annals*. However, as you seem to have totally misunderstood me, and to be anxious for a solution, with which I cannot at present, on many accounts, gratify you, I have converted the problem into the subsequent theorem, which I trust some of your readers, to whom my ideas may have occurred, will have the goodness to demonstrate. And here I must beg leave to observe, that I have chosen this theorem in preference to some of much greater difficulty, because of the rapid and easy manner in which it flows from our first principles; it being more my intention by easy and obvious things to discover what others have done, than to display the effects of my own industry and patience by unnecessary difficulties.

*Theorem.*

Let the volumes of any two gases be denoted by V, $v$; the weights of these volumes by W, $w$; the numbers of particles in them by N, $n$; the temperatures by T, $t$; and the elasticities by E, $e$. The ratio of E to $e$ is equal to the ratio of $T^2$, $N^2$, $v$, $w$, to $t^2$, $n^2$, V, W.

In this theorem I suppose the particles to be throughout each volume uniformly of the same size; but I do not consider whether these particles are in themselves single atoms, or two, three, or more, united. By the number of particles, therefore, I mean the number of little individual bodies of which the airs may be composed.

*Cor.* 1.—Hence in a given portion of air of any kind, whose temperature is uniform, the elasticity is reciprocally proportional to the space it occupies.

*Cor.* 2.—And if the portion of air and the elasticity remain the same, the temperature will be in the subduplicate ratio of the volume.

*Cor.* 3.—Hence all permanently elastic airs whose temperatures are equal, however variable these temperatures may be, have their volumes in one invariable ratio. Consequently invariable portions of any two airs, under equal elasticities and temperatures, will proportionally expand in volume by equal additions of temperature.

*Cor.* 4.—Since it is found by experiment that all gases, at the temperature of water freezing, expand themselves under the same elasticity, at the temperature of water boiling: in the ratio of 1000 to 1375, it follows, by *Cor.* 2, that the ratio of these temperatures is equal to that of $\sqrt{1000}$ to $\sqrt{1375}$, or of 579 to 679 nearly.—It is remarkable that this result agrees very nearly with what I have read somewhere in Dr. Henry's Chemistry, obtained by widely different means.

*Cor.* 5.—If the temperatures and elasticities of two gases are the same, the numbers of the particles are in the subduplicate ratio of the weights and volumes. Hence if oxygen be 16 times heavier than hydrogen, as some late experiments seem to make it, two particles of oxygen unite with one of hydrogen to form water.

*Cor.* 6.—In any given portion of gas it appears that the temperature is always as a mean proportional between the volume and elasticity.

*Scholium.*

I do not trouble you with all the consequences that may be drawn from this theorem, because, having the theorem before you, they cannot fail of being easily perceived; nor do I mention other properties of gases, that follow from a combination of this theorem with other consequences, derived from the same principles, as this would be little less than doing myself what I want to see done by

others. But there is one circumstance which I must not omit to observe, lest it be productive of error. This is, that the temperature included in our theorem is not such as is shown by the common thermometers. These do not show the ratio of the temperatures of any two bodies, nor do they point out the real difference of temperature, as is well known. They only show us that there is a difference, and that this difference is greater or less than another difference. Indeed, from any thing that has been published, we are wholly unable to discover what is the difference, or what is the ratio of temperature of two bodies, or which is the point of absolute cold. These are things that cannot be known without a mathematical explication of the cause of heat, and the assistance of instruments constructed on mathematical inductions from it. In the sixth *Cor.* we have endeavoured to give the data from which instruments for this purpose may be made. The first and third *Cor.* present us with results perfectly coinciding with two of the most general and important laws of gases which observation has made known to us. The fifth *Cor.* is remarkable for being diametrically opposite to the opinion of some of the most celebrated chemical philosophers of the day. In consequence of the definite union of gases by volume, it has been supposed that the atoms or particles of these gases unite in the same proportion. Thus two in volume of hydrogen combining with one in volume of oxygen to form water, it has been therefore urged that two particles of the former unite with one of the latter. But this is taking two positions for granted which remain to be proved; namely, that equal volumes of any two gases, *cæteris paribus*, contain equal numbers of particles; and that if the elasticity of gases depends on the repulsive force of their particles, all particles at the same distance repel each other with the same force, however different may be their sizes. Whether the experiments that have been made on the definite union of gases authorize us to consider it an invariable law, is questionable. This at least is certain, that whatever may be the proportion in which gases combine, they are capable of remaining in union afterwards in very different proportions. Mr. Harrop's beautiful experiments on the absorption of oxygen from water by nitrogen, sufficiently establish this fact; which perhaps is one of the most valuable discoveries that has lately been made, and which, if pursued, appears likely to disclose truths of considerable importance.

Though the above theorem evidently obviates the necessity of my entering at present into the particulars of our theory, yet perhaps a few remarks on its simplicity and comprehensiveness may not be unacceptable. In the first place it does not show a necessity of there being more than one kind of matter from the different sizes, figures, and modifications, of whose particles all bodies may be composed. It gives but one cause for heat, light, gravitation, electricity, cohesion, aerial repulsion, &c.; from which all these flow, and are easily deducible; and their effects may be computed by mathematical in-

duction.* It shows us that gravitation, cohesion, and affinity, are but the same thing under different modifications; that the difference of the two latter arise from a difference in the figures and sizes only of the particles; that attraction and repulsion are not properties of matter; and that the phenomena of heat and electricity are not the effects of fluids, "*per se et sui generis.*" And if we descend from causes to effects, the rapidity with which the phenomena flow from the theory is rather singular. I can say that as far as I have yet compared it with observation, during a period of some years, there has not occurred a single exception; and wherever mathematics have been applied, the results of theory and experiments perfectly agree, of which the preceding theorem, obtained by the strictest mathematical induction from first principles, may probably be allowed to be no unfavourable specimen. But there are several cases to which I have not yet introduced mathematics, and that partly on account of the difficulty of the investigations, and partly on account of the small portion of time which the duties of my situation allow me to devote to those things. In our solution of the problem concerning the law of gravitation (a problem that is not perhaps one of the easiest), we arrived at a result that ought not to be lightly passed over, particularly as it may lead to consequences of more importance than we may expect; and, at any rate, when ascertained by experiment, will prove pretty decisive with respect to the refutation or confirmation of our theory. It is this, that the intensity of attraction, as deduced from our principles, is subject to the influence of the temperature of the attracting body. I have long wished to put this to the test of experiment, but have not yet been able to devise any effectual means of doing it. The interference of our atmosphere, and the exceedingly small force with which one body attracts another, when compared to the attraction of the earth, added to the very trifling difference we are enabled to make (*Cor.* 4) in the ratio of the temperature of a body, render it extremely difficult, if not impossible, to appreciate by any common experiment the effect that a difference of temperature would have on the attractive force of a body. But though local circumstances make it no easy matter to experiment successfully on this subject, there are perhaps other means to which we may not unprofitably have recourse. One of these is the lunar theory. By a calculation which, some years since, I made on the annual equation to the moon's mean motion, there seems to be reason for supposing that the earth's action on the moon is greater about the winter than the summer solstice, or greater when the earth is nearer the sun than when further from it. This difference, if it be so, can only be attributed to a variation of temperature, arising from a change of distance in the earth with respect to the sun.

* The phenomena of electricity and magnetism I have not yet attempted to reduce to mathematical laws. Indeed, the experiments that have been made on these subjects, notwithstanding they are so numerous, do not appear to be the best adapted for an inquiry into their physical causes.

While speaking of experiments, I may just hint that one has occurred to me, during the time I have been writing this, to ascertain the truth of our theory, in one or two cases, by the help of concave mirrors and thermometers constructed on our principles. But as I have not sufficiently examined the idea to be able to form a correct judgment of it, I defer saying any further at present.

I must now have trespassed considerably on your patience, for which I beg to return my kind acknowledgments, and will conclude with observing, that if this crude trifle prove acceptable, it may be followed by one or two others, probably on the lunar calculation above mentioned, or the phenomena of reflected and refracted heat and cold, in which I hope to set the matter discussed in some of the late numbers of your *Annals of Philosophy*, by the ingenious Messrs. Murray and Davenport, in a light that may not be altogether unsatisfactory.

I am, Sir, respectfully, your humble servant,

J. HERAPATH.

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## ARTICLE XI.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

ON Thursday, May 23, a paper, by Thomas Andrew Knight, Esq. On the Cause of the Formation of Ice in the Bottom of Rivers, was read. After a very cold night last winter, he observed a great many icy spiculæ upon the surface of the river Team, in Herefordshire, near the place of his residence. Had the weather continued cold, he has no doubt that bottom ice would have been formed in this river, as it had been about eight years before. The current would have driven these spiculæ against the stones at the bottom of the river, to which they would have stuck, and soon produced an icy concretion.

At the same meeting, a paper, by Sir Everard Home, On the Formation of Fat in the Tadpoles and Frogs, was read. An anatomical description of the tadpole of the rana paradoxica was given, and likewise a set of chemical observations on the eggs of birds and the spawn of frogs, by Mr. Hatchett. The yolk of birds' eggs is a mixture of albumen and a buteraceous oil. But the spawn of the frog contains no oil whatever. Hence Sir Everard concludes the necessity of tadpoles to form a quantity of fat before they can complete their change into a frog. And as the intestines of the tadpoles are longer than those of any other animal, Sir E. considers it as evident that this fat is formed by the intestines.

On Thursday, May 30, a paper, On the Application of Galvanism to the Cure of certain Nervous Diseases, by Dr. Wilson.

Philip, was read. The author, in the papers already submitted to the Society, conceives that he has shown a radical difference between the nervous and the sensorial energies. Galvanism, if it be not the very same with the former, may be substituted for it, and acts in the same way. He thinks, therefore, that galvanism can be applied successfully only to disorders of the nervous system; though it may at times, perhaps in consequence of the intimate connexion between the sensorial and nervous systems, successfully excite the former to activity. Asthma appears to be entirely a disease of the nerves; for the lungs, even in obstinate chronic cases, are not in the least injured. He therefore thought of applying galvanism as a cure of that disease, and found it of material benefit in all the cases tried. It was had recourse to in 18 cases in the Worcester Infirmary, and in four cases of private practice. In every one of them immediate benefit was experienced; in most of them it afforded greater relief than any preceding medicine tried, and in two of the cases it produced a complete cure. The method was to apply a piece of tin foil to the nape of the neck, and another to the pit of the stomach. The wires from the two ends of a galvanic battery were connected with these, and the galvanism was continued for about 10 minutes. At first it was very weak, being confined to three or four pairs of plates of four inches square, excited by water mixed with $\frac{1}{20}$th of its weight of muriatic acid; but was gradually increased till it consisted of 20 or 25 pairs of plates, by removing one of the wires along the battery. The galvanism was applied once in 24 hours; and in two cases, in which it produced permanently beneficial effects, it was applied twice in 24 hours.

On Thursday, June 13, a paper, by Thomas Andrew Knight, Esq. On the detached Leaves of Vegetables, was read. Mr. Knight was of opinion that liquids similar to the true sap pass down through the footstalks of the leaves of plants, and supply all the nourishment by which vegetables are supported; and his experiments related in this paper confirm his opinion. He planted single leaves of the potatoe in garden pots, and watered them regularly. In this situation they lived till winter, and the bottom of the leaf had swelled out to a matter similar in its nature to the potatoe tuber. Leaves of mint treated in this way likewise lived all winter, like evergreens, and sent out numerous roots. Branches of the vine about a yard long were placed so that their full grown leaves dipped partly into a bason of water each. In this position the branches lived for a month; the small leaves increased in size, and the small twigs continued to elongate.

At the same meeting a paper by Dr. Holland was read, On the Manufacture of Sulphate of Magnesia at Monte de la Guardia, about eight miles north from Genoa. The mountains in that district consist of primitive slate, with subordinate beds of serpentine and lime-stone. The serpentine contains veins of pyrites, both copper and iron, and the manufacture was first established to obtain

blue and green vitriol. But the appearance of sulphate of magnesia in great quantity, during the process, gradually induced the proprietors to turn their chief attention to the preparation of that salt. The pyrites is roasted for about a week. It is then exposed to the air for some months, during which period it is occasionally watered. It falls down into small fragments, and minute crystals of sulphate of magnesia form on its surface. It is lixiviated with water, and the liquid filtered. It contains sulphates of magnesia, of iron, and of copper. The metals are precipitated by means of the magnesian lime-stone which abounds in that country. It is supposed that the magnesia contained in the lime-stone may somewhat increase the product of sulphate of magnesia. But I do not see how that can happen. An analysis of the pyrites employed in the manufactory would be very desirable. Probably the magnesia may be derived from serpentine or steatite merely mechanically mixed with the pyrites.

On Thursday, June 20, a paper by Dr. Brewster was read, On the Optical Structure of the Lens of the Eyes of Fishes. Much pains have been bestowed in investigating the structure of the eyes of animals. But the investigation has not rewarded philosophers for the labour bestowed upon it. Dr. Brewster subjected the lens of a cod fish to examination in a glass parallelopiped surrounded with Canada balsam, and described the optical phenomena which presented themselves. From these phenomena he draws as a conclusion that the central nucleus of the lens is in a state of compression, and the external coats are in a state of dilatation, in consequence of which they both polarize light like crystallized bodies; while the coats between the central nucleus and the external coats do not polarize light at all. He conceives that the object of this structure is to correct the effect of spherical aberration.

At the same meeting a paper by Sir Everard Home was read, containing a further Account of the Fossil Skeleton found near Lyme, and described by him in a former paper read to the Society. Having examined some further remains of the same fossil animal in the collection of Mr. Buckland, of Oxford, and Mr. Johnson, of Bristol, he has ascertained that the animal was a fish, but seemingly of a genus very different from any which exists at present.

## LINNÆAN SOCIETY.

On Friday, May 24, the Anniversary of the Society, the following office-bearers were elected for the ensuing year:—

*President*—Sir James Edward Smith, M.D.
*Treasurer*—Edward Forster, Esq.
*Secretary*—Alex. Macleay, Esq.
*Under Secretary*—Mr. Richard Taylor.

There were retained of the old Council—

The President,
The Lord Bishop of Carlisle,
Aylmer Bourke Lambert, Esq.
Alexander Macleay, Esq.
William George Maton, M.D.
Daniel Moore, Esq. F.R.S.
Joseph Sabine, Esq. F.R.S.
William Kent, Esq.
Rev. Thomas Rackett,
Thomas Thomson, M.D. F.R.S.

The five following Fellows were elected into the Council—

Edward Forster, Esq.
George Bellas Greenough, Esq.
Wm. Horton Lloyd, Esq.
John Lord Bishop of Salisbury,
Edward Lord Stanley.

On Tuesday, June 4, and on the two preceding ordinary meetings, the Society was occupied by a paper on the genus *rosa*, by Mr. Wood.

On Tuesday, June 18, part of a paper on the genus of plants called *juncus*, containing a description of all the indigenous species, by James Ebenezer Bicheno, Esq. was read.

The Society adjourned till Nov. 5.

## ROYAL SOCIETY OF EDINBURGH.

On Monday, June 3, a paper by Mr. Cadell was read, On the Lines that divide each Semidiurnal Arc into Six equal Parts.

The intertropical parts of these lines for the climates of Greece and Italy constitute the hour lines on the antique sun-dials. Most of the writers on gnomonics have considered these lines as great circles; Clavius alone demonstrates that they are not great circles; and afterwards Montucla states, but without discussion, that they are curves of a peculiar nature. The celebrated and profound astronomer Delambre, having examined only the portions that occur on the Greek dials, controverts the opinion of Montucla.

The object of the paper is to show that the curved surfaces, whose sections form these lines, are undulated, and of the nature of cones, the apex of one undulation being as much elevated above the equator as the apex of the next undulation is depressed below it.

To see the curvature of these lines, it is sufficient to draw them on a globe. And the undulated cone is completed by conceiving the diameter of the sphere which has described the first branch, to move progressively and continuously between the two parallels that touch the horizon, until the extremities of the diameter arrive at the points from which they set out.

If it be proposed, for example, to draw on a globe the curve which contains the third and ninth antique hour line, that the

figure may be more conveniently delineated, elevate the pole about 60°, and divide each semidiurnal arc into two equal parts, and a line drawn through the points of division is one bicrural branch of the curve. This branch terminates at a point in the greatest, always seen parallel; and to complete the curve, the semidiurnal arcs belonging to this point, considered as the middle point of a horizon, (forming the same angle with the equator as the first horizon, but on the other side) are to be divided into two equal parts, and the points of division being joined, a complete re-entering curve is formed on the surface of the sphere. A diameter of the sphere revolving, with its extremity applied to this curve, forms the undulated conical surface, the portion of the diameter on the other side of the centre forms at the same time an opposite cone equal and similar.

The five undulated surfaces, each of which contains a pair of the antique hour lines have each a different number of undulations.

## ROYAL INSTITUTE OF FRANCE.

### *Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year* 1815.

PHYSICAL DEPARTMENT.—*By M. le Chevalier Cuvier, Perpetual Secretary.*

### ZOOLOGY, ANATOMY, AND PHYSIOLOGY.

(*Continued from* Vol. VII. p. 467.)

The observations on the envelopes of the fœtus, made by M. du Trochet, Physician of Chateau-Renaud, of which we have already spoken several times, have been repeated by the Commissioners of the Class, who, being once engaged in this work, made themselves some observations, tending, like those of M. du Trochet, to confirm the great analogy which has been already observed between viviparous and oviparous animals.

The oviparous animals, which after their birth respire by means of lungs, have all eggs nearly of the same structure. Within a double membrane, which covers the inside of the shell, are contained the white and yolk of the egg. This last is suspended by two poles, by means of cords, called chalazæ, which are productions of its peculiar tunic, the most external, under which is also a second. It is below this that the first traces of the chicken are perceived, and the beautiful vascular circle by which it is connected with the yolk, and from which vessels come the arteries and veins of its mesentery. The umbilical vessels do not proceed to the yolk, but are distributed on a membrane which communicates with the cloaca, and which corresponds with the allantois of quadrupeds. This singular organ, at first invisible, and appearing only on the fourth day, and as a vesicle coming out of the abdomen, grows with astonishing rapidity.

It pierces the epidermis of the yolk, pushes the white to the narrow end of the egg, and soon encloses the whole fœtus and yolk with a double membrane. The external tunic thus produced by the prodigious increase of the allantois is what the old observers have called *chorion*; but it does not correspond to the true chorion of quadrupeds, which is represented by the proper membrane of the shell, as the shell itself represents the *membrane caduque* in quadrupeds. It is extremely probable that this net of the allantois serves for respiration, and supplies the place of the lungs, which cannot exercise its functions till the animal comes into the elastic air. What leads to this opinion is, that oviparous animals, which breathe during their life, or immediately after birth, by gills, have never in the egg either allantois or umbilical vessel, probably because the liquid in which they live furnishes enough of oxygen to their gills, and receives it itself from the ambient element.

In the false viviparous animals with lungs, such as the viper, the shell of the egg, and the peculiar membrane, which are much thinner, are speedily torn and rejected; so that the exterior and vascular membrane of the allantois serves as an exterior tunic. It is immediately embraced by the edges of the oviducts; and as it sometimes adheres to their edges, M. du Trochet supposes that as intimate a connexion exists between them as between the placenta and uterus in mammiferous animals; so that the vipers would be still more completely viviparous than they appear to be. But the observations of the Commissioners have not confirmed this opinion. The case is different with the remarks made by our skilful observer on tadpoles. Their skin and tail does not fall off, as is commonly believed, to allow the frog to appear. But the skin, after being pierced by the feet, forms, on drying, a kind of epidermis, and the tail is entirely re-absorbed.

M. du Trochet had been in some respects anticipated in his observations on eggs by the German anatomists, and particularly by Blumenbach, Hochstetter, and Emmert. But he has added a great deal to what they knew; and he has contrived to make the different degrees of developement very clear, by means of ideal sections, in which the eye is enabled to follow all the changes of proportion of the different parts.

M. Cuvier, one of the Commissioners appointed to verify the observations of M. du Trochet, has continued them in some respects on the fœtuses of true viviparous animals; that is to say, on the mammifera, assisted by M. Diard, a young physician, who likewise assisted M. du Trochet.

To perceive the analogy between the envelopes of these fœtuses and those of the egg, it was necessary to observe them in carnivorous animals, and especially in the cat. The membrane called, very improperly, umbilical, and which receives vessels only proceeding from those of the mesentery, represents in them the yolk of the egg, and so completely that in the cat it is likewise a yellow liquor, which it contains at a certain period of gestation. Fixed by its two

chalazæ to the two extremities of the chorion, as the yolk is to the membrane of the shell, it is likewise enclosed, as well as the fœtus, and its amnios, by the double membrane of the allantois; and between it and the chorion there is a very vascular membrane, wholly supplied by the umbilical vessels, and which most authors have confounded with the chorion, which, on the contrary, has no vessels.

The principal difference, then, between mammiferous and oviparous animals, will be, besides the existence of the placenta in the first, that in them the allantois doubles the chorion and envelopes within it from the first, the fœtus and yolk, so that we cannot see their original, nor follow their developement.

In certain orders of mammiferous animals, and particularly in the *rangeurs*, there is a still more singular difference; the allantois remains smaller, and it is the umbilical membrane which encloses it, as well as the fœtus, and doubles the chorion.

M. Cuvier has found, as Messrs. Oken, Hochstetter, and Emmert, had done, the umbilical membrane in all the mammifera, even in man; but he could never perceive the pedicle by which the first of these observers pretends that it communicates with the intestine, and which would have completely established its analogy with the yolk of the egg of birds. He thinks, likewise, that the allantois always exists, and that it has not been observed in man, because it adheres too intimately to the inner surface of the chorion. This adherence is not less intimate in the horse. But as in this animal the *ouraque* is hollow, it has been easy to perceive the existence of the allantois. It has been misunderstood in man, because the *ouraque* is usually obliterated.

From these observations it follows that the only essential difference between the eggs of different animals with lungs is, that in oviparous animals the umbilical membrane contains a quantity of food sufficient to nourish the fœtus by means of its omphalo-mesenteric vessels, till it be hatched, and even after its birth, and that the umbilical vessels on the inside of the allantois merely answer the purposes of respiration; but in viviparous animals this umbilical membrane alone not being sufficient for nutrition, the umbilical vessels, after having enveloped the allantois, pierce the chorion, in order to make their way into the uterus, to obtain at once from the blood of the mother both nourishment and the oxygenation of that nourishment.

As to animals with gills, whether fishes or the fry of frogs, the organization of their eggs is much more simple. Without allantois and umbilical vessels, their vitellus communicates with the intestine by a duct, so large that it may be considered as an appendage of it, as a kind of provisionary stomach already filled with food. This is shown both by the observations of du Trochet and Cuvier, and by the old observations of Steno, Haller, and many other anatomists.

In his striking experiments on vomiting, M. Magendie had observed that this operation was preceded by efforts in which the stomach swelled, after a movement of deglutition. He supposed this

to be what we called nausea, and supposed its cause to be the deglutition of air. It was known, indeed, by the experiments of M. Gosse, that the swallowing of air provokes vomiting. A young conscript, in order to appear ill, had carried the art of swallowing air so far as not only to swell his stomach, but likewise his intestines, and this state occasioned violent pain. M. Magendie has proved, by direct experiments, this nature of nausea. Vomiting produced in dogs, either by immediate pressure on the stomach, or by injecting tartar emetic into the veins, always occasioned movements fitted to make air pass through the œsophagus into the stomach, and these movements were quite similar to nausea.

We would willingly add to the department of physiology a memoir of M. de Montegre on ventriloquism. By lessons from M. Comte, who has become so famous by the practice of this singular art, M. Montegre explains, not only the processes by which one can differently modify the sound of his voice; but likewise all the artifices by which we can deceive the hearers with respect to the direction and distance of the sound. Unfortunately, these details are of such a nature as to be understood by examples, and imitated by practice, rather than to be explained by words, at least by words as short as we must employ in our analysis.

### MEDICINE AND SURGERY.

It is more than half a century since the surgeon Garangeot pretended that he had seen a nose which had been bit off in a quarrel, thrown on the ground, and allowed to get cold again, fixed to the face, and adhering as at first. This did not at first excite even surprise; but the miracle was at length called in question, the narrator was generally ridiculed, and no one attempted to repeat the pretended operation. However, a fact, no less extraordinary, which took place in Scotland, has been judicially attested. A finger, entirely cut off, adhered again in a few days, losing merely the nail. It seems, even from different authors of the 16th century, that a lost nose was sometimes supplied by flesh taken from the arm.

M. Percy, who might have had more opportunities than any man of practising these animal engraftments, and who tried them upon dogs, whose sores are so easily cured, could never succeed. He has seen members adhere again, or pieces of flesh, which were only attached by a single tack; but this tack has always been to him a necessary condition. He does not pretend, however, that others may not be more fortunate; on the contrary, he requests surgeons to try every means to render common an operation which seems at first sight to contradict every thing that we know of the animal economy in the higher classes of animals.

Surgeons have long known that when the anterior extremity of the foot alone is affected with gangrene, it is better to amputate a part than to cut off the whole leg, or even its lower extremity; yet this operation has been entirely neglected for many years, and it is only since 1789 that Messrs. Percy and Chopart have put it in

practice, but upon different bones. There is some difficulty in quickly finding the lines of articulation of the bones, and MM. Richeraud, Dupuytren, Roux, and Villermé, have given different marks to guide the operator in looking for them. M. Lisfranc St. Martin, in a memoir read to the Class, has pointed out some additional ones; but he mentions a general inconvenience attending the operation, the drawing back or forced extension of the rest of the foot which the action of the muscles of the calf often occasions when they are no longer counterbalanced by the anterior muscles of the limb; especially when the first cuneiform bone is not preserved, into which the most powerful of these last muscles is inserted. The author recommends particularly this point to the attention of operators.

M. Leveillé, physician in Paris, has presented some interesting facts to the Class respecting the diseases whose progress is stopped by other diseases, and which go on again when these last are cured.

M. Larrey, Inspector of Military Health, has drawn the attention to different ideas contained in a work which he published in 1812, under the title of Memoires de Chirurgie Militaire, &c. Not being able to enter into details, for which the public may have recourse to the printed work, we shall only mention the amputation of the arm by its superior articulation, one of the principal titles of surgical glory of the author, from the security to which he has brought it, by means of a peculiar process, both simple and expeditious, and almost always successful, since he has uniformly saved 90 patients out of 100.

The last two parts of the General Treatise on Poisons, by M. Orfila, a young Spanish physician, have been presented to the Class before being printed. The author treats in them, with his usual attention and sagacity, of vegetable and animal poisons, which, with M. Fodéré, he divides into *acrid*, *narcotic*, *acrid narcotic*, and *septic*. The first produce a violent inflammation; but a part of them merely produce a sympathetical action on the brain, which is the principal cause of death. Others, on the contrary, are absorbed, and act directly on the brain. Opium is neither a stimulant nor a narcotic; its action is quite peculiar; it begins by stupifying, and then produces acute pain and horrible convulsions. The author proves, contrary to Fontana, that the distilled water of lauro-cerasus injected in the veins, even in small quantity, is mortal. The solanums do little harm in our climates; and it is probably from their having been confounded with the belladonna that the contrary has been believed. The most exact experiments have proved to the author that acids, water, and mucilaginous liquids, employed as an antidote to narcotics, hasten death; but that acidulated water is very useful after the poison has been thrown out by an emetic. The infusion of coffee, and blood-letting, are equally useful.

Among the acrid narcotics are upas, camphor, ether, &c. Camphor swallowed or injected acts upon the brain and spinal marrow, and occasions immediate asphyxia. In small pieces, it first ulcerates

the stomach, and occasions a slower death. The introduction of air into the lungs is good against all the poisons that occasion asphyxia.

The author terminates his work by a description of all the spontaneous diseases which may be confounded with poison, as indigestion, cholera-morbus, &c. &c. by pointing out the means of recognising a poisonous substance introduced into the intestines, notwithstanding the alterations which it may have undergone—a very important problem in medical jurisprudence, on the accurate solution of which may depend the lives of many innocent persons, and the punishment of many guilty; for miscreants often have recourse to this mode of bringing the object of their hatred before the judge.

The author, after employing three whole years in the disagreeable experiments which form the basis of his book, proposes, on returning to his native country, to make similar ones on the poisonous plants of the south of Europe. Important results may be still expected from so skilful and zealous an observer; and the Class, to which he promises to communicate his researches, have enrolled him among its Correspondents.

(*To be continued.*)

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## Article XII.

### SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Death of Mr. Henry, of Manchester.**

"Died, on Tuesday, June 18, in the 82d year of his age, Mr. Thomas Henry, President of the Literary and Philosophical Society of Manchester, Fellow of the Royal Society of London, and Member of several other learned Societies, both in this country and abroad. As a practical and philosophical chemist, he had obtained a high and merited reputation. His contributions to that science, besides a small volume of Essays, and his Translations of the early Writings of Lavoisier, which he first introduced to the notice of the English reader, consist chiefly of memoirs, dispersed through the Transactions of the various Societies to which he belonged, and relating both to those parts of chemistry that are purely scientific, and to those which have a connexion with the useful arts. On a subject intimately connected with the success of the cotton manufacture (the employment of mordaunts or bases in dyeing), "Mr. Henry was the first," to use the words applied to him by a subsequent author, "who thought and wrote philosophically." In the introduction, too, of the new mode of bleaching, which has worked

* Copied from the Manchester Chronicle for June 22, 1816.—T.

an entire revolution in that art, and occasioned an incomparably quicker circulation of capital, he was one of the earliest and most successful agents. In addition to the acquirements connected with his profession, he had cultivated, to no inconsiderable a degree, a taste for the productions of the Fine Arts; he had obtained a knowledge of historical events remarkable for its extent and accuracy; and he had derived, from reading and reflection, opinions to which he was steadily attached, on those topics of political, moral, and religious inquiry, which are most important to the welfare of mankind. For several years past he had retired from the practice of medicine, in which he had been extensively engaged, with credit and success, for more than half a century; and, from delicate health, he had long ceased to take an active share in the practical cultivation of science. But possessing, almost unimpaired, his faculties of memory and judgment, he continued to feel a lively interest in the advancement of literature and philosophy. Retaining, also, in their full vigour, those kind affections of the heart that gave birth to the most estimable moral qualities, and secured the faithful attachment of his friends, he passed through a long and serene old age, experiencing little but its comforts and its honours, and habitually thankful for the blessings with which Providence had indulged him."

## II. *Showers of Fish.*

In Prince of Wales's Island, in the East Indies, the inhabitants usually collect the rain-water in tanks placed upon the tops of their houses. Frequently these tanks are completely dry for weeks together. When the rainy season comes, they are speedily filled with water. Small fishes are found swimming about in this water, which gradually increase, and acquire the length of several inches. I have been told that the same thing happens in Bengal. These fishes must come down with the rain. It is a matter of some curiosity to be able to explain the source from which these animals are derived. Perhaps some of my readers may be able to inform the public what the name of this fish is, and to what species it belongs, and whether it be a salt or fresh-water fish. My information was obtained from an East India Captain, who assured me he had seen the fishes frequently, though he was ignorant of their name, and could not describe their appearance with sufficient precision to enable us to make out the species.

## III. *Insect which appears in Brine.*

Connected with the preceding curious phenomenon is one which I have myself witnessed, and which is no doubt known to many of my readers. In the Isle of Wight, a few miles east from Ryde, there is a place where common salt is made from sea water. The sea water is let into a kind of shallow and wide pit, where it is gradually concentrated by spontaneous evaporation. It is then boiled down in the usual way, and the salt extracted from it. I saw the

water in the pit in a concentrated state. It was full of a small insect that was swimming about in it very briskly. On questioning the workmen about this insect, I was told by them that it never made its appearance till the brine had become of a certain strength, and that it was most abundant of all when it had acquired its greatest degree of concentration. I conceive that this is the *cancer salinus* of Linnæus, of which there is a figure and an account by Mr. Rackett, in the Transactions of the Linnæan Society, vol. xi. p. 205.

### IV. *Combinations of Oxygen and Azote.*

Gay-Lussac has lately read a paper to the Academy of Sciences the object of which was to describe the properties of *nitrous acid*, which he has first succeeded in obtaining in a separate state. He found his new acid to be composed of 100 oxygen gas + 400 nitrous gas. Nitrous vapour is formed of 100 oxygen gas + 200 nitrous gas, and nitric acid of 100 oxygen gas + 133 nitrous gas. The new acid can be obtained only in combination with a base, an acid, or with water. It is resolved into nitrous vapour and nitrous gas whenever we attempt to obtain it in a separate state. We obtain it in combination with water by decomposing the nitrate of lead by heat, and condensing the gaseous products by means of ice, or a mixture of ice and salt; for it is very volatile. Water alone added to this combination is sufficient to decompose it, and to disengage a great quantity of nitrous gas. Nitrous vapour, on the contrary, exists only in a separate state, or at least is decomposed with extreme facility, when combined with an alkali, sulphuric acid, or even with water. When absorbed by potash, it produces nitric acid and the new acid.

Gay-Lussac has shown that no oxynitric acid exists, and that 100 oxygen gas and 133 nitrous gas form colourless nitric acid. For that purpose he employed the solution of red sulphate of manganese, which is instantly discoloured by all the nitrous acids, but not by nitric acid.

The five combinations of azote and oxygen may, therefore, be represented as in the following table :—

| | *Volumes.* | | | *Atoms.* | | |
|---|---|---|---|---|---|---|
| | Azote. | | Oxygen. | Az. | | Oxy. |
| Oxide of azote .... | 100 | + | 50 | 1 | + | 1 |
| Nitrous gas ..... | 100 | + | 100 | 1 | + | 2 |
| Nitrous acid ..... | 100 | + | 150 | 1 | + | 3 |
| Nitrous vapour .... | 100 | + | 200 | 1 | + | 4 |
| Nitric acid ..... | 100 | + | 250 | 1 | + | 5 |

The novelty of this memoir consists in the description of the properties of nitrous acid. Whoever will take the trouble to read what I have written about nitrous acid will find that I was all along aware of the difference between it and nitrous vapour. In my table

of the weight of atoms (*Annals of Philosophy*, vol. iii. p. 135), I have given the very same composition of the different combinations of azote and oxygen as in the above table, omitting only nitrous vapour, which I think should rather be considered as a compound of nitric acid and nitrous gas than of azote and oxygen. Dalton stated long ago that 100 oxygen combine with 180 nitrous gas, or with 360. I had corrected these experiments from the doctrine of volumes, and had concluded that the correct quantities were 100 oxygen + 200 nitrous gas, or 400 nitrous gas, which are precisely the numbers given by Gay-Lussac.

V. *Account of an extraordinary Explosion on Board a Ship laden with Coals.* By Mr. Pemberton.

An extraordinary occurrence lately happened to a brigantine lying in the harbour of Sunderland, in the county of Durham. She began to take in a lading of coals at six o'clock of the morning of Tuesday, the 18th of this month, and completed it about six in the evening of the same day; when the main hatchway was battened down, and covered with a tarpaulin, and the others also closed up. The ship was laden with Nesham's Main coal, and carries about 175 London chaldrons. Nobody was on board from that time till near midnight, when two of the apprentices came to sleep there. One of them, George Watson, aged 15, got to bed in the half deck, laying his clothes on the floor under his hammock, as he said. In the left pocket of his waistcoat he admitted there was some gunpowder, the contents of a musket cartridge that had been given him. But the master of the ship said this boy is addicted to lying; so his testimony must be received with caution. The other boy, James Norton, about 19 years old, struck a light for some purpose before he went to bed; and on going to light a lamp in the half deck with the kindled match, he observed the flame of the match to turn bluish, and he thought a spark fell from it, which raised an explosion in the half deck, that drove him violently against its hatch or cover. At the same time an explosion happened at the cable stage, 28 feet distant from the half deck, where the boys were driven off the hatch, and burst into a large blaze eight or nine feet high, that ran up the fore-mast to a great height. This hatch was also fastened down, but has two holes in it big enough to let the cable run through.

This is the substance of the information given by the two apprentices on board, and by two others who were on the quay near the ship at the time, and saw the explosion at the cable hatchway. Another magistrate and myself examined them, and saw no reason to doubt the general correctness of their report. There was no possibility of a train of gunpowder being laid under the ship's deck from one hatchway to the other; for she was full of coals up to the deck. Whether George Watson had scattered any of the powder which he had on the half deck beneath the lamp, so as to be kindled by the spark that fell, is uncertain; but the little quantity he had

was gone, without any apparent damage to the pocket, or waistcoat, or his other clothes; and it seems doubtful whether the explosion excited there, by which Norton was driven violently against the half deck hatch, can be wholly ascribed to so small a quantity of powder. Watson, who was in his hammock, only three or four feet off, on seeing the flame, covered himself over with his blanket, and suffered no harm. Nor is it at all probable, or even credible, that the powder, supposing it to have been ignited, should extend its effect for 28 feet along the surface of the coals, to the cable stage, and do so much mischief there; for not only did the surface of the coals in the hold appear sooty, as if they had been exposed to a flame, but some pieces appeared a little charred. Two deck beams at or near the cable stage were quite broken, and another much damaged; many knees were split, and many boards of the deck there blown up and broken, and the hatch forced open. A cable was damaged by the fire, the windlass was shattered, and some of the fore-shrouds scorched.

It appears, therefore, that the explosion was much stronger at the cable stage than where it began. But as this is not the ordinary effect of gunpowder, especially in so small a quantity, I am led to suspect that another powerful agent was concerned in producing it: I mean carbureted hydrogen gas; for I conceive it very possible that a cargo of coals in a ship, whose hatchways are all closed, may, under certain circumstances, generate a quantity of carbureted hydrogen gas; and that this gas may be accumulated in a period of close confinement so as to attain the inflammable point; and when this happens, it follows of course that a burning match or flaming gunpowder may kindle the gas. This theory enables us to account for the blueness of the flame of the match employed to light the lamp, this colour of the flame being always considered by pitmen as a sign of hydrogen gas, or fire-damp, as they call it. And it is remarkable that Norton did not notice this colour of the flame where he lighted the match, on a box standing at the bottom of the half deck, but only when he brought it near the lamp at the upper part of it, where the hydrogen gas would be collected, on account of its specific levity. Suppose, then, the match to have kindled the carbureted hydrogen gas at the upper part of the half deck of the ship, the first effect would be an explosion there, which might drive Norton strongly against the hatch, so as to force it quite off its place. And this happened accordingly. But as the hydrogen gas takes the uppermost region it can find, it would be found between the coals and the deck; and being kindled at the explosion in the quarter deck, would instantly spread over the coals, scorching them as it passed, till it came to the cable stage, when its force had become so great that it heated and scorched parts of the cable there, broke beams and boards, burst open the hatch, and did the other damage already described. It seems to have passed under the main hatch without hurting it, because it was strongly secured, and the burning gas found ready vent further along. When it reached the cable

stage, it probably became more violent, from an accession of air, and a communication with the atmosphere, so as to produce the luminous explosion. Not having gunpowder enough to account for the phenomena that occurred, I have hazarded this novel opinion with much diffidence, but with the concurrence of the Gentleman who favoured me with his assistance in examining the apprentices, and agreed with me in his judgment of the case.

It is said that some flashes of lightning were seen that night in this neighbourhood, without thunder; but the apprentices had not observed it, and two or three other persons, who supposed they had seen lightning about the time the explosion happened, were afterwards convinced that they had mistaken for it the light caused by the fire in the ship. They heard no thunder. Besides, it does not appear that the ordinary effects of lightning on a ship agree with the phenomena that occurred on this occasion.

I have since heard that many years ago a ship laden with coals from Sunderland took fire, and was burnt at Portsmouth or Plymouth; and that the damage was commonly supposed to have been caused by the sulphur of the coal taking fire, as had been often noticed in the heaps of waste round coal-pits. The production and nature of carbureted hydrogen gas were then little known, but it probably was the cause of this ship being burnt.

*Bishop Wearmouth, June* 21, 1816. H. PEMBERTON.

## VI. *Curious Galvanic Experiments.* By Mr. Porrett.

(To Dr. Thomson.)

DEAR SIR, *Tower, June* 6, 1816.

The conversation which took place at your house the other evening on the subject of voltaic arrangements, brought to my recollection two experiments that I made about 18 months since, which appear to me to be interesting, the second one especially. I did not then publish them, conceiving that at some future time, when leisure and inclination should coincide, I might follow up with some advantage the new facts which these elementary experiments disclosed; but as it continues to be very uncertain when this coincidence will take place, and still more so whether I should be able to do any thing like justice to the subject, I will no longer be induced to delay giving the information which is now in my power, by the hope (probably fallacious) of hereafter making it more complete. The experiments are, therefore, quite at your service for publication, in case you think proper to honour them with a place in your *Annals.* They are as follow:—

*Exper.* 1.—A small battery of 50 pairs, of $1\frac{1}{4}$ inch double plates, was put into action by diluted muriatic acid. The wires from its two extremities were immersed in water, the decomposition of which of course took place rapidly. The battery was left thus disposed until its action on the water had nearly ceased to be visible. At this period the greater part of the liquid contained in the cells of the battery was withdrawn by means of a syringe, only about $\frac{1}{4}$

of the original quantity being left. The effect of this abstraction was a renewal of the rapid decomposing action on the water, which the battery exercised at the commencement of the experiment. I ascertained that this extraordinary effect was not occasioned by the increased distance between the surfaces of the liquid in the cells, by constructing a battery with very deep cells, from which I could withdraw as much liquid as before, but without uncovering the plates, an operation which I found to be quite inefficacious. It appears to me, therefore, that the explanation of this fact must be sought for in the action of the atmosphere upon the humid surfaces of the metallic plates.

*Exper.* 2.—I took an ounce medicine phial, and with a red-hot rod of iron cut it in a horizontal direction, so as to form the lower part into a small jar. I threw away the upper part, and divided the small jar into two equal parts in the direction of its length, so as to make a vertical section of it. The two halves of the jar were then pressed together in their original position, having first interposed a piece of moistened bladder. All the parts of the bladder which protruded beyond the outside of the jar were then cut away; and when this was completed, melted sealing-wax was run down the outer edge of the bladder, and thus the two halves of the glass vessel were firmly united. By this means the inside of the glass jar was divided into two cells, by the bladder interposed between them.

One of these cells having been filled with water, and left for several hours, was found to have retained the water. The bladder, therefore, was not sufficiently porous to allow the water to filtrate through it. The cell filled with water was now positively electrified, with a battery of 80 pairs of $1\frac{1}{4}$ inch double plates, and a few drops of water were put into the empty cell, so as to cover the bottom of it. This small quantity of water was then negatively electrified. The phenomena which ensued were exceedingly curious and instructive. Independent of the decomposition of a small part of the water, which of course took place in the usual manner, the principal part of it obeyed the impulse of the voltaic current *from the positive to the negative wire*, first overcoming the resistance occasioned by the compact texture of the bladder, so as in about half an hour to have brought the water in both cells to the same level, and afterwards overcoming the additional resistance occasioned by the gravitation of the water, by continuing to convey that fluid into the negative cell, until its surface in that cell was upwards of $\frac{3}{4}$ of an inch higher than in the positive cell. A much greater difference of level might doubtless be obtained by operating with a larger apparatus, and for a longer time; but the results are perfectly conclusive when the experiment is performed on the small scale in which I tried it.

I have repeated the above experiment several times, and invariably found the liquid, whatever it was, descend on the side posi-

tively electrified, and ascend on that negatively electrified, chemical changes at the same time going on, as in the celebrated transfer experiments of Sir H. Davy; but those experiments could not show the mechanical action of the voltaic current, consequently only the chemical action was observed in them. To render the mechanical action evident, it is an indispensable condition that there should be interposed between the positively and negatively electrified liquids a body which, although porous, is yet sufficiently compact to prevent filtration taking place in common circumstances. Bladder answers this condition. I do not think, however, that it does so as well as filtering paper that has been prepared in the following manner, suggested to me by my very ingenious friend Mr. Wilson, of Guy's Hospital:—Spread the white of an egg thinly upon filtering paper; then immerse the paper into boiling water, so as to coagulate the albumen; it is then well adapted for these experiments. Thick paper of a very compact texture would probably do without this preparation; but I cannot state this positively, not having tried it.

I think that by the above experiment I have demonstrated the existence of a power not before noticed in the voltaic current, namely, that of conveying fluids through minute pores not otherwise pervious to them, and of overcoming the force of gravity.

Is not this electro-filtration, jointly with electro-chemical action, in constant operation in the minute vessels and pores of the animal system?

I wish that some person well versed in the sciences of anatomy, chemistry, and electricity, would answer this question. I am not qualified to attempt its solution, being a stranger to the first mentioned science, and possessing but a moderate knowledge of the other two; and it appears to me that only a proficient in all should venture to propose any new physiological opinions, but I cannot help thinking that an affirmative answer to the above question is capable of a good defence.

To those who may be inclined to repeat the preceding experiment, it may be useful to mention that by letting fall from a dropping tube a little sulphuric acid into the cells of the battery occasionally, its action is prolonged, without the trouble of renewing the liquid in the cells, or the inconvenience of disturbing the whole arrangement, the partial action of this dense acid on the plates is prevented by stirring the liquid afterwards with a little stick.

I remain, with much respect, dear Sir,

Yours very truly,

R. PORRETT, jun.

VII. *Prize proposed by the Royal Medical Society of Edinburgh.*

"What are the chemical changes produced in the air by the growth of plants, and do they on the whole purify or deteriorate the atmosphere?"

A set of books, or a medal of five guineas value, shall be given

annually to the author of the best dissertation on an experimental subject proposed by the Society; for which all the members, honorary, extraordinary, and ordinary, shall alone be invited as candidates.

The dissertations are to be written in English, French, or Latin, and to be delivered to the Secretary on or before Dec. 1, of the succeeding year to that in which the subjects are proposed; and the adjudication of the prize shall take place in the last week of February following.

To each dissertation shall be prefixed a motto; and this motto is to be written on the outside of a sealed packet, containing the name and address of the author. No dissertation will be received with the author's name affixed; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

## VIII. *Modification of the Linnæan Arrangement of Plants.*

Mr. Clairville, the author of L'Entomologie Helvetique,* published in 1811 a small botanical work, entitled Manuel d'Herborisation en Suisse et en Valais, redigé selon le Systeme de Linnè. Corrigé par ses propres Principes. In the preface he says he means this work for amateur botanists during their travels among the Alps, and wishing to make it as short as possible, has endeavoured to fix on the essential distinctive character of each genus and species.

Admiring the Linnæan System as an artificial one, and thinking that some of its anomalies might be removed by more strictly attending to the principles laid down in the Philosophia Botannica, he set himself to seek for the cause of its defects. He thinks he has discovered it, in a great measure, to be owing to the manner in which Linnæus has treated the defective flowers; for example, the not counting the imperfect stamina and abortive germina. He considers it as a certain mode of finding the true number of stamina to observe that it generally answers to the number of petals, or of divisions in a monopetalous corolla, or to their double. The adoption of this rule does away with the class Polygamia.

The classes Monadelphia, Diadelphia, and Polyadelphia, are also done away with, because he does not think the circumstance of the union of the filaments a good character, as several plants that are in that respect really monadelphous are nevertheless left in the other classes: the papilionaceous plants are therefore made a subdivision of the Decandria Monogynia.

He has united the classes Monœcia and Diœcia into one under the name of Hetuothalmia, which he has subdivided into, 1. Flowers solitary. 2. Flowers andiogynous, assembled in a common envelope: and, 3. Flowers in catkins.

He has altered the name of the class Syngenesia to that of Solenandica (anthers forming a tube), and, consistently with his principle of excluding defects from being essential characters, has changed

* Of which we shall shortly give an analysis.

the orders, and divided the class into, 1. Flowers congregate, the former subdivided into the semiflosculous, the discoid, and the capitate: and, 2. Flowers segregate. The other classes remain the same. In the arrangement of the genera of each class there appears some improvement, for instance, in that of Pentandria Digynia, the umbelliferous plants are placed according to the resemblance of their fruits, and, in general, pains appear to have been taken to keep Jussieu's natural families of plants as much together as was consistent with the Linnæan system.

Upon the whole, the work appears to be very much what the author described it in his preface. It is certainly not calculated for quite a beginner, as there are no entire descriptions of each genus, but only their essential characters. It is to be wished that the author had entered more at large on the system founded on the anthers, of which he gives a slight indication, as he truly observes, "qu'on ne peut observer la nature sous trop de points de vue, et c'est toujours gagner beaucoup que d'en saisir un nouveau."

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## ARTICLE XIII.

### *Scientific Books in hand, or in the Press.*

The first number of a work entitled, Strata identified by organized Fossils, containing Prints, on *coloured Paper*, of the most characteristic Specimens in each Stratum, by Mr. William Smith, Mineral Surveyor, and Author of Maps of the Strata of England and Wales, has just made its appearance. The Plates are engraved by Sowerby, and references are made to his work on British Mineralogy, and the Mineral Conchology of Great Britain.

Mr. Sowerby, having concluded the work on English Botany, in which he has been so long engaged, is now continuing his British Mineralogy, with the intention of completing it in three or four more numbers, with copious Indexes. He hopes his friends will assist him to make it as complete as possible.

Mr. Wm. Phillips will publish, early in July, a new edition of his Outlines of Mineralogy and Geology, Revised and Improved. To this edition will be added some Account of the Geology of England and Wales, together with a Coloured Map and Section of the Strata, which is intended also to be published separately, for the use of the purchasers of the first edition.

The Third Volume of the Transactions of the Geological Society will be published about the middle of July. It will be illustrated by a large number of highly finished Plates, chiefly Coloured.

Mr. George Kerr, of Aberdeen, is about to publish Observations on the Harveian Doctrine of the Circulation of the Blood.

Mr. Berry, author of a History of Guernsey, has in the Press a series of Tables, entitled the Genealogical Mythology, intended as a Book of Reference for Classical Students.

Mr. Holmes's work on the Coal Mines is nearly ready for publication.

## Article XIV.

# METEOROLOGICAL TABLE.

| 1816. | Wind. | Barometer. Max. | Min. | Med. | Thermometer. Max. | Min. | Med. | Hygr. at 9 a. m. | Rain. | |
|---|---|---|---|---|---|---|---|---|---|---|
| 5th Mo. | | | | | | | | | | |
| May 19 | N E | 29·82 | 29·76 | 29·790 | 55 | 35 | 45·0 | 51 | | ☾ |
| 20 | E | 29·84 | 29·82 | 29·830 | 65 | 36 | 50·5 | 48 | | |
| 21 | N E | 29·84 | 29·77 | 29·805 | 62 | 41 | 51·5 | | | |
| 22 | N E | 29·82 | 29·77 | 29·795 | 66 | 40 | 53·0 | 60 | | |
| 23 | N | 29·82 | 29·77 | 29·795 | 65 | 48 | 56·5 | 75 | | |
| 24 | N | 29·77 | 29·75 | 29·760 | 56 | 50 | 53·0 | 70 | — | |
| 25 | S W | 30·03 | 29·77 | 29·900 | 62 | 38 | 50·0 | 45 | ·20 | |
| 26 | S W | 30·03 | 29·90 | 29·965 | 66 | 43 | 54·5 | 73 | ·28 | |
| 27 | N E | 30·05 | 29·90 | 29·975 | 62 | 46 | 54·0 | 45 | — | ● |
| 28 | S E | 30·12 | 30·10 | 30·110 | 66 | 51 | 58·5 | | | |
| 29 | S E | 30·10 | 29·87 | 29·985 | 69 | 54 | 61·5 | | | |
| 30 | N W | 29·87 | 29·80 | 29·835 | 67 | 49 | 58·0 | | | |
| 31 | N E | 30·00 | 29·80 | 29·900 | 64 | 46 | 55·0 | | | |
| 6th Mo. | | | | | | | | | | |
| June 1 | N W | 30·00 | 30·00 | 30·000 | 70 | 55 | 62·5 | | | |
| 2 | N W | 30·00 | 29·90 | 29·950 | 72 | 49 | 60·5 | | | |
| 3 | N W | 30·08 | 29·97 | 30·025 | 65 | 46 | 55·5 | | | ☽ |
| 4 | S W | 30·05 | 29·90 | 29·975 | 65 | 50 | 57·5 | | | |
| 5 | N W | 29·96 | 29·85 | 29·905 | 67 | 41 | 54·0 | 38 | — | |
| 6 | N W | 29·90 | 29·80 | 29·850 | 64 | 44 | 54·0 | 45 | — | |
| 7 | Var. | 29·76 | 29·48 | 29·620 | 61 | 46 | 53·5 | 58 | ·20 | |
| 8 | W | 29·40 | 29·15 | 29·225 | 62 | 42 | 52·0 | 70 | ·27 | |
| 9 | N W | 29·52 | 29·35 | 29·435 | 57 | 37 | 47·0 | 47 | 1 | |
| 10 | N E | 29·86 | 29·52 | 29·690 | 58 | 41 | 49·5 | 47 | ·22 | ○ |
| 11 | S W | 30·00 | 29·86 | 29·930 | 65 | 39 | 52·0 | 45 | | |
| 12 | S E | 30·00 | 29·97 | 29·985 | 70 | 37 | 53·5 | 39 | | |
| 13 | S W | 29·97 | 29·91 | 29·940 | 75 | 54 | 64·5 | 53 | | |
| 14 | N | 29·91 | 29·87 | 29·890 | 53 | 48 | 50·5 | 57 | 8 | |
| 15 | N | 29·92 | 29·86 | 29·890 | 59 | 44 | 51·5 | 47 | | |
| 16 | N W | 29·92 | 29·92 | 29·920 | 67 | 36 | 51·5 | 45 | | |
| | | 30·12 | 29·15 | 29·850 | 75 | 35 | 54·15 | 52 | 1·26 | |

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.*—19. Hoar frost: a fine day. 20. Clear morning. 23. a. m. Cloudy: much wind at N. 24. Misty: small rain at intervals. The hygrometer, these two days, noted at seven, a. m. 25. a. m. Overcast: wind at S. W.: rain, evening and night. 26. *Cumulus* cloud by day: *Cirrostratus* at evening. The hygrometer noted at half past six, a. m. 27. A wet morning, succeeded by close *Cumulostratus* through the day. 28—31, inclusive. Fair days.

*Sixth Month.*—2. A fine breeze: large *Cirri* and *Cumuli.* 3. *Cirrostratus* prevails, with a cooler atmosphere. 5, 6. Showery. 7—10. Rain. 12. The hygr. receded to 30°: *Cumulus* prevailed, and was succeeded by *Cirrus* in the evening. 13. This afternoon there was a fine, but transient, display of *Cirrocumulus.* In the N. and N. W. there was an obscurity, mixed with rudiments of *Nimbi.* 14. After a warm, still night, a cold blowing day, with small rain at intervals. 15. Overcast, with *Cumulostratus:* cool breeze: in the evening, *Cirrostratus.* 16. A fine day: the air becomes calmer.

## RESULTS.

Winds rather variable, but for the most part Northerly.

| | | |
|---|---|---|
| Barometer: | Greatest height ..... | 30·12 inches |
| | Least ..... | 29·15 |
| | Mean of the period .... | 29·850 |
| Thermometer: | Greatest height ..... | 75° |
| | Least ..... | 35 |
| | Mean of the period .... | 54·15 |

Mean of the hygrometer (for 20 days), 52°. Rain, 1·26 inch.

The character of this period has been, on the whole, ungenial; though not one frosty night has occurred, yet cloudy weather, with blighting winds, mostly predominated; and the mean temperature turns out nearly 5° lower than that of the corresponding portion of 1815.

TOTTENHAM, L. HOWARD.
*Sixth Month,* 17, 1816.

ANNALS

OF

PHILOSOPHY.

*AUGUST*, 1816.

## Article I.

*Biographical Account of Dr. Benjamin Rush, of Philadelphia.* By David Hosack, M.D. F.R.S. F.L.S. Professor of the Theory and Practice of Physic and Clinical Medicine in the University of the State of New York.*

DR. RUSH was born Dec. 24, 1745, on his father's estate, about 12 miles from the city of Philadelphia. His ancestors followed William Penn from England to Pennsylvania, in the year 1683. They chiefly belonged to the society of Quakers, and were all, as well as his parents, distinguished for the industry, the virtue, and the piety, characteristic of their sect. His grandfather, James Rush, whose occupation was that of a gunsmith, resided on his estate near Philadelphia, and died in the year 1727. His son John, the father of Dr. Rush, inherited both his trade and his farm, and was equally distinguished for his industry and ingenuity. He died while his son Benjamin was yet young, but left him to the care of an excellent and pious mother, who took an active interest in his education and welfare. In a letter which I had the pleasure to receive from Dr. Rush a short time before his death, and which was written upon his return from a visit to the tomb of his ancestors, he thus expresses the obligation he felt for the early impressions of piety he had received from his parents:—

"I have acquired and received nothing from the world which I prize so highly as the religious principles I inherited from them;

* From An Introductory Discourse to a Course of Lectures on the Theory and Practice of Physic, by Dr. Hosack, delivered at the College of Physicians and Surgeons, Nov. 3, 1813.

and I possess nothing that I value so much as the innocence and purity of their characters."*

But this was not the only source of that virtue and religion for which he was so eminently distinguished. His mother, as if influenced with a presentiment of the future destinies of her son, resolved to give him the advantages of the best education which our country then afforded. For this purpose he was sent, at the early age of eight or nine years, to the West Nottingham Grammar School, and placed under the care of his maternal uncle, the Rev. Dr. Samuel Finley, an excellent scholar and an eminent teacher, and whose talents and learning afterwards elevated him to the Presidency of the College of Princeton. At this school young Rush remained five years, for the purpose of acquiring a knowledge of the Greek and Latin languages, and other branches necessary to qualify him, as preparatory for a collegiate course of study. But under the tuition and guidance of Dr. Finley, he was not only instructed in classical literature; he also acquired, what was of no less importance, and which characterized him through life—a habit of study and observation, a reverence for the Christian religion, and the habitual performance of the duties it inculcates; for his accomplished and pious instructor not only regarded the temporal, but the spiritual, welfare of those committed to his care.

At the age of 14, after completing his course of classical studies, he was removed to the College of Princeton, then under the superintendance of President Davies, one of the most eloquent preachers and learned divines our country has produced.

At College, our pupil not only performed his duties with his usual attention and success, but he became distinguished for his talents, his uncommon progress in his studies, and especially for his eloquence in public speaking. For this latter acquirement he was doubtless indebted to the example set before him by President Davies, whose talents as a pulpit orator were universally acknowledged, and were frequently the theme of his pupil's admiration.

Dr. Rush received the degree of Bachelor of Arts in the autumn of 1760, at the early age of 15. The next succeeding six years of his life were devoted to the study of medicine, under the direction of Dr. John Redman, at that time an eminent practitioner in the city of Philadelphia. Upon commencing the study of medicine, the writings of Hippocrates were among the very first works which attracted his attention; and, as an evidence of the early impression hey made upon his mind, and of the attachment he had formed to them, let it be remembered that Dr. Rush, when a student of medicine, translated the aphorisms of Hippocrates from the Greek into his vernacular tongue, in the 17th year of his age. From this early exercise he probably derived that talent of investigation, that

* The letter here referred to was originally addressed by Dr. Rush to the Hon. John Adams, Esq. late President of the United States: from a copy of the same, sent to the author by Dr. Rush, several of the preceding interesting particulars have been taken.

spirit of inquiry, and those extensive views of the nature and causes of disease, which give value to his writings, and have added important benefits to the science of medicine. The same mode of acquiring knowledge which was recommended by Mr. Locke, with the very manner of his commonplace book, was also early adopted by Dr. Rush, and was daily continued to the last of his life To his records, made in 1762, we are at this day indebted for many important facts illustrative of the yellow fever, which prevailed in, and desolated the city of Philadelphia, in that memorable year. Even in reading, it was the practice of Dr. Rush, and for which he was first indebted to his friend Dr. Franklin, to mark with a pen or pencil any important fact, or any peculiar expression, remarkable either for its strength or its elegance. Like Gibbon, " he investigated with his pen always in his hand;" believing, with an ancient classic, " that to study without a pen is to dream—" Studium sine calamo, somnium."

Having with great fidelity completed his course of medical studies under Dr. Redman, he embarked for Europe, and passed two years at the University of Edinburgh, attending the lectures of those celebrated professors, Dr. Monro, Dr. Gregory, Dr. Cullen, and Dr. Black.

In the spring of 1768, after defending an inaugural dissertation " De Coctione Ciborum in Ventriculo," he received the degree of Doctor of Medicine. In that exercise which was written with classical purity and elegance, it was the object of Dr. Rush to illustrate by experiment an opinion that had been expressed by Dr. Cullen, that the aliment, in a few hours after being received into the stomach, undergoes the acetous fermentation. This fact he established by three different experiments made upon himself; experiments which a mind less ardent in the pursuit of truth would readily have declined.

From Edinburgh Dr. Rush proceeded to London, where, in attendance upon hospitals of that city, the lectures of its celebrated teachers, and the society of the learned, he made many accessions to the stock of knowledge he had already acquired.

In the spring of 1769, after visiting Paris, he returned to his native country, and immediately commenced the practice of physic in the city of Philadelphia, in which he soon became eminently distinguished.

Few men have entered the profession in any age or country with more numerous qualifications as a physician than those possessed by Dr. Rush. His gentleness of manner, his sympathy with the distressed, his kindness to the poor, his varied and extensive erudition, his professional acquirements, and his faithful attention to the sick, all united in procuring for him the esteem, the respect, and the confidence of his fellow citizens, and thereby introducing him to an extensive and lucrative practice.

It is observed, as an evidence of the diligence and fidelity with

which Dr. Rush devoted himself to his medical studies, during the six years he had been the pupil of Dr. Redman, that he absented himself from his business but two days in the whole of that period of time. I believe it may also be said that from the time he commenced the practice of medicine to the termination of his long and valuable life, except when confined by sickness, or occupied by business of a public nature, he never absented himself from the city of Philadelphia, nor omitted the performance of his professional duties a single day. It is also stated that during the thirty years of his attendance as a physician to the Pennsylvania hospital, such was his punctuality, his love of order, and his sense of duty, that he not only made his daily visit to that institution, but was never absent ten minutes after the appointed hour of prescribing.

In a few months after his establishment in Philadelphia, Dr. Rush was elected a Professor in the Medical School, which had then been recently established by the laudable exertions of Dr. Shippen, Dr. Kuhn, Dr. Morgan, and Dr. Bond. For this station his talents and education peculiarly qualified him. As in the case of Boerhaave, such too had been the attention bestowed by Dr. Rush upon every branch of medicine, that he was equally prepared to fill any department in which his services might be required.

The Professorships of Anatomy, the Theory and Practice of Physic, Clinical Medicine, and the Materia Medica, being already occupied, he was placed in the chair of Chemistry, which he filled in such manner as immediately to attract the attention of all who heard him, not only to the branch he taught, but to the learning, the abilities, and eloquence of the teacher.

In the year 1789 Dr. Rush was elected the successor of Dr. Morgan to the chair of the Theory and Practice of Physic. In 1791, upon an union being effected between the College of Philadelphia and the University of Pennsylvania, he was appointed to the Professorship of the Institutes of Medicine and Clinical Practice; and in 1805, upon the resignation of the learned and venerable Dr. Kuhn, he was chosen to the united Professorships of the Theory and Practice of Physic and of Clinical Medicine, which he held the remainder of his life. To the success with which these several branches of medicine were taught by Dr. Rush, the popularity of his lectures, the yearly increase of the number of his pupils, the unexampled growth of the Medical School of Philadelphia, and the consequent diffusion of medical learning, bear ample testimony; for, with all due respect to the distinguished talents with which the other Professorships of that University have hitherto been, and still continue to be filled, it will be admitted that to the learning, the abilities, and the eloquence of Dr. Rush, it owes much of that celebrity and elevation to which it has attained. What Boerhaave was to the Medical School of Leyden, or Dr. Cullen to that of Edinburgh, Dr. Rush was to the University of Pennsylvania.

But Dr. Rush did not confine his attention and pursuits either to

the practice of medicine or to the duties of his Professorship: his ardent mind did not permit him to be an inactive spectator of those important public events which occurred in the early period of his life.

The American revolution; the independence of his country; the establishment of a new constitution of government for the United States, and the amelioration of the constitution of his own particular state, all successively interested his feelings, and induced him to take an active concern in the scenes that were passing. He held a seat in the celebrated Congress of 1776 as a representative of the state of Pennsylvania, and subscribed the ever memorable instrument of American independence. In 1777 he was appointed Physician General of the Military Hospital for the Middle Department; and in the year 1787 he received the additional gratification and evidence of his country's confidence in his talents, his integrity, and his patriotism, by being chosen a member of the State Convention for the adoption of the Federal Constitution.

These great events being accomplished, Dr. Rush gradually retired from political life, resolved to dedicate the remainder of his days to the practice of his profession, the performance of his collegiate duties, and the publication of those doctrines and principles in medicine which he considered calculated to advance the interests of his favourite science, or to diminish the evils of human life. In a letter which I received from him as early as the year 1794, he expresses this determination, adding, "I have lately become a mere spectator of all public events." And in a conversation on this subject, during the last two years of his life, he expressed to me the high gratification which he enjoyed in his medical studies and pursuits, and his regret that he had not at a much earlier period withdrawn his attention from all other subjects, and bestowed it exclusively upon his profession.

Such was the attachment of Dr. Rush to his profession, that, speaking of his approaching dissolution, he remarks, "when that time shall come, I shall relinquish many attractions to life, and among them a pleasure which to me has no equal in human pursuits; I mean that which I derive from studying, teaching, and practising medicine." But he loved it as a science; principles in medicine were the great objects of all his inquiries. He has well observed that medicine without principles is a humble art and a degrading occupation; but, directed by principles,—the only sure guide to a safe and successful practice,—it imparts the highest elevation to the intellectual and moral character of man.

But the high professional character and attainments of Dr. Rush did not alone display themselves in his skill as a physician, or his abilities as a teacher; he was equally distinguished as a writer and an author.

The present occasion does not allow me to recite even the numerous subjects of his medical publications; much less does it afford

an opportunity to review the opinions they contain. I must, however, observe generally, that the numerous facts and principles which the writings of Dr. Rush contain, the doctrines they inculcate relative to the nature and causes of disease, and the improvements they have introduced into the practice of medicine, recommend them to an attentive perusal and study, while the perspicuity and elegance of the style in which they are written give them an additional claim to attention as among the finest models of composition. The same remarks are equally applicable to the epistolary stile of Dr. Rush, and that of his conversation; in both of which he eminently excelled.

Mr. Fox declared in the British House of Commons that he had learned more from Mr. Burke's conversation than from all the books he had ever read. It may also be observed of the conversation of Dr. Rush, that such were the riches of his mind; such was the active employment of all its faculties; so constant was his habit of giving expression to his thoughts in an extensive correspondence, in the preparation of his public discourses, and in his daily intercourse with the world, that few persons ever left his society without receiving instruction, and expressing their astonishment at the perpetual stream of eloquence in which his thoughts were communicated.

It has frequently been the subject of surprise that amidst the numerous avocations of Dr. Rush, as a practitioner and a teacher of medicine, that he found leisure for the composition and the publication of the numerous medical and literary works which have been the production of his pen.

Although Dr. Rush possessed by nature an active and discriminating mind, in which were blended great quickness of perception, and a retentive memory; although he enjoyed the benefits of an excellent preliminary and professional education, it was only by habits of uncommon industry, punctuality in the performance of all his engagements, the strictest temperance and regularity in his mode of life, that enabled him to accomplish so much in his profession, and to contribute so largely to the medical literature of his country. Dr. Rush, like most men who have extended the boundaries of any department of human knowledge; who have contributed to the improvement of any art or science, was in habits of early rising, by which he always secured what Gibbon has well denominated "*the sacred portion of the day.*"

The great moralist* justly observes, that "to temperance every day is bright, and every hour is propitious to diligence." The extreme temperance of Dr. Rush in like manner enabled him to keep his mind in continual employment, thereby "setting at defiance the morning mist and the evening damp—the blasts of the east, and the clouds of the south."† He knew not that "lethargy of indo-

* Dr. Johnson. † Boswell.

lence" that follows the inordinate gratifications of the table. His ciesto did not consist in indulgence upon the bed or in the armed chair, to recover those powers which had been paralysed or suspended by an excessive meal, or the intemperate use of vinous or spirituous drinks.

Dr. Johnson, during his tour to the Hebrides, when fatigued by his journey, retired to his chamber, and wrote his celebrated Latin ode addressed to Mrs. Thrale.* Dr. Rush, in like manner, after the fatigues of professional duty, refreshed his mind by the perusal of some favourite poet, some work of taste, some volume of travels, biography, or history. These were the pillows on which he sought repose.

But the virtues of the heart, like the faculties of his mind, were also in continued exercise for the benefit of his fellow men; while the numerous humane, charitable, and religious associations, which do honour to the city of Philadelphia, bear testimony to the philanthropy and piety which animated the bosom of their departed benefactor, let it also be remembered that, as with the good Samaritan, the poor were the objects of his peculiar care; and that in the latter and more prosperous years of his life, one-seventh of his income was expended upon the children of affliction and want. Dr. Boerhaave said of the poor, that they were his best patients, because God was their paymaster.

Let it also be recorded that the last act of Dr. Rush was an act of charity, and that the last expression which fell from his lips was an injunction to his son, "Be indulgent to the poor."

"Vale egregium academiæ decus! tuum nomen meeum semper durabit; et laudes et honores tui in æternum manebunt." †

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## Article II.

*Experiments on Phosphureted Hydrogen Gas.* By Thomas Thomson, M.D. F.R.S.

Phosphureted hydrogen gas was discovered in 1783 by M. Gengembre, who published a dissertation on it in the Memoires des Savans Etrangers, vol. x. p. 651. He informs us that it takes fire when it comes in contact with common air; that it is twice as heavy as oxygen gas; that six cubic inches of it require 300 cubic lines of air before they cease to burn; and that the volume of the gas was diminished 100 cubic lines (p. 654). From this statement it is obvious that Gengembre had obtained this gas only in a very impure

* Boswell.

† These words were addressed by Dr. Rush, upon his taking leave of the University of Edinburgh, to his particular friend and preceptor, Dr. Cullen.

state. Indeed, I hardly believe that it is possible to procure pure phosphureted hydrogen gas by heating a mixture of phosphorus and potash dissolved in water, which was the method employed by Gengembre.

In 1786 a few experiments on it were added by Mr. Kirwan by way of appendix to his dissertation on hepatic air. (Phil. Trans. 1786, p. 118.) He does not seem to have been aware that it had been already discovered. He ascertained its spontaneous inflammability, and considered it as phosphorus in an aerial state. He saturated water with it, tried the effects produced by this water on different metalline salts, and ascertained that this water has the property of precipitating various metallic oxides.

In 1791 M. Raymond published a new process, by means of which he succeeded in obtaining phosphureted hydrogen gas in greater abundance, and with more facility. His method was to mix together two ounces of newly slacked quick-lime, a quarter of an ounce of phosphorus in grains, and half an ounce of water. This mixture is put into a small retort, and heated. It yields phosphureted hydrogen gas for a long time, and in considerable quantity. Mr. Raymond substituted white oxide of zinc and black oxide of iron for the lime, and obtained small quantities of phosphureted hydrogen gas in both cases. (Ann. de Chim. vol. x. p. 19.)

In 1799 M. Raymond published another dissertation on this gas. (Ann. de Chim. vol. xxxv. p. 225.) He describes the properties of water impregnated with phosphureted hydrogen. It has a yellow colour, an intensely bitter taste, and a very disagreeable odour, not quite the same with that of the gas. Water, according to Raymond, absorbs not quite the fourth part of its volume of this gas. Heat disengages the gas again without any alteration. The water produces no change on the infusion of litmus. It throws down silver, lead, mercury, and copper, from their acid solutions in the state of phosphurets. From nitrate of mercury it throws down the metal, at first black; but it speedily becomes white and crystallized, being converted into phosphate of mercury.

In the year 1810 Mr. Dalton, in his New System of Chemical Philosophy, vol. ii. p. 457, gives us a series of experiments which he made upon this gas. They are characterized by that simplicity and sagacity which peculiarly distinguish all the labours of Mr. Dalton. The new facts determined by Mr. Dalton are the following:—The specific gravity of the gas is 0·85, that of air being 1. Water absorbs $\frac{1}{27}$th of its bulk of this gas. It is decomposed by electricity, leaving its bulk of pure hydrogen gas. It requires either $1\frac{1}{2}$ volumes or 1 volume of oxygen gas to burn it completely. In the first case water and phosphoric acid; in the second case, water and phosphorous acid, are formed. In most cases it is largely contaminated with hydrogen gas. This gas is a compound of one atom hydrogen and one atom phosphorus united together.

These are all the facts respecting this gaseous body that have been

ascertained by preceding chemists, as far at least as I am acquainted with the subject. Sir Humphry Davy, indeed, in his Elements of Chemical Philosophy, p. 294, relates some experiments to which he had subjected it. But as he appears to have operated upon a very impure gas, his experiments do not furnish us with any very useful conclusions.

Some time ago I subjected this gaseous substance to a set of experiments, with the view of determining its composition with more accuracy than had hitherto been done. I propose in this paper to relate the various results which I obtained, and the conclusions which I consider myself as warranted to draw.

Phosphureted hydrogen gas may always be obtained in a state of perfect purity by the following method:—Take a small tubulated retort capable of holding about 12 cubic inches; fill it up to the tubulated mouth with a mixture of three parts water that has been recently boiled, in order to deprive it of the air which it contains, and one part of common muriatic acid. Drop into this liquid, as rapidly as you can, about half an ounce of phosphuret of lime in lumps. Then put the stopper into the retort, and fill the whole of the beak to the very extremity with water that has been recently boiled. Plunge the beak of the retort into a small water tub filled with water recently boiled. Apply a very gentle heat to the retort. The phosphureted hydrogen gas is rapidly generated, and may be collected in glass jars for examination. Half an ounce of good phosphuret of lime will furnish 70 cubic inches of pure phosphureted hydrogen gas.

1. Phosphureted hydrogen gas thus prepared is colourless, like common air. It has a smell similar to that of onions, and an exceedingly bitter taste. I do not find that it is decomposed by being kept in contact with pure water in close vessels. But, when left standing over water impregnated with common air, it soon loses the property of burning spontaneously when it comes in contact with atmospheric air. This spontaneous combustability, indeed, depends upon the rapidity with which the phosphorus combines with oxygen, and the heat generated in consequence. If this heat rises as high as 148°, spontaneous combustion takes place. But if it does not rise so high, the phosphorus only combines with half a volume of oxygen, and the hydrogen remains unaltered. Accordingly phosphureted hydrogen may be deprived of the whole of its phosphorus by putting it into a narrow glass tube, and letting up half a volume of oxygen gas to it. A white smoke takes place, the half volume of oxygen gradually disappears, and there remains behind a quantity of hydrogen gas amounting exactly to the original volume of the phosphureted hydrogen gas.

2. The gas is likewise decomposed by passing electric sparks through it for some time. The phosphorus is deposited, and a quantity of hydrogen gas remains exactly equal to the original bulk of the phosphureted hydrogen gas.

3. If a dry flask containing some sulphur be exhausted of air, filled with phosphureted hydrogen gas, and then heat be applied sufficient to melt the sulphur, the whole of the phosphorus separates from the gas, and combines with the sulphur; while a portion of the sulphur at the same time unites with the hydrogen of the gas, and converts it into sulphureted hydrogen gas. By this conversion of the phosphureted hydrogen gas into sulphureted hydrogen its bulk is not altered.

4. From the preceding experiments it is abundantly evident that phosphureted hydrogen is a compound of phosphorus and hydrogen, and that when hydrogen gas is converted into phosphureted hydrogen, its bulk is not altered. Therefore, in order to obtain the composition of this gas with accuracy, we have only to subtract the specific gravity of hydrogen gas from that of phosphureted hydrogen gas.

I have taken the specific gravity of phosphureted hydrogen gas four times. The mean of my experiments gives 0·865 for the specific gravity of this gas, that of common air being 1. But the experiment which was made with the greatest care, and which, therefore, I consider as by far the most accurate, makes the specific gravity of this gas 0·903. I am disposed to consider 0·9022 as the true specific gravity, because this is the weight that would result from the gravity of a volume of phosphorus deduced from other experiments, which I have not room to detail here. The difference between this number and that which I obtained by experiment is within the limits of unavoidable error to which we are liable. We have, therefore, the following calculations for the composition of phosphureted hydrogen gas:—

| | |
|---|---|
| Specific gravity of phosphureted hydrogen .. | 0·9022 |
| ——— hydrogen gas ..... | 0·0694 |
| Phosphorus ..... = | 0·8328 |

Therefore phosphureted hydrogen gas is composed of

| | | | |
|---|---|---|---|
| Hydrogen ..... | 694 | or | 1 |
| Phosphorus ..... | 8328 | | 12 |
| | 9022 | | 13 |

So that phosphureted hydrogen gas contains $\frac{1}{13}$th of its weight of hydrogen and $\frac{12}{13}$ths of phosphorus. Supposing it composed of one atom of hydrogen and one atom of phosphorus, then it follows that an atom of phosphorus is 12 times the weight of an atom of hydrogen; but an atom of oxygen is just eight times the weight of an atom of hydrogen. Therefore if we represent the weight of an atom of oxygen by 1, that of phosphorus will be 1·5.

The weight of a volume of phosphorus will be 0·8328, or (if we reckon the specific gravity of oxygen 1), 0·75, which is just half the weight of an atom; so that in this respect phosphorus agrees with

hydrogen, carbon, and sulphur; the number representing its volume is just one half of that representing its atom.

5. When oxygen and phosphureted hydrogen gases are mixed in any proportions whatever in a wide vessel, a vivid combustion takes place, accompanied by a very white light. My experiments, which were 23 in number, agree exactly with those of Mr. Dalton. One volume of phosphureted hydrogen is completely consumed when mixed either with one volume of oxygen gas, or with $1\frac{1}{2}$ volume. In the first case, water and phosphorous acid are formed; in the second case, water and phosphoric acid. In both cases half a volume of oxygen gas goes to the formation of water. So that phosphorous acid is formed by the combination of one volume of phosphorus with half a volume of oxygen, and phosphoric acid by the combination of one volume of phosphorus with one volume of oxygen gas; or, which is the same thing, phosphorous acid is composed of 1 atom phosphorus + 1 atom oxygen, and phosphoric acid of 1 atom phosphorus + 2 atoms oxygen. But we have seen that an atom of phosphorus weighs 1·5, and an atom of oxygen 1. Therefore phosphorous acid is composed of

| | | | |
|---|---|---|---|
| Phosphorus | 1·5 or | 3 or | 100 |
| Oxygen | 1·0 | 2 | 66·6 |

and phosphoric acid of

| | | | |
|---|---|---|---|
| Phosphorus | 1·5 or | 3 or | 100 |
| Oxygen | 2·0 | 4 | 133·3 |

These results are confirmed by numerous other experiments which I have made on phosphorous and phosphoric acids, and which I shall take a future opportunity of laying before my readers.

6. Nitrous gas may be mixed in any proportion whatever with phosphureted hydrogen gas without producing any alteration on it. But if an electric spark be passed through the mixture, an explosion takes place, and the bulk is diminished. I find that when one volume of phosphureted hydrogen is mixed with three volumes of nitrous gas, there remains after the explosion $1\frac{1}{2}$ volume of pure azotic gas. Now one volume of nitrous gas is composed of $\frac{1}{2}$ volume of oxygen gas + $\frac{1}{2}$ volume azote; so that three volumes of this gas contain $1\frac{1}{2}$ volume of oxygen and $1\frac{1}{2}$ volume of azote. All the phosphureted hydrogen gas, and all the oxygen of the nitrous gas, disappear; and the residue consists of the whole azote of the nitrous gas. This combustion, then, is precisely the same as when oxygen gas is employed. Water and phosphoric acid are formed.

If two volumes of nitrous gas be mixed with one volume of phosphureted hydrogen gas, and exploded, a very curious effect is produced. Only one half of the phosphureted hydrogen gas appears to be decomposed, though the whole oxygen of the nitrous gas has disappeared. The residue amounts to $1\frac{1}{2}$ volume. The half volume would seem to be phosphureted hydrogen; for if $1\frac{1}{2}$ time its bulk of oxygen be added, and an electric spark passed through it, an

explosion takes place, white smoke appears, and nothing remains but a quantity of azotic gas equal to half the bulk of the nitrous gas employed. I expected to have been able to decompose the whole of the phosphureted hydrogen by two volumes of nitrous gas into phosphorous acid and water. But the result was as just stated.

If phosphureted hydrogen gas and nitrous gas be mixed in the requisite proportions, on letting up a bubble of oxygen gas an explosion immediately takes place, and the residuum consists of azotic gas, amounting to half the bulk of the nitrous gas employed. This is a very elegant way of exploding these two gases, and may be introduced with great effect into a lecture. Proper proportions are 20 measures of phosphureted hydrogen, 52 measures of nitrous gas, and four measures of oxygen gas. The measure which I employ is $\frac{1}{100}$th of a cubic inch.

7. When an electric spark is passed through a mixture of phosphureted hydrogen and oxide of azote, a loud explosion takes place, accompanied by a vivid light. When one volume of phosphureted hydrogen is mixed with three volumes of oxide of azote there remain after the explosion exactly three volumes of azotic gas. But oxide of azote is composed of 1 volume of azote + $\frac{1}{2}$ volume of oxygen condensed into one volume. Hence three volumes of this gas, if decomposed, would constitute three volumes of azote and $1\frac{1}{2}$ volume of oxygen. We see, therefore, that the combustion in this case is precisely the same as when pure oxygen is employed. The phosphureted hydrogen is converted into water and phosphoric acid, and the azote of the oxide of azote remains behind unaltered.

8. When phosphureted hydrogen gas is let up into chlorine gas it burns vividly with a greenish yellow flame, and a brown matter is deposited, which very speedily dissolves in the water. When we employ three volumes of chlorine and one volume of phosphureted hydrogen, and mix them over water, the whole mixture disappears, being converted into muriatic acid and the brown matter above mentioned. This brown matter is a bichloride of phosphorus. By solution in water it is converted into muriatic acid and phosphoric acid.

It is probable that two volumes of chlorine would likewise disappear when mixed with one volume of phosphureted hydrogen gas, forming muriatic acid and proto-chloride of phosphorus. But I did not try this proportion, and cannot, therefore, state such a result, though sufficiently probable, as a fact decided by experiment.

9. If a quantity of iodine be put into a glass tube, and if the tube be exhausted of air, and then filled with phosphureted hydrogen gas, the phosphorus combines with the iodine, and forms a white solid substance, which is iodide of phosphorus. If the requisite proportion of the two substances be employed, the phosphureted hydrogen gas is completely decomposed, and a quantity of hydrogen gas remains, precisely equal in bulk to the original gas.

But if any water be present, one-third of the hydrogen also disappears. The proportions which I found to answer are four grains of iodine for $1\frac{1}{2}$ cubic inch of gas. If more iodine be used, part of it remains in the state of a brownish yellow substance, which is probably a biniodide of phosphorus.

10. My experiments respecting the absorption of phosphureted hydrogen gas by water agree very nearly with those of Dr. Henry. According to him, 100 measures of water absorb 2·14 measures of phosphureted hydrogen. I tried the experiment in a glass tube divided into 100 parts of a cubic inch. I found 100 measures of water to absorb rather more than two measures of gas. Mr. Dalton states the quantity absorbed by 100 measures of water as 3·7 measures of gas. This considerably exceeds the proportion which I obtained; but it is possible that the water employed by Mr. Dalton was better freed from air than what I used in my experiments.

Water impregnated by phosphureted hydrogen gas has a yellow colour, and an intensely bitter taste. Its smell is similar to that of the gas. It does not alter vegetable blues. When dropped into a solution of iodide of zinc or hydriodate of potash, it produces no alteration in these liquids. It has the property of precipitating various metallic solutions. The following table exhibits the result of the experiments which I made, by dropping water impregnated with this gas into various metalline salts:—

| Saline Solutions. | Colour of Precipitates. |
|---|---|
| Nitro-muriate of gold, | Dark purple, almost black. |
| Nitro-muriate of platinum, | Yellow flocks fall slowly. |
| Pernitrate of mercury, | Copious dark brown flocks. |
| Nitrate of silver, | Black flocks. |
| Sulphate of copper, | Dark brown precipitate. |
| Nitrate of lead, | A slight white powder. |
| Persulphate of iron, | 0. |
| Sulphate of zinc, | 0. |
| Muriate of manganese. | 0. |

Such are the properties of phosphureted hydrogen gas, as far as I have ascertained them. There is another gas, composed of two atoms hydrogen and one atom phosphorus, possessed of properties that differ a good deal from those which I have just detailed. I shall take a future opportunity of laying an account of the history and properties of this second gas before my readers. The gas which I have described in this paper ought to be called *hydroguret of phosphorus*; and the second gas, to be described hereafter, should be called *bi-hydroguret of phosphorus.*

## Article III.

### *Experiments on the Resistance of Air, and on Air as a moving Power.* By Col. Beaufoy, F.R.S.

(To Dr. Thomson.)

MY DEAR SIR, *Bushey Heath, April* 19, 1816.

I have taken the liberty of sending you more experiments, which may be termed a continuation, or, more properly speaking, are connected with those you have been pleased to publish; and should these induce other persons to bestow their attention on this branch of philosophical investigation, no doubt far better experiments and deductions would result. Scientific men, in parts of Europe far less maritime than our own, have been, and are now, busily employed in researches of this nature. It is to be regretted that more attention has not been bestowed in Great Britain on this subject; but it may not be too much to expect that an emulation will arise among those who have an opportunity of benefiting the community by multiplying experiments, and thus do away the stigma so frequently applied, that the first maritime nation which ever existed is chiefly indebted, to strangers for the elementary works of that science which has rendered her no less the admiration, than the dread, wonder, and envy, of the rest of the universe.

I remain, my dear Sir, yours very sincerely,

Mark Beaufoy.

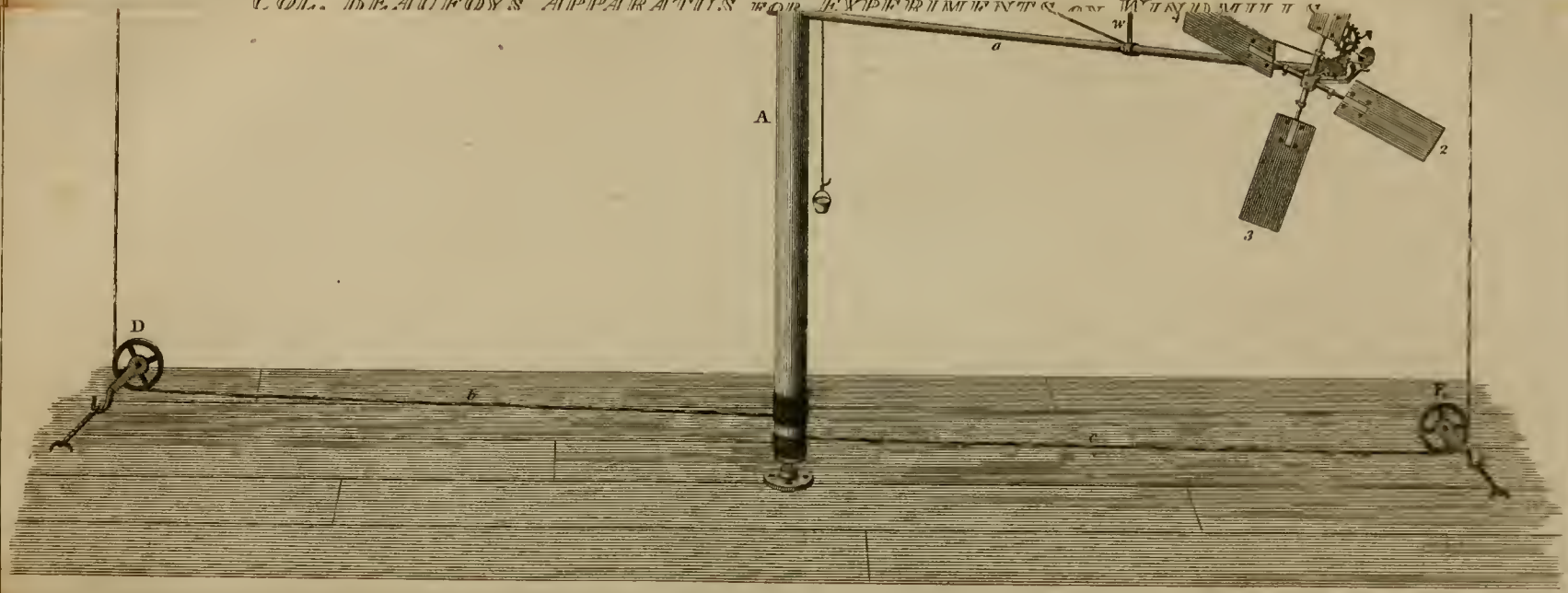
### *Description of the Apparatus.*

In making these experiments, the first object is to ascertain exactly the velocity with which the air strikes the sails. This is most readily accomplished by causing the windmill to move through the air with a certain velocity, which can be much more accurately determined than the motion of the air. The windmill has four sails, numbered 1, 2, 3, 4, (Plate LIII. Figs. 1, 2) mounted on an axis supported in a frame fixed at the end of a brass tube, *a*, which is fixed perpendicularly into a vertical axis, A, revolving freely on pivots at its ends. The lower pivot rests in a step supported on the floor, and the upper is fitted into a brass socket fixed to the beam, B, B, extending horizontally across the room at about five feet from the ground. When this vertical axis is turned round, the sails will pass through the air with any required velocity, the centre then describing a circle of 106·92 inches in diameter. This motion is caused by two small cords, *b*, *c*, lapped round the lower end of the axis in contrary directions. The cords are conducted round pulleys hooked to the floor at D and E; then passing over two other pulleys, F and G, have leaden weights appended to them, which by their descent

Plate LIII.



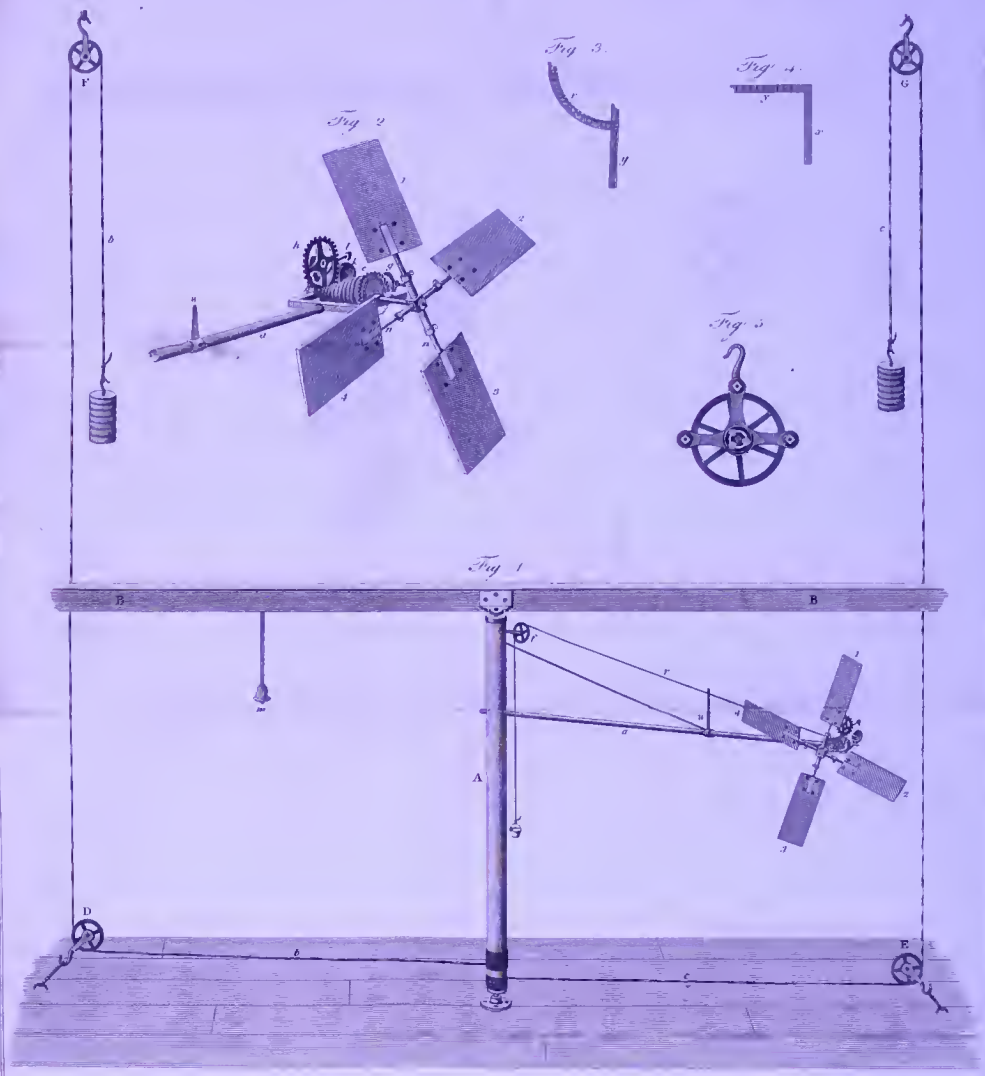
# COL.L BEAUFOY's APPARATUS FOR EXPERIMENTS ON WINDMILLS




J. Farey Junr. del.

J. Skury sculp.

# COLL. BEAUFOY'S APPARATUS FOR EXPERIMENTS ON WINDMILLS.




J. Farey Junr. del. J. Basire sculp.

Engraved for the Thomsons Annals by Baldwin, Cradock & Joy, Paternoster Row, August 1, 1818.

actuate the machine. As the cords are put round the axis in contrary directions, they mutually operate to turn the axis round, without causing any extraordinary strain upon the point at the lower end. The axis of the sails has a fusee, *e*, Fig. 2, fixed upon it, similar to the fusee of a clock, except that it is a conical figure. Upon this a fine line, *r*, Fig. 1, is wound, which then passes round a pulley fixed to the vertical axis, A, at *f*, and has a small weight suspended from it. On the largest end of the fusee a ratchet wheel is fixed, and a click, *g*, Fig. 2, engages its teeth. These are 100 in number, and are figured on the face of the wheel. On the smaller end of the fusee is a pinion of eight leaves, turning a cog wheel, *h*, of 80 teeth. This has a pin fixed in it, which raises a small hammer, *l*, and causes it to strike on a bell, *k*, at every 10 revolutions of the sails. The principal axis, A, Fig. 1, is also provided with a bell, to point out every turn it makes. This bell is fixed by a spring from the beam, B, and *w* is a small spring fixed to the arm, *a*, carrying the vanes, which at every revolution meets the bell, *m*, and strikes it. The vanes of the sails are fixed to their axis by a steel wire, *n*, Fig. 2, fastened to each of them, which are inserted into four tubes disposed in the form of a cross, and screwed upon the axis. The tubes are split at their ends, and each surrounded by a screw clamp, *o*, which, when screwed fast, closes the tube round the wire, and by that means fixes the same at any required angle to the plane of motion. These angles are determined by the small protractor, Fig. 3, the clamps being loosened. All the four vanes are first set by the eye exactly in the plane of their motion. By turning them round while the eye is held in their plane, the smallest deviation from the truth in this respect will be rendered apparent. The straight ruler, *g*, of the protractor is now applied against the edge of one of the sails; No. 1, for instance; the ruler in the direction of the sail's length; the plane of the arch, *r*, perpendicular to the sails; and its centre coincident with the centre of the wire of the adjacent sail, No. 2; consequently when the latter is turned about in the socket of the wire, *n*, it will follow the arch, *r*, and the degrees thereon will point out the angle which it is required to make with the first sail. This operation is repeated to the other sails; and by this means three of them can be set to have the same angle. For the fourth a different method must be resorted to, as there is nothing left in the plane of motion to apply the ruler, *g*, of the protractor against. The square, Fig. 4, is now used. The ruler, *x*, is placed against the edge of one of the sails; No. 2, for instance; which is set to its proper angle; so that the other ruler, *y*, is perpendicular to the plane of the sail; and projecting from its front, this applies itself to the edge of the adjacent sail, No. 1, which is one of those previously set to its intended angle. It is now noticed what division upon the ruler, *y*, of the square is cut by the edge of the sail 1, the square is then removed to the sail No. 1, next to that which remains to be set; and this No. 4 is turned round until the same division on the ruler, *y*,

of the square is cut by its edge. The divisions on this square are merely arbitrary, as they are only intended to put all the sails in the same position relative to each other, three of them having first been set at the intended angle with the plane of motion. The sails being set by these means, the clamps, *o*, are screwed tight, to retain them in their positions. The weights being prepared and wound up by turning the axis, A, in a contrary direction, a weight is suspended at the end of the line, *r*, which is all unwound off the fusee, and the machine is permitted to revolve by the action of the weights. After making a turn or two, it acquires its proper velocity. This is regulated by the quantity of descending weights, and determined by the spring, *w*, striking the bell, *m*, the operator having a stop watch to ascertain the period of several revolutions after the motion becomes uniform. This motion of the sails through the air has the same effect in turning them round as a current of air would have by blowing against them. As they turn round, the fusee on their axis winds up the line, *r*, and weight; the resistance of which represents the work the windmill has to perform; as by turning round, the fusee acts upon a larger radius, this resistance constantly increases, and will cause the sail to stop when it becomes equal to their power. When the operator perceives the sails have lost their motion, the machine may be stopped. The click, *g*, now prevents the weight turning the sails backwards; and by observing the division at which the click stands, and the grooves of the fusee on which the line acts, the resistance of the weight, and consequently the power exerted by the sails, may be determined by a table previously made for the purpose. The fusee has 14 turns of the spiral groove upon it; and, as before mentioned, the 100th part of a revolution can be determined by the teeth of the ratchet wheel. The resistance of winding a weight on this fusee is constantly increasing, as the thread winds from the smallest to the largest end; and by the above divisions the intensity can be determined at 1400 different points. The table is made by experiments; a circle of 18·224 inches diameter is fitted upon the axis of the sails, and pinned to them; a fine thread is then wound upon this circle, and a small tin cup suspended from it; the line, *r*, and weight are applied to the fusee; a small weight is now put into this cup sufficient to turn the fusee one division; the weights used for this purpose are very fine shot, and made up to the proper weight by a small quantity of filings, or some other fine metallic masses. This being done, the contents of the cup are weighed, and recorded in the table. The second division is now ascertained in the same manner; and by these means the whole number are gone through. It is not essential to try every individual division, unless it should appear from the trials made that the accuracy of the spiral on the fusee is to be suspected in any particular part by these trials; showing that the increments of the resistance are unequal in equal spaces. In such cases every division should be proved by experiments. Where no idea of this kind is entertained, the blanks in the table may be

filled up by making the resistance increase in the regular progression from one ascertained point to the next. A table so found will show accurate results, notwithstanding any defects in the regularity of the spiral groove of the fusee, as it will exhibit the value of the resistance at every division of the fusee. When the velocity of sails is the only object of inquiry, the line, *r*, is removed from the fusee, and the machine set in motion as before. The small bell, *k*, now strikes at every 10 turns of the sails, and may be counted by means of a stop-watch to determine the number of revolutions they make for moving through the air a certain space. Fig. 5 is a view of one of the pulleys used in place of F and G, when extraordinary accuracy is desired. The pivot of the pulley is surrounded by six friction rollers, which roll round in a circular aperture made in the frame which contains the pulley. Only three of the friction rollers are shown in the drawing, to avoid confusion.

N. B. The sails are made of thin mahogany bevelled to the edges in parallelograms, each 12 inches long and six wide; consequently the superficial contents are two square feet. The distance from the axis of the fusee to the centre of the sails is 9·112 inches, or equal to half the diameter of the circle (18·224 diameter) fitted upon the axis of the sails.

*Experiments made with the View of ascertaining the Resistance which Air offers to Plane Surfaces moving with different Velocity, when they are placed at right Angles, and obliquely to the Impulse. Also to ascertain the Power exerted by Wind on the Sails of Windmills and Ships, to move those Bodies from a State of Rest to Motion.**

(See Table A.)—The table is divided into several horizontal and vertical lines. 1. Horizontal line, contains the position of the sails. 2. The horizontal velocity with which the sails struck the air. 3. The motive weights reduced into ounces. 4. The friction of the apparatus, which subtracted leaves, in the 5th, the experimented or true motive weights. 6. The calculated motive weights. 7. The resistance of the arm, which subtracted from the calculated motive weights leaves, in the 8th, the relative or sail's resistance, which numbers, being multiplied by 1·435, the radius of the axis round which the cord was wound, and divided by 53·46, the length of the arm, leaves, in the 9th and last line, the true resistance. The mean exponent is set down for the purpose of calculating the resistance answering to any other velocity, or the velocity answering to any other resistance.

The other tables have fewer horizontal lines, it being unnecessary to enter so much into the detail. It is, however, to be remarked, that an additional line is introduced, which contains the resistance, calculated from the commonly received opinion that the resistance

* In these experiments the sails were prevented from moving in a vertical direction, or round the horizontal axis.

decreases in proportion to the squares of the sines of the angle of incidence.

If it were possible to construct a machine perfect in all its parts; if the weight of the atmosphere did not vary; and if experiments could be made without committing errors; the sixth line would be unnecessary. But as all these causes render perfection impossible, it is requisite, for comparing the experiments more satisfactorily with each other, to get rid of the unavoidable irregularities, by bringing the whole into a regular series; which is done by the theorem explained in the *Annals of Philosophy*, vol. vi. p. 279. By examining the experiments, and comparing them with the resistances, calculated according to the squares of the sines of the angle of incidence, it is evident that the theory does not coincide with facts, except at the angle of 55°, the variation in this case not being more than $\frac{4}{1000}$ of an ounce. But in the angles between 85° and 55° the computed exceeds the experimented resistances, except at 65°; and afterwards the experimented resistances become greater than the calculated.

(See Table B.)—The subsequent table contains the experiments made when the sails were permitted to revolve round their shaft or horizontal axis. This table, like the former, is divided into vertical and horizontal lines. 1. The angle of the sails, reckoning from the plane of their motion. 2. The horizontal velocity in feet per second. 3. The weights in ounces hung to the cone, and which were wound up by the rotatory motion of the sails. 4. The revolutions and parts of the revolution of the cone when the sails came to a state of rest. 5. The value of the revolutions and parts, in ounces and decimal parts, which balance or are equal to the efforts of the sails. 6. The calculated values or the regular series of the sail's efforts. 7. Those values divided by 9·112, the distance of the centre of gravity, or rather the centre of pressure of the sails from their axis; and the result is the impelling power as applied to the sail of a vessel. By examining these experiments, it appears that the most advantageous angle to move the sails of a mill from a state of rest to that of motion is the angle of 60°, which widely differs from the generally received opinion on this subject. Those who have made experiments on the sails of windmills may have been led into considerable errors by permitting them to acquire great velocity, and consequently great momentum; for it was observed in making these experiments, that with a small degree of celerity the apparent effect was far greater than should be attributed merely to the impulse of the wind. If the direct impulse of the wind be supposed to be 10·000, the annexed table will show how much its effect is decreased by altering the obliquity of the sail every 5°, and the effort of a sail to drive a ship to leeward, and its power to give the vessel a progressive velocity:—

*Angle of the Sail with the Direction of the Wind.*

| | 5 | 10 | 15 | 20 | 25 | 30 | 35 | 40 |
|---|---|---|---|---|---|---|---|---|
| Total effect ..... | 217 | 438 | 940 | 1561 | 2274 | 3475 | 4118 | 4832 |
| Leeward effect .... | 83 | 202 | 533 | 1028 | 1630 | 2722 | 3408 | 4124 |
| Progressive effect .. | 134 | 236 | 407 | 533 | 644 | 753 | 710 | 708 |

*Angle of the Sail continued.*

| | 45 | 50 | 55 | 60 | 65 | 70 | 75 | 80 | 85 | 90 |
|---|---|---|---|---|---|---|---|---|---|---|
| Total effect ..... | 5377 | 6069 | 6708 | 7410 | 8244 | 8615 | 8934 | 9279 | 9551 | 10000 |
| Leeward effect ..... | 4711 | 5508 | 6160 | 6914 | 7789 | 8273 | 8662 | 9095 | 9453 | 10000 |
| Progressive effect ..... | 666 | 561 | 548 | 496 | 455 | 342 | 272 | 184 | 98 | 10000 |

To apply this table to practice, suppose a windmill with four sails, each sail 35 feet long and six feet wide, and that 30 feet, counting from the extremity of each sail, is covered with canvas; the area of the four sails will be 720 square feet, the surface exposed to the action of the wind. Let the velocity of the wind be 20 feet per second, and the angle which the sails make with the plane of their motion 35°. The power of the wind, when blowing 20 feet in a second on one superficial foot exposed at right angles to the current, is 0·9066 parts of a lb. avoirdupoise, or 14·505 oz. By inspecting the column under 55° (the complement of 35) the numbers 6708, and 6160, and 548, are found. The first, 6708, shows how much the direct force of the wind is reduced from 10·000, by placing the sails at an angle of 55°. The second number is the proportional part to overturn the mill. And the third number the force exerted to give the sails a rotatory motion, or a ship progressive velocity, as 10000 : 6708 :: 722 × 0·9066 = 652·75 : 437·87 lbs. the total effect of the wind on the sails. Again, 10000 : 6160 :: 652·75 : 402·09, the power to overturn the mill, or leeward effect. Again, 10000 : 548 :: 652·75 : 35·771 lbs. the power of the sails' rotatory motion. 35·771 multiplied by 20, the distance of the centre of gravity of the sails, or rather of the pressure from the centre of the axis, gives 715·42 lbs. the power of the mill. Had the power of the mill been calculated in the usual mode, it would have given 2503 lbs.; but 715·42 is to 2503 nearly as 2 to 7, a very considerable difference, which proves that no reliance can be placed on the usual theory. Mathematicians demonstrate the most advantageous angle for the sails to make with the plane of their motion to be nearly 35°; but these experiments indicate that the angle should be 60°. Mills would be better constructed, supposing 60° to be the best angle, if the sails were less rapid in their motion, which would permit their being placed more obliquely to the impulse of the wind; but then a disadvantage

would arise from the increase of friction caused by the head of the shaft projecting more than in the usual method of building. This inconvenience might be remedied by using Mr. Garnet's ingenious invention of friction rollers for mills; which, if not sold in London, can be readily procured from Bristol. Another improvement the writer thinks would accrue if the sails were made narrower at the extremity than at the axis. This he is aware is contrary to what has been recommended; but as a narrow sail will admit in practice of being more weathered than a broad one, the advantage gained by the sails making an acute angle at their extremities with the direction of the wind, will more than counterbalance the disadvantage of having the centre of gravity nearer the axis.

Meteorological journals would be much improved if a simple and cheap instrument for registering the velocity of the wind was used. From numerous experiments made with this (see the Plate), it appeared well adapted for the purpose of a wind-measurer or gauge. The following table contains the velocity of the extremity of the sails at the various angles of 5°, 10°, 15°, &c.; therefore the constructor may choose any angle he thinks proper. The greatest velocity the sails can acquire is when the angle is 15°, the velocity in this case being more than five times that of the wind. As no advantage accrues from this increase of velocity, some other angle might be more advantageous; perhaps the angle of 60°, as it is the best from which to move the sail from a state of rest, and consequently the best adapted for light winds. But as wheels of 1000 and 1642 teeth are too large for practice, the following smaller numbers will be more convenient, and they bear nearly the same proportion as the larger:—

As 11 is to 18; or 14 is to 23; or 81 is to 133; or 500 is to 821, which is exactly in the same proportion as 1000 is to 1642.

Suppose the velocity of the wind 1000:—

*Inclination of the Sail from the Plane of Motion.*

| | 5° | 10° | 15° | 20° | 25° | 30° | 35° | 40° | 45° |
|---|---|---|---|---|---|---|---|---|---|
| Velocity of sails ..... | 3972 | 4940 | 5132 | 4838 | 4554 | 4007 | 3526 | 3078 | 2744 |

*Inclination of the Sail continued.*

| | 50° | 55° | 60° | 65° | 70° | 75° | 80° | 85° | 90° |
|---|---|---|---|---|---|---|---|---|---|
| Velocity of sails..... | 2325 | 1984 | 1642 | 1234 | 685 | 52 | 32 | 12 | .. |

When this instrument is used as a wind-gauge it must be fixed on a spindle, like a weather-cock, and be furnished with a long wedge-like vane, which will render it steady, by preventing the ill effects of the vibration caused by the eddy wind from the sails.

To apply these experiments to a ship, suppose an 80-gun man of war on two decks to have in the

| | Square of Feet of Canvas. |
|---|---|
| Spanker | 3147·75 |
| Main-sail | 4707· |
| Fore-sail | 3493· |
| Mizen-top-sail | 2106· |
| Main-top-sail | 4693·5 |
| Fore-top-sail | 3933· |
| Fore-top-stay-sail | 1154·25 |
| Mizen-top-gallant-sail | 863·50 |
| Main-top-gallant-sail | 1552·83 |
| Fore-top-gallant-sail | 1339·50 |
| Jib | 2035· |
| Mizen-royal | 407· |
| Main-royal | 1021· |
| Fore-royal | 552· |
| Total | 31005·33 |

Suppose that this quantity of canvas is set, that the wind is on the beam, or at right angles to the ship's course, and blowing with a velocity of 20 feet per second; that the mean height of the centre of gravity, or pressure of all these sails, is 74 feet above the load water line; that the sails are braced up to make with the direction of the wind an angle of 30° (which is found by experiment to be the most advantageous angle to give the first impulse to the ship). Then 31005 × 0·9066 is equal to 28109 lbs. avoirdupoise, or 12·549 tons, the force of the wind blowing at right angles on 31005·33 feet of canvas. Then as 10000 : 3475 :: 28109 : 9768 lbs. or 4·3607 tons, the power of the wind on the sails when braced up to an angle of 30°. Then as 10000 : 2722 :: 28109 : 7651·4 lbs. or 3·4158 tons, the leeward effect. 3·4158 multiplied by 74 gives 252·77 tons, the power of the wind to overturn the vessel. Lastly, as 10000 : 753 :: 28109 : 2116·7, or 0·94493 tons, the power of the wind to give the ship progressive velocity. By several experiments made with different vertical sections of his Majesty's ship Cambridge, of 80 guns, the mean height of the metacentre was found to be 14·76 inches above the load water line. If the centre of gravity of this ship, when ready for sea, be at the surface of the water, the angle of inclination the vessel will assume by the force of the wind is thus found. Divide the momentum of the sails 252·77 by 3438, the tons of sea water displaced by the ship: the quotient, 0·88226 inches is the length of lever on which the water acts to counterbalance the force of the wind. Then to find the angle of inclination, as 14·76 : radius :: 0·88220 : sine of 3° 25′ 36″, the ship's inclination by the power of the wind. The power of the wind on the masts, yards, and rigging, is not taken into the account, because this force is included in the sails; nor is

the effect of the wind on the hull of the vessel included in the calculation. To find the distance between the centre of gravity and the metacentre of his Majesty's ship Cambridge, let the weight of each gun, gun-carriage, and gun-tackles, be ascertained. Then run the guns out on one side, and in on the other, and measure the distance they are moved. By multiplying the space each gun has been moved into the weight of each gun, carriage, tackles, &c. the momentum of each gun is obtained. The sum of all these momenta divided by the weight of the guns will be the mean distance of those guns from the place in which they stood. To solve this problem there is given the weight of the water displaced by the ship, the weights which made the ship incline, the angle of inclination measured by hanging up a plummet, and the height of the metacentre 14·76 inches above the load water line, whence the distance between the metacentre and the centre of gravity of the vessel is thus calculated. From the log. cosine of the ship's inclination subtract the log. sine, and to the remainder add the logarithm of the mean space the weight is moved, and the logarithm of the weight itself. From the sum of these three logarithmic numbers subtract the logarithm of the weight of the water displaced by the ship; and the natural number answering to this last difference will be the distance of the metacentre from the centre of gravity of the vessel.

*Example.*—Suppose the weight of the guns, carriages, &c. of the Cambridge to be 178 tons, and that this weight was removed 17·04 inches from the centre line of the ship, which gave the ship an inclination of 3° 25′ 36″, required the distance of the centre of gravity from the metacentre:—

| | |
|---|---|
| Cosine 3° 25′ 36″ ..... | 9·9992228 |
| Sine 3 25 36 ..... | 8·7765102 |
| | 1·2227126 |
| 17·04 log. ..... | 1·2314696 |
| 178· log. ..... | 2·2504200 |
| | 4·7046022 |
| 3438· log. ..... | 3·5363059 |
| Natural number 14·733 = ..... | 1·1682963 |

Centre of gravity 14·733 inches below the metacentre.

To put a ship in motion, 30° is the proper angle for the sails to make with the wind, if the wind is on the beam; but after the ship has acquired a progressive velocity, the angle should be more than 30°, because the velocity of the wind and ship make the apparent wind different from the true current. Experiments either by log. or the comparative sailing of another ship in company, must determine the most advantageous angle for the sails, and which, when ascertained, should be noted in the log-book, with such remarks as

may be deemed necessary. By the means of such experiments, with no expense and but little trouble, a curious and valuable set of observations would be made, which, when collected, might lay the foundation of great improvements in nautical science. The writer believes it has never been ascertained what proportion the velocity of a vessel sailing with the wind on its beam bears to the velocity of the wind; but from an experiment, not pretending to great accuracy, a fast sailing cutter under favourable circumstances of still and smooth water, with the wind on its beam, was found to have about two-fifths of the velocity of the wind.

Whilst on the subject of experiments, the writer is not acquainted with, or aware that any have been made to measure the velocity of waves, although it might be determined by the following mode:—If two ships be in company, and the height of the truck of each ship above the surface of the water be known, when one of the ships is on the top of a remarkable wave, let the altitude of its truck be taken with a Hadley's sextant by an observer on board the other ship, noting at the same time the minute and second. Then, when the vessel from which the observation was made is lifted by the same wave, let the minute and second also of that be written down. The distance being found, and the time given, the celerity of the wave's motion can be determined. It is by experiment alone that any rational hope can be entertained of forming a true theory for the improvement of naval mechanics; for although enough has been done by experiment to throw a bright twilight on the subject, yet still much remains to be done before the full day sun may be said to illuminate nautical science. The usual rules given for calculating the resistance of fluids are generally known to be so inadequate for the end in view, that no reliance can be placed on their results. It is a solecism in language to designate that to be a theory which does not coincide with practice; and as peace has taken place, it is much to be wished that some of the public boards may find leisure to attend to this branch of nautical knowledge; and, by directing numerous experiments, dispel the obscurity which hangs over the subject.

N.B. By subsequent experiments, the direct resistance was found to be somewhat less. (See *Annals of Philosophy*, vol. vi. p. 277.)

## TABLE A.

*Position of the Sails with respect to the Impulse of the Air, Right Angles.*

| Velocity in feet per second | 4 | 8 | 10 | 12 | 16 | 20 |
|---|---|---|---|---|---|---|
| Motive weights in oz. avoird. | 47·00 | 180·00 | 283·00 | 418·00 | 741·00 | .. |
| Friction subtracted | 3·234 | 5·87 | 6·92 | 16·98 | 26·22 | .. |
| Remains the true motive weights | 43·766 | 174·13 | 276·08 | 401·02 | 714·78 | .. |
| Calculated motive weights | 430·30 | 175·38 | 275·69 | 398·95 | 714·78 | 1123·6 |
| Resistance of the arms subtracted | 2·575 | 8·64 | 12·75 | 17·53 | 28·97 | 42·8 |
| Remains the sails' resistance | 40·455 | 166·74 | 262·94 | 381·42 | 685·81 | 1080·8 |
| Which, multiplied by 1·435, and divided by 53·46, gives the true resistance | 1·0860 | 4·4757 | 7·058 | 10·238 | 18·409 | 29·011 |

Mean Exponent, 2·0410.

### *Angle* 85°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 1·0678 | 4·3458 | 6·8252 | 9·8681 | 17·652 | 27·709 |
| Resistance as the square of ang. of inc. | 1·0777 | 4·4417 | 7·0043 | 10·160 | 18·269 | 28·791 |

Mean Exponent, 2·0229.

### *Angle,* 80°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 1·0782 | 4·3152 | 6·7412 | 9·7052 | 17·421 | 26·920 |
| Resistance as the square of ang. of inc. | 1·0533 | 4·3408 | 6·8451 | 9·9293 | 17·854 | 28·136 |

Mean Exponent, 1·9990.

### *Angle,* 75°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 1·0610 | 4·2060 | 6·5500 | 9·4075 | 16·650 | 25·919 |
| Resistance as the square of ang. of inc. | 1·0132 | 4·1759 | 6·5852 | 9·5523 | 17·176 | 27·068 |

Mean Exponent, 1·9855.

### *Angle,* 70°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 1·0128 | 4·0323 | 6·3048 | 9·0424 | 16·031 | 24·993 |
| Resistance as the square of ang. of inc. | 0·9589 | 3·9521 | 6·2323 | 9·0403 | 16·256 | 25·617 |

Mean Exponent, 1·9912.

### *Angle,* 65°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·9280 | 3·7665 | 5·9099 | 8·5380 | 15·254 | 23·918 |
| Resistance as the square of ang. of inc. | 0 8920 | 3 6763 | 5·7973 | 8·4094 | 15·121 | 23·830 |

Mean Exponent, 2·0187.

### *Angle,* 60°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·8664 | 3·4587 | 5·3983 | 7·7656 | 13·780 | 21·496 |
| Resistance as the square of ang. of inc. | 0·8145 | 3·3567 | 5·2934 | 7·6784 | 13·806 | 21·758 |

Mean Exponent, 1·9949.

### *Angle,* 55°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·7315 | 3·0112 | 4·7458 | 6·8805 | 12·359 | 19·162 |
| Resistance as the square of ang. of inc. | 0·7287 | 3·0032 | 4·7359 | 6·8650 | 12·343 | 19·466 |

Mean Exponent, 2·0384.

## TABLE A, *continued.*

### *Angle,* 50 .

| Velocity in feet per second | 4 | 8 | 10 | 12 | 16 | 20 |
|---|---|---|---|---|---|---|
| True resistance | 0·6635 | 2·7285 | 4·2988 | 6·2460 | 11·187 | 17·607 |
| Resistance as the square of ang. of inc. | 0·6373 | 2·6264 | 4·1417 | 6·0078 | 10·803 | 17·024 |

Mean Exponent, 2·0380.

### *Angle,* 45°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·6130 | 2·4754 | 3·8774 | 5·6072 | 9·9680 | 15·600 |
| Resistance as the square of ang. of inc. | 0·5430 | 2·2397 | 3·5290 | 5·1190 | 9·2046 | 14·506 |

Mean Exponent, 2·0131.

### *Angle,* 40°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·5212 | 2·1574 | 3·4052 | 4·9420 | 8·8918 | 14·017 |
| Resistance as the square of ang. of inc. | 0·4487 | 1·8492 | 2·9162 | 4·2301 | 7·6062 | 11·987 |

Mean Exponent, 2·0448.

### *Angle,* 35°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·4382 | 1·8251 | 2·8858 | 4·1947 | 7·5629 | 11·947 |
| Resistance as the square of ang. of inc. | 0·3573 | 1·4725 | 2·3220 | 3·3683 | 6·0564 | 9·544 |

Mean Exponent, 2·0387.

### *Angle,* 30°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·332 | 1·4977 | 2·3852 | 3·4868 | 6·3423 | 10·081 |
| Resistance as the square of ang. of inc. | 0·271 | 1·1189 | 1·7645 | 2·5595 | 4·6022 | 7·253 |

Mean Exponent, 2·1052.

### *Angle,* 25°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·279 | 1·0917 | 1·6930 | 2·4223 | 4·2604 | 6·5987 |
| Resistance as the square of ang. of inc. | 0·1939 | 0·7994 | 1·2606 | 1·8286 | 3·2880 | 5·1816 |

Mean Exponent, 1·9660.

### *Angle,* 20°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·159 | 0·679 | 1·080 | 1·576 | 2·857 | 4·5275 |
| Resistance as the square of ang. of inc. | 0·127 | 0·524 | 0·826 | 1·198 | 2·153 | 3·3937 |

Mean Exponent, 2·0786.

### *Angle,* 15°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·1235 | 0·4304 | 0·6753 | 0·9749 | 1·7446 | 2·7277 |
| Resistance as the square of ang. of inc. | 0·0727 | 0·2998 | 0·4728 | 0·6858 | 1·2332 | 1·9434 |

Mean Exponent, 1·9642.

### *Angle,* 10°.

| | | | | | | |
|---|---|---|---|---|---|---|
| True resistance | 0·0678 | 0·2397 | 0·3598 | 0·5014 | 0·8462 | 1·2720 |
| Resistance as the square of ang. of inc. | 0·0327 | 0·1349 | 0·2128 | 0·3087 | 0·5552 | 0·8748 |

Mean Exponent, 1·8210.

## TABLE A, *continued.*

### *Angle,* 5°.

| Velocity in feet per second | 4 | 8 | 10 | 12 | 16 | 20 |
|---|---|---|---|---|---|---|
| True resistance | 0·0353 | 0·1223 | 0·1824 | 0·2529 | 0·423 | 0·631 |
| Resistance as the square of ang. of inc. | 0·0825 | 0·0340 | 0·0536 | 0·0778 | 0·139 | 0·226 |

Mean Exponent, 1·7976.

### *Resistance of the Arm.*

| Velocity in feet per second | 4 | 8 | 10 | 12 | 16 | 20 |
|---|---|---|---|---|---|---|
| Calculated motive weights | 2·5748 | 8·6370 | 12·752<br>17·533 | 17·533<br>28·975 | 28·975<br>42·781 | 42·781 |

Mean Exponent, 1·7462.

## TABLE B.

### *Position of the Sails with respect to the Plane of their Motion.*

### *Angle* 5°.

| Horizontal velocity in ft. per second | 4 | 8 | 10 | 12 | 16 | 20 |
|---|---|---|---|---|---|---|
| Weights hung to the cone, in ounces | .. | 4 | 8 | 8 | 16 | .. |
| Revolutions and parts | .. | 7·08 | 6·07 | 12·03 | 11·60 | .. |
| Value of revolutions & parts, in oz. | .. | 0·4581 | 0·6553 | 0·9744 | 1·6935 | .. |
| Calculated values, in oz. | 0·1167 | 0·4447 | 0·6839 | 0·9722 | 1·6935 | 2·6046 |
| Calculated values, divided by 9·112 | 0·0128 | 0·0448 | 0·0750 | 0·1067 | 0·1858 | 0·2858 |

Mean Exponent, 1·9115.

### *Angle,* 10°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values, in oz. | 0·2428 | 0·8823 | 1·3365 | 1·8766 | 3·2058 | 4·8565 |
| Calculated values, divided by 9·112 | 0·0266 | 0·0968 | 0·1467 | 0·2059 | 0·3518 | 0·5330 |

Mean Exponent, 1·8614.

### *Angle,* 15°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz. | 0·3043 | 1·1902 | 1·8407 | 2·6335 | 4·6342 | 7·1838 |
| Calculated values, divided by 9·112 | 0·0334 | 0·1306 | 0·2020 | 0·2898 | 0·5086 | 0·7884 |

Mean Exponent, 1·9645.

### *Angle,* 20°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz. | 0·4354 | 1·6070 | 2·4481 | 3·4531 | 5·9416 | 9 0516 |
| Calculated values divided by 9·112 | 0·0478 | 0·1764 | 0·2687 | 0·3790 | 0·6521 | 0·9934 |

Mean Exponent, 1·8865.

## TABLE B, *continued.*

### *Angle*, 25°.

| Horizontal velocity in ft. per second | 4 | 8 | 10 | 12 | 16 | 20 |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·4575 | 1·8689 | 2·9402 | 4·2564 | 7·6359 | 12·013 |
| Calculated values divided by 9·112 | 0·0502 | 0·2051 | 0·3227 | 0·4671 | 0·8380 | 1·3204 |

Mean Exponent, 2·0306.

### *Angle*, 30°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·5865 | 2·2351 | 3·4382 | 4·8883 | 8·5170 | 13·102 |
| Calculated values divided by 9·112 | 0·0639 | 0·2453 | 0·3773 | 0·5365 | 0·9347 | 1·4379 |

Mean Exponent, 1·9300.

### *Angle*, 35°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·6172 | 2·3999 | 3·7165 | 5·3108 | 9·3310 | 14·480 |
| Calculated values divided by 9·112 | 0·0677 | 0·2634 | 0·4079 | 0·5828 | 1·0240 | 1·5891 |

Mean Exponent, 1·9591.

### *Angle*, 40°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·7975 | 2·8086 | 4·2122 | 5·8655 | 9·8906 | 14·833 |
| Calculated values divided by 9·112 | 0·0875 | 0·3082 | 0·4623 | 0·6437 | 1·0854 | 1·6278 |

Mean Exponent, 1·8162.

### *Angle*, 45°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·7055 | 2·8196 | 4·4047 | 6·3414 | 11·2697 | 17·605 |
| Calculated values divided by 9·112 | 0·0774 | 0·3094 | 0·4834 | 0·6959 | 1·2368 | 1·9321 |

Mean Exponent, 1·9989.

### *Angle*, 50°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 8·8104 | 3·1323 | 4·8405 | 6·9075 | 12·61 | 18·708 |
| Calculated values divided by 9·112 | 0·0889 | 0·3437 | 0·5312 | 0·7581 | 1·3286 | 2·0531 |

Mean Exponent, 1·9504.

### *Angle*, 55°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·8179 | 3·1513 | 4·8650 | 6·9731 | 12·143 | 18·765 |
| Calculated values divided by 9·112 | 0·0898 | 0·3459 | 0·5339 | 0·7653 | 1·3326 | 2·0594 |

Mean Exponent, 1·9461.

### *Angle*, 60°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·8190 | 3·2374 | 5·0388 | 7·2330 | 12·795 | 19·914 |
| Calculated values divided by 9·112 | 0·0899 | 0·3553 | 0·5530 | 0·7938 | 1·4042 | 2·1858 |

Mean Exponent, 1·9826.

### *Angle*, 65°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... ... | 0·9304 | 3·2537 | 4·8686 | 6·7672 | 11·3779 | 17·025 |
| Calculated values divided by 9·112 | 0·1021 | 0·3571 | 0·5343 | 0·7427 | 1·2487 | 1·8685 |

Mean Exponent, 1·8061.

TABLE B, *continued.*

*Angle*, 70°.

| Horizontal velocity in ft. per second | 4 | 8 | 10 | 12 | 16 | 20 |
|---|---|---|---|---|---|---|
| Calculated values in oz ... ..... | 0·7636 | 2·6729 | 4·0009 | 5·5627 | 9·3568 | 14·006 |
| Calculated values divided by 9·112 | 0·0380 | 0·2933 | 0·4391 | 0 6105 | 1·0268 | 1·5461 |

Mean Exponent, 1·8076.

*Angle*, 75°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·6088 | 2·0978 | 3·1240 | 4·3254 | 7·2277 | 10·763 |
| Calculated values divided by 9·112 | 0·0668 | 0·2301 | 0·3428 | 0·4747 | 0·7932 | 1·1812 |

Mean Exponent, 1·7847.

*Angle*, 80°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·4178 | 1·3487 | 1·9669 | 2·6770 | 4·3540 | 6·3495 |
| Calculated values divided by 9·112 | 0·0458 | 0·1480 | 0·2159 | 0·2938 | 0·4778 | 0·6859 |

Mean Exponent, 1·6907.

*Angle*, 85°.

| | | | | | | |
|---|---|---|---|---|---|---|
| Calculated values in oz..... | 0·1174 | 0·4267 | 0·6467 | 0·9077 | 1·5508 | 2·3495 |
| Calculated values divided by 9·112 | 0·0129 | 0·0468 | 0·0710 | 0·0996 | 0·1702 | 0·3878 |

Mean Exponent, 1·8617.

## Article IV.

### *Experiments on Prussic Acid.* By M. Gay-Lussac.

(*Concluded from* p. 52.)

### IV. *Of the Combinations of Hydro-cyanic Acid.*

I flattered myself, when I set about examining hydro-cyanic acid, that I should be able to throw some light on these combinations. But the duties which I have to fulfil obliged me to interrupt my experiments before they had attained that degree of perfection which I thought I could have given them. I acknowledge to chemists the numerous blanks of which I am myself sensible, and I hope that they will give me their indulgence.

Hydro-cyanic acid forms, as we know, with bases simple and triple compounds, the properties of which are still very difficult to explain. I shall begin with the first.

We cannot deny the existence of the hydro-cyanates; for we have seen above that on passing hydro-cyanic vapour over potash or barytes, at a temperature of a dull red, hydrogen is disengaged, which proves evidently that the reduction of the alkali does not take place.

But if it does not take place at a high temperature, it will not, *à fortiori*, take place at the ordinary temperature, and then the acid unites to the alkali without the disengagement of hydrogen. The hydro-cyanates are all alkaline, even when we employ in their formation a great excess of acid. They are decomposed by the weakest acids, and in many respects are analogous to the hydro-sulphurets. When they are destitute of water they support a high temperature, without ceasing to produce Prussian blue with the solutions of iron; or, to speak more precisely, they are changed into cyanurets of oxides. But, in contact of air and water, they are entirely decomposed, and converted into carbonates. To understand the constant alkalinity of the hydro-cyanates, we must recollect that potassium disengages from hydro-cyanic vapour a quantity of hydrogen equal to that which it would disengage from water, and that the cyanuret which we obtain forms an alkaline solution in water. We see, in fact, that the potash formed, or which may be formed, by means of the oxygen of water, ought to exercise a very strong affinity on the portion of acid, which contains hydrogen enough to saturate its oxygen; but that the action of the alkali on the portion of acid whose hydrogen cannot be saturated by its oxygen ought of necessity to be much weaker than the first. The chloruret of potassium gives with water a neutral solution, because the quantity of chlorine which it contains can absorb all the hydrogen corresponding to the oxygen of the potash. While with respect to the cyanuret, and even the sulphuret of potassium, the quantity of oxygen which the metal takes is less than that which corresponds to all the hydrogen in the hydro-cyanic or sulphureted hydrogen acid which the oxide is capable of neutralizing. I believe that this phenomenon of alkalinity and neutrality is general.

Among the simple hydro-cyanates, that of ammonia is the most remarkable. It crystallizes in cubes, in small prisms crossing each other, or in feathery crystals, like the leaves of a fern. Its volatility is such, that, at the temperature of $71\frac{1}{2}°$, it is capable of bearing a pressure of 17·72 inches of mercury; and at 97° its elasticity is equal to that of the atmosphere. Unfortunately this salt is charred, and decomposed with extreme facility. Its great volatility prevented me from determining the proportions of its elements.

If the existence of the hydro-cyanates is undoubted, that of the cyanurets is not less so. I have shown that the oxide of mercury was reduced by hydro-cyanic vapour, that water was formed, and consequently a cyanuret. The compound which has been called prussiate of mercury is likewise a true cyanuret. At a gentle heat, it allows cyanogen to escape, and melts into a reddish-brown liquid, which becomes solid, and assumes a grey colour, on cooling. This last compound bears a high temperature; but if air be present it is reduced to pure mercury; I consider it as a sub-cyanuret. I consider as a character of the cyanurets, at least of those whose existence has been well ascertained, to allow cyanogen to be disengaged when they are exposed to the action of heat. I think it very likely that

the white precipitate obtained when a solution of gold is mixed with hydro-cyanate of potash, is a metallic cyanuret. When hydro-cyanate of potash and iron were melted in a platinum crucible, and kept for some time in a red heat, I obtained a brown mass, which, being dissolved in water, precipitated abundantly a grey powder, soluble only in aqua regia, and which at the temperature of between 392° and 572° burns in the air like phosphorus. It contains a good deal of platinum, and is no doubt a sub-cyanuret of that metal. The aqueous solution, being evaporated, gave me first common hydro-cyanate of potash, and then a great quantity of crystals in acicular needles, quite colourless.

Mr. Porrett analysed cyanuret of mercury, which he considered, with all chemists, as a compound of red oxide of mercury with common prussic acid, and obtained as the result—

| | |
|---|---|
| Hydro-cyanic acid | 13·2 |
| Red oxide of mercury | 86·8 |
| | 100·0 |

If we correct this analysis, by the consideration that the mercury in it is in the metallic state, and that 125½ parts of it absorb 10 of oxygen to form red oxide,* we obtain—

| | |
|---|---|
| Mercury | 79·9 |
| Cyanogen | 20·1 |
| | 100·0 |

On calculating the proportions from the number which expresses the capacity of cyanogen (3·252), which I have given in a preceding part of this paper, we obtain—

| | |
|---|---|
| Mercury | 79·91 |
| Cyanogen | 20·09 |
| | 100·00 |

numbers which correspond perfectly with the analysis of Mr. Porrett.† I shall not state the proportions of the other cyanurets

* This is the result of the analyses of Fourcroy and Thenard; but according to that of cinnabar by Proust, namely, 85 mercury and 15 sulphur, which agrees with some experiments made by myself, we should have 10 oxygen and 140 mercury.

† These statements are not correct. Mr. Porrett's analysis gave

| | |
|---|---|
| Prussic acid | 13·8 |
| Red oxide of mercury | 86·2 |
| | 100·0 |

If we suppose, with Gay-Lussac, that an atom of mercury weighs 25·1, and an atom of cyanogen 3·252, and that they combine atom to atom, we obtain cyanuret of mercury composed of

| | |
|---|---|
| Cyanogen | 11·47 |
| Mercury | 88·53 |
| | 100·00 |

numbers very different from Gay-Lussac's, and approaching Porrett's, analysis.—T.

and hydro-cyanates, because the capacity of saturation which I have given for cyanogen and hydro-cyanic acid will enable chemists to deduce them with facility.

There is no doubt that cyanurets may be formed with the metals but little oxidable. But is this the case, likewise, with the metals which have a strong affinity for oxygen? In particular, what is the nature of Prussian blue?

I think, with M. Proust, that it contains no alkali, and that of consequence it is a simple combination; for after having prepared it without alum, and well washed it, the residue of its calcination gave to water only traces of alkali. It is, then, either a cyanuret or hydro-cyanate of iron. The question thus reduced is not easy to resolve. I can only state the arguments in favour of each of the two opinions which may be adopted.

On distilling Prussian blue, after having strongly dried it, and examining at all the periods of the distillation the elastic fluids disengaged, we always find present carbonic acid and hydro-cyanic acid, but never cyanogen. But if we constantly obtain carbonic acid, hydro-cyanic acid, and even ammonia, it follows that Prussian blue must contain oxygen and hydrogen, and therefore it is natural to suppose that it is a hydro-cyanate of iron. At the same time, if we call to mind that the cyanuret of mercury does not give cyanogen by heat except when dry, and that when moist it furnishes exactly the same products as Prussian blue, we may likewise suppose that this last is a cyanuret of iron, but that it retains water, and that we ought to consider it, according to the fine experiments of Proust on the combinations of water, as a hydrated cyanuret. This supposition becomes probable when we consider that Prussian blue at the instant of its formation is very bulky; that on drying it exhibits the same phenomena as alumina; and, like it, retains water with obstinacy. Further, if Prussian blue were a hydro-cyanate, would it not be very surprising that hydro-cyanic acid should yield to the weakest acids the alkalies which are much more energetic than oxide of iron, while it does not yield that oxide to the most powerful acids? We conceive much better why the cyanurets are decomposed with difficulty by the acids. Thus cyanuret of mercury is decomposed by the hydracids, but not by the acids containing oxygen when diluted with water, because mercury is not easily oxidated. In the same manner, cinnabar and carburet of iron are not attacked by sulphuric acid mixed with water.

Further, the same theory explains a great deal better why Prussian blue is decomposed by red oxide of mercury; for if we consider the first as a cyanuret, its decomposition by the second is the result of the great affinity of iron for oxygen; while, if we consider it as a hydro-cyanate, it would follow that oxide of mercury separates an acid from oxide of iron, which is contrary to all analogy.

Supposing Prussian blue to be a cyanuret of iron, it would be necessary to explain the difference between the white precipitate

obtained by means of hydro-cyanic acid and the proto-salts of iron; or, to account for the change of the white precipitate into blue, by the action of oxygen. I acknowledge that I have not made a sufficient number of experiments to give a satisfactory explanation; but the changes of colour of which I have spoken do not appear to me incompatible with the suppositions that the precipitates of iron are cyanurets.

These precipitates might, according to the opinion of M. Berthollet, differ in part only, in consequence of their proportions, just as is the case with the two chlorides of mercury. But I should consider it as most probable that the white precipitate is a combination of subcyanuret of iron with hydro-cyanic acid, analogous to that of sulphuret of potassium and sulphureted hydrogen, of which I have spoken above. When the hydrogen of the hydro-cyanic acid is removed by means of oxygen or chlorine, we obtain a cyanuret of iron containing the total of the cyanogen which existed in the subcyanuret and the acid. In this case the green precipitate formed by chloro-cyanic acid and the proto-salts of iron would be a combination of subcyanuret of iron and chloro-cyanic acid.

As to the ochry residue, which Prussian blue leaves when digested with an alkaline solution, and which M. Berthollet has considered as a subprussiate of iron, it would be a combination of oxide of iron and Prussian blue.

I have likewise obtained a green precipitate without the presence of chlorine, and, as I conceive, by means of oxygen. Having left cyanogen with water on deutoxide of lead and peroxide of manganese, it was gradually totally absorbed, and the water lost all odour. When filtered, it was of a lemon-yellow colour, and neutral. Lime disengaged from it ammonia in abundance. But muriatic acid did not show in it the presence of hydro-cyanic acid or of carbonic acid; and barytes produced no precipitate. This is the liquid which gave a green precipitate with proto-salts of iron. It is obvious that this green matter is different from that which chloro-cyanic acid forms. It is characterized by not becoming blue when mixed with sulphurous acid. It is no doubt identic with that into which Prussian blue is changed by long exposure to the air.

Since ammonia is formed when cyanogen acts upon moist minium, water must be decomposed. But what becomes of the oxygen in this case? What is the nature of the acid which saturates the alkali? Has cyanic acid been formed? This does not appear unlikely; and I hope to determine the point by operating upon a greater scale than I have hitherto been able to do. On evaporating the ammoniacal solution of which I spoke, I obtained a deliquescent residue, which gave me, by distillation, carbonate of ammonia.

The triple combinations of hydro-cyanic acid with the bases present difficulties not less than those which appear when we consider the binary combinations. Mr. Porrett, to explain their permanence, admits the existence of a peculiar hydro-cyanic acid, containing

oxide of iron as one of its constituents, and possessing a strong neutralizing power. He quotes, in support of his opinion, a very curious experiment. When solutions of the hydro-cyanate of potash or iron are exposed to the action of a galvanic battery, the alkali alone appears at the negative pole, while the hydro-cyanic acid and the oxide of iron appear at the positive pole. I do not dispute the result of this experiment, which I have not repeated; but is it sufficient to establish the opinion of Mr. Porrett; and can we not explain in a different and satisfactory manner the permanence of the hydro-cyanates? *

Hydro-cyanic acid is separated from potash by carbonic acid; and when to this compound we add oxide of iron, more powerful acids are required to decompose it. This is known to every body. But we must observe that the triple hydro-cyanates are analogous to common triple salts, the formation of which supposes a reciprocal affinity between the salts constituting their elements. Alumina offers a striking example; for its combination with sulphuric acid is less neutral, and less permanent, than that which it forms with this acid and potash. The iron, it will be said, is not precipitated from the triple hydro-cyanate of potash by the alkalies, nor even by the hydro-sulphurets, which precipitate it from all its other combinations. But magnesia is precipitated by potash or ammonia from its simple combinations; but it is not when in the state of a triple salt; and this is the case with many other bodies. But can we admit that, when combined with acids, it increases their acidifying power; while we know, on the other hand, that the neutrality of neutral salts is not changed by combination?

We have seen that the hydro-cyanate of potash is alkaline; and we have endeavoured to assign the cause of this. This alkalinity itself announces a disposition to form triple salts when this salt comes in contact with opposite compounds having a common element. Thus the hydro-sulphurets dissolve a great number of sulphurets; and if these combinations were more studied, we should doubtless find that they have a great analogy to the triple hydro-cyanates.

We see from the observations which constituted the object of this memoir that the knowledge of cyanogen opens a new field of researches, which will not be soon exhausted. This gas, when it combines with hydrogen, shows us a remarkable example, and hitherto unique, of a body which, though compound, acts the part of a simple substance in its combinations with hydrogen and metals. It likewise fills up a gap in chemistry, by making us acquainted with a combination of carbon and azote, which was hitherto wanting. I should have wished to have presented to the attention of

* Gay-Lussac does not mention the proofs which Mr. Porrett brought to show the existence of ferrureted chyazic acid. He obtained it in a separate state, and examined its properties.—T.

chemists a more numerous collection of facts, and less hypothetical; but I have already said that I was obliged to interrupt my labours, and that I am myself aware of their imperfection.

While this memoir was in the press I made some experiments on the triple hydro-cyanate of potash and silver, which I shall state, because I consider them as likely to throw some light on the nature of the triple hydro-cyanates.

I prepared a hydro-cyanate of potash with an excess of acid; and by evaporating almost to dryness, I brought it to a determined state of saturation, which is the same as that obtained by dissolving cyanuret of potassium in water. After having dissolved it in a certain quantity of water, I divided it into two portions, and to one of them I added a quantity of cyanuret of silver recently prepared, and well washed. The solution took place rapidly; but the alkalinity remained sensibly the same as that of the other portion, which I preserved for the sake of comparison. I then added hydro-cyanic acid. An additional quantity of cyanuret of silver was dissolved, and the compound became perfectly neutral. By evaporation I obtained hexagonal plates mixed with each other confusedly, which were very soluble in water. Their solution precipitates salts of iron and copper white. The hydro-chlorate of ammonia does not render it turbid. Muriatic acid disengages hydro-cyanic acid from it, and chloride of silver precipitates. Sulphureted hydrogen produces in it an analogous change.

This compound is evidently the triple hydro-cyanate of potash and silver, and its formation ought to be entirely analogous to that of the other triple hydro-cyanates.

Now we have just seen that the cyanuret of silver, on combining with the hydro-cyanate of potash, does not diminish its alkalinity; but that, in consequence of the triple compound formed, becomes capable of saturating itself completely with hydro-cyanic acid, just as ammoniacal gas and carbonic acid gas form a neutral salt by means of water, which however does not possess saturating properties; while, without the presence of that liquid, nothing is ever produced but an alkaline salt. In fine, just as we see a great number of oxides form more neutral triple salts, and more permanent than their binary salts. Hence the comparison which I had made of the triple hydro-cyanates with the common triple salts appeared to me perfectly just.

As we cannot doubt that hydro-cyanate of potash and silver is a combination of cyanuret of silver and hydro-cyanate of potash, I conceive that the hydro-cyanate of potash and iron is likewise a compound of neutral hydro-cyanate of potash and subcyanuret of iron, which I believe combined with hydro-cyanic acid in the white precipitate. We may obtain it perfectly neutral, and then it does not decompose alum; but the hydro-cyanate of potash, which is always alkaline, produces in it a light and flocky precipitate, which is doubtless alumina. To the same excess of alkali we must ascribe

the ochry colour of the precipitates which hydro-cyanate of potash forms with the per-salts of iron. They are combinations of cyanuret of iron, or (not to allege too much) of Prussian blue and oxide of iron. If we make use of a hydro-cyanate perfectly neutral, we obtain only blue precipitates. However, they have still that colour when we employ a hydro-cyanate slightly alkaline, because the solutions of iron, being always acid, saturate the excess of alkali. Thus the remarkable fact, which ought to fix the attention of chemists, and which appears to me to overturn the theory of Mr. Porrett, is that hydro-cyanate of potash cannot become neutral except when combined with the cyanurets.

The affinity of cyanuret of iron for the hydro-cyanate of potash is at least as great as that of cyanuret of silver; for if we boil this last with a solution of hydro-cyanate of potash and iron, no change appeared to me to take place.

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## Article V.

### *On the Chemical Analysis of Soils. In a Letter from Professor Schubler, of Hofwyl, to Professor Pictet.**

SIR, *Hofwyl, Dec.* 10, 1815.

I HAVE the honour to communicate to you, in the annexed table, the results obtained by the chemical analysis of the principal soils found in the immediate neighbourhood of the town of Stuttgart,† which I examined during the course of the last summer with regard to their chemical and physical qualities. If these results demonstrate to us, on the one hand, that these qualities are often analogous in the same kind of soil, they furnish us likewise with proofs of the contrary, very striking and interesting to agriculture; examples which prove sufficiently to what errors we are exposed when we judge of the physical properties of soils, and their influence on vegetation, simply by a knowledge of their chemical elements.

* Translated from the Bibliotheque Britannique for Dec. 1815—Agriculture, p. 411.

† Stuttgart is situated in a warm and fertile valley in the south of Germany, which opens perpendicularly into that of Neckar. Its height above the level of the sea is between 700 and 800 French feet. Its latitude is 48° 46′ 30″. Its mean temperature is 50°. The mean annual quantity of rain falling in its environs between 1807 and 1812 is 24 inches 7·4 lines French. Such researches made in other countries would put it in our power to judge with more justice of the statements respecting the agriculture of different countries, and the ameliorations suitable to each. These countries present peculiar modifications, which depend upon the nature of the soil and the climate. I have published more detailed researches on the situation and climate of Stuttgart, together with the chemical composition of these countries, in an extract, entitled, Versuch einer Medicinischen topographie von Stuttgart, von Cees und Schubler. Stuttgart in der Sattlerichen Buchhandlung.

*Experiments on the principal Soils which form the upper Beds in the immediate Neighbourhood of Stuttgart.*

| | Chemical Composition in 100 parts. | | | | | Physical Qualities. | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Clay. | Sand. | Carbonate of lime. | Gypsum. | Humus, or vegetable earth. | Weight of a French cubic foot. Dry. | Weight of a French cubic foot. Soaked with water. | Property of retaining water. 100 parts of soil retain of water. | Property of retaining heat, that of sand being 100. | Solidity, that of pure clay being 00. |
| | | | | | | Fren. lb. | Fren. lb. | | | |
| *From the valley near Stuttgart.* | | | | | | | | | | |
| Arable soil between Stuttgart and Canstatt .... | 73·7 | 14·0 | 8·8 | 0 | 3·1 | 120 | 156·7 | 61·2 | 72·6 | 82·9 |
| Arable soil from the corn-fields beyond Buchsen-Thor ..... | 70·6 | 25·2 | 1·2 | 0 | 2·8 | 128·1 | 155·7 | 50·0 | 76·0 | 70·2 |
| Soil of a kitchen garden near l'Allée ..... | 65·0 | 24·4 | 6·0 | 0 | 4·5 | 127·0 | 149 9 | 59·0 | 75·6 | 73·8 |
| Arable soil of the fields near Weinsteig ..... | 55·4 | 35·0 | 7·0 | 0 | 2·6 | 129·4 | 150·0 | 58·5 | 76·5 | 72·2 |
| Soil from the meadows under Weinsteig..... | 53·2 | 40·4 | 4·0 | 0 | 2·3 | 129·6 | 157·4 | 52·4 | 76·6 | 68·5 |
| *Soil of vineyards.* | | | | | | | | | | |
| Soil of a vineyard on the Bopser ..... | 61·7 | 33·6 | 3·6 | 0 | 2·0 | 124·3 | 157·7 | 49·5 | 75·9 | 49·2 |
| Soil of the vineyards of Moentchshalden..... | 51·5 | 43·3 | 3·0 | 0 | 2·1 | 130·1 | 165·2 | 47·4 | 76·9 | 34·8 |
| Soil of the vineyards called Capitaelweinberge.. | 54·4 | 40·0 | 3·2 | 0 | 2·3 | 142·3 | 155·4 | 40·0 | 84·8 | 35·3 |
| Soil of vineyards called Griegsberge. They produce an excellent wine ..... | 38·0 | 53·5 | 4·2 | 1·4 | 2·8 | 133·3 | 159·4 | 46·4 | 78·4 | 39·9 |
| *Minerals used as manure.* | | | | | | | | | | |
| Red indurated marl. The kind called Leberkies | 92·6 | 0 | 2·8 | 4·5 | 0 | 147·8 | 187·6 | 36·2 | 88·1 | 23·3 |
| Blue schistose marl (blauer leberkies)..... | 83·4 | 0 | 16·6 | 0 | 0 | 147·4 | 186·6 | 36·0 | 87·9 | 34·5 |
| Ditto with 5 per cent. of carbonate of magnesia | 84·7 | 0 | 10·2 | 0 | 0 | 140·3 | 178·9 | 40·0 | 83·5 | 12·2 |

In analytical chemical experiments the method followed being of material importance, it appears to me essential to explain that which I have employed in examining the soils, and which I prefer in general as best adapted to this kind of investigation. I have already made known to you my manner of proceeding in my physical researches.*

I always begin by passing the soil through a screen after it is dried and well pulverized, in order to separate the stones and vegetable fibres, of which I note the amount per cent., provided I have a sufficient quantity of soil for the purpose. This operation being finished, I proceed to some preliminary experiments, to know by means of re-agents the presence or absence—

*a.* Of carbonate of lime (by acetic acid or muriatic acid, and precipitation by means of oxalate of potash).

*b.* Of sulphate (by boiling it with water, and examining the clear solution with muriate of barytes, oxalate of potash, nitrate of silver, &c.).

*c.* Of humus (by boiling with carbonate of potash).

*d.* Of humus impregnated with an acid, or without an acid (by litmus paper).

I then dry a determinate quantity of the soil, usually 400 or 500 grains, at a temperature of from 86° to 104°. I continue this desiccation till the soil ceases to lose weight. I avoid exposing it to a higher temperature, because that would readily destroy the humus. The soils retain, it is true, humidity at a temperature so low. But it has no such influence, either on the result of the experiments, or on the quantity of earths separated, as to lead into an error, as I dry these at the same temperature, and I notice the degree of force with which each soil retains water.

To separate the lime I employ acetic acid diluted with water, preferring it to the stronger mineral acids, such as muriatic and nitric acids, because these usually dissolve a part of the oxide of iron which the clay of arable soils almost always contains. I discover the quantity of carbonate of lime by precipitating with carbonate of potash or of ammonia. When the soil contains magnesia at the same time, it remains in solution, notwithstanding the precipitation by means of alkaline carbonates; but it precipitates as soon as the liquid is boiled, a process which I continue as long as any carbonate of magnesia continues to fall. I then edulcorate the lime on filters of a known weight, and conclude by drying and weighing the washed earths.

I find the quantity of humus by boiling the soil with a quantity of carbonate of potash. When an arable soil contains only a few per cent. of humus, we obtain, by boiling 400 or 500 grains of soil with 120 or 130 grains of carbonate of potash, a solution of a deep brown colour. This must be separated from the earth by means of a well weighed filter. I repeat this boiling as long as the alkaline

* *Annals of Philosophy*, vol. vii. p. 207.

solution retains its brown colour. Three or four boilings are usually sufficient. The quantity of humus is found finally by determining the loss of weight which the earth has sustained. We may, likewise, obtain the humus in substance by precipitation by means of sulphuric acid and alcohol. I prefer this method of finding the proportion of humus to the old one of baking the soil; because by that method not merely humus, but likewise water, charcoal, and vegetable fibres, are acted upon, though they do not constitute an essential part of the humus. Hence by this method we are exposed to rate the quantity of humus too high. I was surprised to see that Davy was not yet acquainted with the other method; at least he makes no mention of it in his last work on agricultural chemistry, in which he gives only the method of baking. It is to Professor Coome that the honour belongs of having first pointed out the efficacy of alkaline solutions, and their utility in accomplishing the object of which we are speaking.

After the separation of the lime, magnesia, and humus, the residuum consists usually of nothing else than clay and sand, easily separated from each other by washing. Finally, the clay is decomposed into silica, alumina, and oxide of iron, by the usual methods.

When the soil subjected to examination contains sulphate of lime, boiling it with distilled water will be sufficient for its separation when the quantity is but small. But when the quantity is considerable, we must have recourse to carbonate of potash to decompose it.

I conceive that I ought still to add the following observations on the results which the table contains:—

The first five soils drawn from the valley are composed of a considerable quantity of clay, with humus, and a small quantity of lime and sand. They constitute a compact soil, and rather stiff than light, as the experiments on the consistence of these soils demonstrate more particularly. They are most suitable for barley (*epautre*). Wheat and rye do not succeed so well on them.

The four following kinds have been taken from vineyards. They contain less clay, but more lime, than the preceding, together with a portion of sulphate of lime, and traces of magnesia. Their property of retaining water is much inferior to that of the first soils, as are likewise their consistence and their solidity. But they retain heat more strongly. These are the qualities which render them favourable for the cultivation of vines. Hitherto the chemical elements of soils agree very well with their physical qualities.

The three soils which terminate the table are marls proper for the amelioration of other soils. When exposed to the air they easily fall into powder. It is clay which predominates in their composition. Besides the clay and the lime, the last contains five per cent. of carbonate of magnesia.* They contain no vegetable earth. It deserves attention that these soils, notwithstanding their great pro-

* I have found, likewise, another kind of slaty marl in that country, which contains 18 per cent. of carbonate of magnesia.

portion of clay, retain a much smaller quantity of water than the other soils; that their consistence is weak, and their power of retaining heat very considerable. They are often used with advantage in vineyards, by rendering the soil lighter, drier, and hotter.

This I conceive is owing to their fine and slaty texture. When they lose this texture by the slow effect of efflorescence, they recover the properties of common clay; I mean a greater power of retaining moisture, a smaller power of retaining heat, and a stronger consistence. I have even succeeded in bringing about this change by a mechanical process, by triturating small quantities of them strongly, and for a long time. These phenomena explain to us why these mineral ameliorativec gradually lose their efficacy, and require to be renewed from time to time. They cease to be fertilizing as soon as they have lost their slaty texture, and by efflorescence recover the state of common clay. Nothing shows more clearly that soils composed of the same chemical elements may, in consequence of the different forms of their parts, and their dissimilar disposition, exhibit likewise heterogeneous physical qualities, and an opposite effect upon vegetation. Perhaps organic bodies, on the nature of which chemical analysis has hitherto given us so little satisfaction, would exhibit the same kind of compositions if they were subjected to experiments similar to those which we have explained.

The clays separated from the various soils do not all contain the same proportion of silica, alumina, and oxide of iron. From several comparative analyses I have found the mean quantity of these ingredients in clay as follows: 58·5 silica, 32·5 alumina, and 9 oxide of iron. The clay of the arable soils above-mentioned contained only 7 per cent. of oxide of iron, while that of the red marls in the table contained 15 per cent. In all clays which I have examined, silica was always the predominating constituent.

The proportion of magnesia found in some of these soils is equally deserving of attention. It furnishes a new proof that this earth in combination with others may have an advantageous effect on vegetation. The experiments which I have made with small quantities of the carbonate of magnesia confirm this. Grains of corn germinate in it, and grow for some time with the same vigour and health as in garden mould, provided we take care to preserve the requisite degree of dryness or moisture.

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## Article VI.

*Theorems for determining the Amount of Annuities increasing in the constant Ratio of the Natural Numbers,* 1 . 2 . 3 . . . . . *n.* By Mr. James B. Benwell.

(To Dr. Thomson.)

SIR,

I have taken the liberty of sending you the following correct, and I believe new, theorems, for determining in all cases the amounts

of annuities certain when increasing in the constant ratio of the natural numbers $1 . 2 . 3 \ldots\ldots n$, and of the squares and cubes of the natural numbers. This matter has not been properly illustrated by any author that I know of. I am, therefore, desirous to announce them in your Journal, since to readers who are conversant with this doctrine they will, I conceive, be interesting. Mr. Baily, in his Doctrine of Interest and Annuities, has given the investigations relating to this subject; but in the several series there made to represent respectively the amount of these annuities, a remarkable inconsistency exists, the consequence of which is, that the formulæ thence derived are of no avail in satisfying the objects of our inquiries when the annuities are thus annually increasing; and further they are applicable to this purpose only on the assumption of the annuities being actually decreasing in the order specified above; and I cannot but express my surprise that an author who comments with so much freedom and severity upon the scientific labours of his contemporaries should suffer such absurdities to disgrace what, in every other respect, is a very useful and popular performance. I shall now proceed to give the theorems themselves; in order to which I take X to denote the amount of an annuity increasing according to the natural numbers $1 . 2 . 3 \ldots . n$, and Y and Z that increasing by the squares and cubes of them.

Now putting $x$ to denote $1 + r$, the amount of $1l.$ for a year, and $n$ the number of years, these several quantities will be equal to, and truly represented by, the following different series:—

$$X = n + (n - 1)\, x + (n - 2)\, x^2 + (n - 3)\, x^3 + (n - 4)\, x^4 + (n - 5)\, x^5 \ldots\ldots x^{n-1}$$

$$Y = n^2 + (n - 1^2)\, x + (n - 2^2)\, x^2 + (n - 3^2)\, x^3 + (n - 4^2)\, x^4 + (n - 5^2)\, x^5 \ldots\ldots x^{n-1}$$

$$Z = n^3 + (n - 1^3)\, x + (n - 2^3)\, x^2 + (n - 3^3)\, x^3 + (n - 4^3)\, x^4 + (n - 5^3)\, x^5 \ldots\ldots x^{n-1}$$

And the analytical expression for the sum of each series will be respectively,

$$X = \frac{\frac{x}{x-1} \cdot (1 + r^n - 1) - n}{r}$$

$$Y = \frac{\frac{x+1}{x-1} \cdot (1 + r^{n+1} - 1) - (2 . n + 1 + r . \overline{n+1}^2)}{r^2}$$

$$Z = \frac{\frac{6x}{x-1} \cdot (1 + r^{n+1} - 1) + r . 1 + r^{n+1} - r^2 . \overline{n+1}^3 + 3(r . \overline{n+1}^2 + 2n + 2 + r . n + 1) - r}{r^3}$$

As an example in each case when the term is five years, and rate of interest five per cent., we have for

| | |
|---|---|
| X | 16·03825625 |
| Y | 57·56850625 |
| Z | 232·4440064 |

But which numbers, by the erroneous process of computation given in the work already quoted, would come out successively for this term and rate of interest,

| | |
|---|---|
| X | 17·11553125 |
| Y | 64·03215625 |
| Z | 265·19378125 |

The difference in each case rapidly increasing as the rate of interest and term of years increase.

*Pump-row, Old-street Road, June* 8, 1816. J. B. Benwell.

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## Article VII.

### *Remarkable Case of Palsy.* By Dr. Cross.

(To Dr. Thomson.)

SIR, *Glasgow, May* 18, 1816.

Although your publication does not profess to take up medical subjects, yet as "Arts" may be understood to comprehend the healing art, as you have frequently admitted medical remarks, as you must have a partiality for things that are medical, and as the number of people who are crippled for life, or carried off by palsy, continues to increase rapidly, I use the freedom of troubling you with the following case:—

*Glasgow, April* 18, 1816.—Daniel M'Kechnie, petty officer in the royal navy, unmarried, aged 35 years, five feet five inches high, short neck, large head, sanguine temperament, and full habit; had been 15 years in service; nine of them in the West Indies, two in the Mediterranean, and the rest in home stations; had been always while living in a cold climate afflicted with piles, which uniformly disappeared on entering a warm climate; has been as far back as he recollects habitually costive; since leaving the service, in March, 1815, he has continued to eat and drink freely, and has not as yet betaken himself to any employment. He had been drinking pretty freely for about a week past with some of his shipmates who had paid him a visit, and, especially on the Monday, Tuesday, and Wednesday, had been deeply intoxicated. He had, however, refrained from the glass all the Thursday; had in the evening stepped into a hardware shop to sit and converse with a few friends; and a little after eight o'clock, when the shop was about to be shut, was reaching out his hand for his hat, when in a moment he felt himself "quite overcome at the heart," and became for a minute completely blind. After swallowing a little cold water, he came to himself, and soon felt refreshed, and, as he thought, quite recovered. However, on attempting to rise, his right foot gave way under him, the right arm hung powerless from the shoulder,

and the face was drawn towards the left side. In this condition he was carried home. About three quarters of an hour after the attack I saw him. The right extremities, with the exception of very slight motion of the humerus and femur, were completely paralysed. His speech was almost unintelligible. His tongue, when thrust out of the mouth for my inspection, formed an arch, and universal tremor pervaded his frame. From the puffiness of the left temple, the pulsation of an artery could not be felt; the lancet was therefore put in at a venture. No blood having come, the external jugular vein was opened, and about ℥iii. of blood obtained with difficulty. A vein of the left arm was then opened, and ℥xviii. of blood allowed to flow in a full stream. While the blood was yet flowing, the tremors disappeared, and his articulation became more distinct. By and bye he became faintish, and was laid in bed. After recovering a little, he swallowed gr. xxv. of calomel. In two hours afterwards ℥xiv. more of blood were taken from the arm. This time he stood the bleeding better.

*April* 19. *Morning.*—His face is less drawn to a side, and he can now move the whole right arm and leg, though with considerable difficulty. This improvement he dates from a most copious stool in the night-time. He has had two more stools since. All the three had a very dark colour, and most offensive smell. Two strong purgative pills were ordered. A blister was applied to the back of his neck. His diet was restricted to soups, and his drink to water.

*Evening.*—He has again lost the power of moving the right limbs, and the face is considered more drawn to a side; ℥xviii. of blood were drawn in a large stream. By the time the arm was dressed he had recovered the same power which he possessed in the morning of moving the paralytic limbs, and felt "more lightsome about the heart."

*April* 20. *Morning.*—He continues to keep the ground he had gained last night. He had free purging early in the morning. He is ordered to swallow ℥i. of castor oil.

*Evening.*—He has considerably lost the power of moving the right arm, owing, as he thinks, to his having exposed it too much above the bed-clothes. His face is redder than usual; ℥xiv. of blood were drawn from the arm in a very large stream. Soon after the bleeding he acquired the power which he possessed in the morning of moving the arm; gr. xx. of calomel were given.

*April* 21.—Continues to improve; ℥vi. of Epsom salts, and ℥iii. of Glauber, are ordered to be melted in a chopin of cold water, and a wine-glassful to be given thrice a day. He is ordered to keep his bed, and to encourage gentle perspiration.

He continued to improve, and on the 27th was taken out of bed, for the purpose of ascertaining how he could walk, when to his agreeable surprise he walked across the room without making the slightest halt. He was ordered to take a wine-glassful of the solution every night and morning for at least two months.

At this date he is quite well, and going about as usual, but is persevering in the solution.

Instances there have been of spontaneous recovery from hemiplegia. But here the severity and the repetition of the paralytic attacks show the fatal tendency of the disease; while the marked benefit that repeatedly accompanied depletion, or followed at its back, establishes a manifest connexion between the treatment and the recovery. A single case like the present, although it does not warrant any conclusion, nor of course deserve any comment, is in the mean time worthy of being recorded as a fact and as a hint.

I remain, Sir, most respectfully,

Your most obedient servant,

JOHN CROSS.

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## ARTICLE VIII.

### *Analysis of the Mineral Waters of Caversham, Berkshire.* By Amicus.

(To Dr. Thomson.)

SIR,

THERE are many mineral springs in this kingdom, all varying in strength, the contents of which are *known*; but there is reason to believe that, as there is hardly a county in England which does not contain some mineral water, some springs have been passed over, with their contents *unknown*, but which deserved a better fate. If a chemist could be repaid for an analysing tour through Britain, beneficial results might ensue to the community.

An old clergyman has been in the custom of going into Berkshire to drink the waters of Caversham, a village on the confines of Oxfordshire. He fancies he derives benefit from such a course; but to put the matter beyond fancy, the following experiments were undertaken :—

When first drawn, the water is transparent and colourless. Temperature 53° Fahr. Emits slightly the smell of hepatic air. Taste primarily chalybeate, secondarily sulphureous. The water, after a lapse of a few hours, loses its transparency, assumes a brown muddy appearance, and the inner surface of the glass in which it is contained is partially covered with air bubbles. By longer standing, it loses its sulphureous smell, the surface becomes covered with a thin iridescent pellicle, and a precipitate appears adhering to the sides of the vessel.

The following were the appearances from re-agents:

1. Equal quantities of lime-water and of the mineral water being mixed together—milkiness, white precipitate.

2. Concentrated sulphuric acid dropped into it—disengagement of air bubbles.

3. The blue of syrup of violets changed to green.
4. The yellow of turmeric changed to brown.
5. The red of Brazil wood changed to blue.
6. The infusion of galls produced black.
7. The prussiate of potash produced blue.
8. The solution of nitrate of silver produced brown.
9. A portion of the water was boiled and filtered. Then experiments 1, 6, 7, repeated without effect; but in the 8th a slight milky cloudiness ensued.

300 cubic inches of the water were boiled for a quarter of an hour. The gaseous products were found to amount to 37 cubic inches. The sulphureted hydrogen gas being absorbed by nitric acid, the remainder proved to be carbonic acid gas.

The water, when deprived of its gaseous contents, was filtered; the substance left on the filter weighed 27 gr.

The filtered water, by evaporation, gave (α) carbonate of soda, (β) muriate of soda.

The 27 grs. were dissolved in diluted muriatic acid, and the iron separated by ammonia, which, being dried and redissolved in muriatic acid, gave, by the addition of carbonate of potash, (γ) carbonate of iron. Carbonate of soda then added to the liquor precipitated (δ) carbonate of lime.

*Results.*—300 cubic inches.

*Gaseous Contents.*

| | | Cub. In. |
|---|---|---|
| *a* | Carbonic acid gas | 33 |
| *b* | Sulphureted hydrogen gas | 4 |
| | | 37 |

*Solid Contents.*

| | | Grs. |
|---|---|---|
| α | Carbonate of soda | 10 |
| β | Muriate of soda | 6 |
| γ | Carbonate of iron | 18 |
| δ | Carbonate of lime | 9 |
| | | 43 |

For further observations vide Medical and Physical Journal for 1803.

Amicus, olim Alumnus Prælect. Chem. Edin.

*London, June,* 1816.

*Quære.*—In how far is the above water applicable to the purposes of medicine?

## Article IX.

### *On some liquid Combinations of Oxymuriatic Acid.**

By Daniel Wilson, Esq.

During a series of experiments made in the year 1812, for the purpose of endeavouring to discover a substance fitted to discharge the colour of the turkey-red dye by the common method of printing on the cloth, I had occasion to combine oxymuriatic acid gas with several of the earthy and metallic bases. The difficulty of forming some of these combinations induced me to attempt their production by means of double decomposition; and I found that in this manner oxymuriatic acid might be combined with nearly all the substances which form soluble salts with those acids that make insoluble compounds with lime.

And although I did not succeed in forming a substance in every respect fitted for the purposes of calico printing, I had the satisfaction to discover a combination which has since then been used extensively, instead of a solution of oxymuriatic acid gas in water, in discharging the colour of turkey-red cloth by means of the press.

Oxymuriatic acid is employed, in the arts, in its pure state, and in combination with potash, soda, lime, and magnesia. But oxymuriate of lime has, from its cheapness, superseded in a great measure the use of the oxymuriates of potash and soda; and the magnesian combination is only employed in clearing the grounds of printed cotton stuffs, from its injuring the colours less than any of the other oxymuriates, because all the rest generally contain an excess of base, which acts on the more delicate shades. If this excess was saturated with muriatic acid before the solutions were used, they would be all equally inactive. The oxymuriate of magnesia is prepared in the same manner as that of lime, by passing the gas into a receiver containing magnesia, either in a dry state, or moistened with water. But the high price of magnesia has hitherto prevented it from being much employed. I shall afterwards describe a method of preparing it by which it will cost very little more than any of the other oxymuriates.

The turkey-red dye resists the action of oxymuriatic acid in combination with any of the bases hitherto employed; for that reason a solution of oxymuriatic acid gas in water is used in order to discharge it. This is prepared by adding sulphuric or muriatic acid to a solution of the oxymuriate of lime. The muriatic acid has the advantage of not occasioning any precipitate, and therefore may be immediately used.

But, owing to the solubility of this substance, it cannot be applied to the cloth in the usual manner of calico printing. A peculiar

* Read to the Kirwanian Society of Dublin, Dec. 13, 1815.

method has, therefore, been resorted to, which I shall here shortly describe. A plate of lead, about three feet square and one inch thick, is perforated with holes of the shape of the pattern intended to be discharged. The upper surface of this plate is made perfectly smooth, and it is securely fixed on a level in a cast-iron frame. Another lead plate, perforated precisely in the same manner, is attached to the end of a screw, and suspended over the one which is fixed in the iron frame. The upper part of this screw is secured by passing through the centre of a cast-iron arch, which forms part of the frame, and passes over the middle of both plates. The screw is worked by a lever. In some presses the upper plate, instead of being lead, is made of cast-iron, having hollow brass types fixed firmly in it, and fitted to the pattern plate below. I believe the lead is considered to be better.

When a piece of cloth is intended to be discharged, it is thoroughly wetted, and the water again wrung out, so as only to leave it damp. It is then folded on a smooth table into squares the size of the press, and about eight or ten folds in thickness. The piece of cloth thus folded is spread smoothly between the two plates, resting on the surface of the under one. The upper plate is then screwed down with considerable force; and it is so constructed (by means of guides) that when the plates are in contact, the holes in the one shall coincide with the holes in the other. The solution of oxymuriatic acid in water is now poured on the top of the upper plate, which is furnished with a small rim to prevent it from running off. This solution begins in a few minutes to drop from the apertures of the under plate, having percolated through the different folds of cloth, destroying the colour in its passage. When all the discharging liquor is passed through, a weak solution of soap or of carbonate of soda is poured on, and allowed to run off, in order to neutralize any acid that might remain. Buckets of water are afterwards dashed all over the press, the upper plate unscrewed, and the piece of cloth taken out and thrown into water.

It will then be seen that the pressure on the surfaces of the plates has prevented the liquor from penetrating beyond the edges of the apertures, and that no part of the cloth is whitened except the figure of the pattern in the press, which will be found to be equally through all the folds.

Considerable improvements have lately been made in the construction of the discharging press by using thin moveable lead plates instead of the fixed ones formerly employed, which admits of the pattern being changed at a less expense. And by the new method, instead of each piece being separately folded into squares, 10 or 12 pieces are spread at full length above one another on a table; and the discharging is commenced on one end of these pieces, which are regularly drawn through the press into a trough of water accordingly as they are discharged.

The noxious vapour exhaled by the solution of oxymuriatic acid is highly deleterious to the health of the workmen employed in this

process. Some means of fixing these suffocating fumes without injuring the rapidity of their action has, therefore, long been an improvement very much desired.

I have found that the discharging power of the oxymuriates is in proportion to the affinities existing between their component parts; and that, on account of its weak affinities, alumina possesses all the properties necessary for such a purpose.

This combination is prepared by adding to a clear solution of the oxymuriate of lime, in water of the specific gravity 1·060, a solution of alum of the specific gravity 1·100, as long as any precipitate is occasioned. On the addition of the alum a slight smell of oxymuriatic acid gas will be perceived, and a copious white precipitate will appear, and fall to the bottom of the vessel, leaving the liquor on the top clear. This is to be separated from the deposited substance, and kept for use in close vessels. The precipitate may be afterwards washed by a quantity of water until all the soluble part is separated.

A double decomposition has here taken place, sulphate of lime being precipitated, and oxymuriate of alumina remaining dissolved in the liquor.

In the same manner oxymuriate of magnesia may be formed by adding, in place of the sulphate of alumina, a solution of the sulphate of magnesia.

The oxymuriate of alumina, when compared with a solution of oxymuriatic acid gas in water, will be found to possess many advantages in discharging the colour of the turkey-red dye, besides its being free from any noxious exhalation. The latter, from the uncombined acid which it contains, acts on the metals of the press, and often occasions black spots on the cloth. And the texture of the stuff is also injured by the action of the acid, which is very apparent from the discharged part being always the first destroyed by wear. The combination which I have here described contains no free acids, and is not in the least injurious to the cloth.

The rapidity of its action is also greater than a solution of oxymuriatic acid, because the alumina fixes more of the gas than water would condense alone; and does not seem, although combined with it, to take away any of its discharging power. The liquor prepared at the gravities which I have before specified will bear to be diluted with several times its bulk of water; or if used at the full strength, the runnings from the discharging press may be saved, and again employed.

The oxymuriate of alumina may also be used advantageously in the bleaching of fine linens, when it becomes an object that no lime should be fixed in the fabric of the cloth, which is always the case when the oxymuriate of lime is employed, as it contains an excess of base. In the bleaching of cloth which is afterwards to be dyed of a colour of which alumina forms the mordaunt, it will be found of considerable importance, as the alumina will to a certain extent combine with the cloth, and prepare it for receiving the dye.

But when the base of the oxymuriate is lime, and it fixes in the cloth, strong acid solutions must be employed to extract it, as lime has a tendency to injure the brilliancy of several dyed colours.

In some cases, where in printing works pure alumina is required, it may be precipitated by potash from the muriate of alumina, which results from the action of the oxymuriate in the process of bleaching.

Various other applications will suggest themselves to practical men; and it may often be found expedient for them to produce, by means of double decomposition, a variety of the earthy and metallic oxymuriates, and to employ them respectively in bleaching the substances which are afterwards to be saturated by a mordaunt of the same description. Where the sulphate of soda is a waste residuum, as it is in those bleaching works in which the oxymuriate of lime is prepared, it will in all cases be found advantageous to convert, by means of double decomposition, the solution of oxymuriate of lime into that of soda.

But notwithstanding any improvements which may be made in the materials employed in discharging the turkey-red dye by means of the press, it will immediately be perceived that this clumsy operation admits of little or no variety of patterns. A repetition of spots bordered by a stiff wave forms its utmost extent. On the contrary, if the discharging substance could be thrown on the cloth by means of a copper plate, the most delicate outlines would be preserved, and an endless variety of patterns be formed at a moderate expense.

A patent has lately been obtained for a method of accomplishing this, which is both simple and ingenious.

The combinations of oxymuriatic acid are decomposed by all the mineral, and almost all the vegetable, acids. This circumstance has been rendered subservient to the discharging of turkey-red. A pigment composed of citric acid or supersulphate of potash is printed on the cloth, which is afterwards immersed in a solution of the oxymuriate of lime. The oxymuriatic acid, being disengaged on all those parts of the cloth where the citric acid has been printed, instantly destroys the colouring matter, and leaves it white, whilst the rest of the cloth is merely brightened, the oxymuriate of lime acting on the colour but very slowly.

This method has been found to succeed extremely well when the pattern to be discharged is small; but in large patterns the discharging matters extend themselves, or *run* at the edges, which prevents the figure from being well defined. For this reason it is not applicable to large patterns; and, besides, the waste of materials renders it too expensive for it ever to become general.

A strong solution of the oxymuriate of alumina thickened with pipe-clay may be employed as a printing discharger of turkey-red. The best method of applying it is by means of a leaden cylinder, and the cloth after passing through which must be made to enter a stove heated to 150° Fahr. This will immediately cause the colour

to be destroyed. In some cases it may be also applied by a block, in the manner of sieve printing. But this is rather a disagreeable operation, from the high degree of heat required to excite an immediate action.

A very small quantity of the discharging matter ought to be prepared at once, not more than what will last five or six minutes. The solutions of oxymuriate of lime and of alum should, therefore, be always at hand.

This compound may also be applied to the discharging of a variety of other colours. But in the common operations of calico printing there are some inconveniences attending its use, which the ingenuity of operative men may possibly overcome. An unobjectionable method of discharging turkey-red has not yet been discovered; until that takes place, it will be found of advantage in the art. But with regard to its application to discharging with the press, a substance better adapted could hardly be desired. Its adoption in this process will, therefore, most probably be both general and permanent.

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## Article X.

*On Safety Lamps for Coal-Mines.* By J. H. H. Holmes, Esq.

Having bestowed a considerable time in investigating the collieries of Durham and Northumberland, and endeavoured to avail myself of every information which may, when public, tend to give more security to the persons employed in the dangerous profession of mining, I beg leave to offer a few observations upon safety lamps. These I have uniformly examined with caution; and a scrutiny which is alone justifiable from the dreadful effects which may be expected from introducing instruments into a mine not calculated to contend with the circumstances which will always exist there.

Dr. Clanny's original insulated lamp could not be productive of danger when carried for the purposes of exploring, with steadiness and care; but under any other circumstance, such as working the mines, &c. it was liable to be upset, and be deprived of the water which forms its valve. This, however, he has now remedied, and rendered the apparatus much more portable. This Gentleman, whose indefatigable exertions for eight years in the cause of the miners entitle him to the gratitude of the community, has since invented other lamps, and I have just received one made for the purpose of burning either with oil or gas; but not having experimented upon it myself, or seen it experimented upon, I forbear for the present to give my opinion.

Mr. Ryan made a lamp, by which he prevented the water from spilling, by having it perfectly secured, and the glass cylinder surrounded and attached to the bottom by a stuffing box. By this lamp

the burnt air or azote floated on the surface of the air sent in by a bellows for combustion, and was from thence propelled down a pipe, through the water, so as to escape at a valve below. It differs considerably from Dr. Clanny's in make; but Mr. Ryan wishes me to acknowledge that it was from Dr. Clanny he obtained the principle.

I examined Mr. Stevenson's lamp, at the time it was presented to the Literary and Philosophical Society, at Newcastle. This lamp had a number of small holes made in copper at the bottom to admit air for combustion, and was extremely light and portable. When I saw the apparatus, and the inventor, who could not account for the principle, I was astonished at his having been in a mine with it, but am willing to believe that he so stated the fact; though I always wish in these cases to have the law and operation of nature demonstrated.

The alteration that Sir H. Davy has made on this lamp, by weaving the holes, instead of having them punched, makes the invention no better, in my opinion; as I cannot find that even this distinguished chemist has been able to explain why flame will not flow through small apertures.

Mr. Ryan says that he never would permit the workmen under him to use the steel mill, as it was known to explode; from which circumstance those that use it were always in fear. When Mr. Buddle and Mr. Ryan went down the Hepburn Pit, Mr. B. did not dare to bring it within 20 yards of a blower. I would then ask, In what situation must the miner be, when he has no other light, and cuts a blower?

On arriving in London, Mr. Ryan got a small lamp of an inch diameter on Sir H. Davy's improvement, which he exposed to carbureted hydrogen, but found it would not explode. He then sprinkled some *coal dust*, which it would have to encounter, as there is a continual atmosphere of coal dust in a mine. He lighted the lamp, and held it over a gas pipe, and by throwing in dust found it ignite the gas on the outside. On repeating the experiment several times, the same results were produced.

Knowing that powder is used in mines in order to blast the coal, and that much of it is frequently spilled, he subjected it to some coal dust mixed with a small quantity of powder, and found it explode regularly.

On Mr. Ryan's mentioning these circumstances to me, he concurred in objecting to the experiments as unfair, from having been practised upon a lamp of only one inch in diameter. Having procured one, however, from Mr. Newman, in Lisle-street, I proceeded yesterday morning, with Mr. Ryan, to the Gas Works in Dorset-street, where we tried the lamp under a variety of circumstances, in the presence of Mr. J. Wheatcroft, Mr. May, Mr. Pitcher, and Mr. Morris, who all bore testimony to the experiments.

I tried it first over a small gas tube, with coal dust and powder,

which ignited the gas outside; next, with coal dust alone, which, after repeated trials, produced the same results, and left an inflammation at the end of the tube.

On inverting a tea-cup over the cylinder, so as to produce a slight compression on the gas, it exploded from coal dust several times.

We then went to a chemist's, and forced some gas from a bladder against one side of the cylinder, while gas from another bladder was gently pressed on the opposite side; in a short time the gas on the outside inflamed; this I compare to a blower, although the power we were able to use was very inferior to what would be given by the velocity of a blower under ground.

I have entered thus fully upon this subject in the hope that some able scientific characters will give further elucidations. And I consider Mr. Ryan's System of Ventilation as calculated to obviate, in the greatest degree, all the dangers arising from carbureted hydrogen and carbonic acid gas.

I found that the flame of the wick would not penetrate the gauze cylinder, but the inflammation of the gas would, when re-acted upon by a strong current of air.

I have been induced to make these experiments more particularly, from the circumstance of Sir H. Davy having been named in the House of Commons as entitled to Parliamentary reward for them, and from a desire to show that my objections against his plans have not rested upon any personal prejudice, but upon convictions of their insecurity; at the same time that I assert the precedence of my friend Dr. Clanny, and of Mr. Stephenson, to a claim upon the country in this instance; and as every means has been taken to hurry Sir H. Davy's pretensions before Parliament, I feel myself justified in adopting this means in support of truth and justice.

*Adam-street, June* 26, 1816. J. H. H. Holmes.

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## Article XI.

### Analyses of Books.

*A System of Mineralogy.* By Robert Jameson, Professor of Natural History in the University of Edinburgh. Edin. 1816. Three volumes, 8vo. Second Edition.

The first edition of this work was published in 1804 and 1805, in two octavo volumes, at a time when mineralogy had been but little cultivated in this country, and when we were possessed of scarcely any other mineralogical work, except the System of Kirwan. Some very extraordinary attempts were made to run down that edition, by a set of men who confounded petulance with argument, and mistook arrogant ignorance for genius. They found the author guilty

of introducing German idioms into the English language, and of using many words which could not be found in the writings of Addison and Swift. These circumstances were laid hold of with some ingenuity, and made the groundwork of one of the most illiberal invectives ever published, even in this country, where party spirit and abuse may be considered as in some measure interwoven with our habits. The merits of the work itself were never touched on, nor was any account given of the Wernerian mode of describing minerals; because that would have required some knowledge of the subject, which the authors of the invective in question probably did not possess. But this immoral attempt entirely failed of the object which its authors had in view. Their purpose seems to have been to destroy the reputation and credit of the author; but both his reputation and authority have been ever since on the increase, and he is now universally allowed to stand at the head of mineralogy in Great Britain.

The present edition will be found to be a very great improvement upon the former, and indeed is one of the most complete works of the kind that has hitherto appeared. It contains a vast body of facts, collected from a great variety of sources; and in general the authors to whom Mr. Jameson is indebted have been fairly cited. In this respect he constitutes a very striking exception to the conduct of some late writers in this country, who have pillaged their predecessors without any acknowledgment whatever. That undue veneration for Werner and German mineralogists, which induced him in the first edition not to give place to any mineral in the system till it had been admitted by Werner and his school, has been very properly laid aside. In the present edition every new mineral, by whomsoever described and discovered, is admitted into the system, provided it possesses distinct characters. I have only observed in the whole work one example of what I consider as an improper degree of deference to the German school. Mr. Clason and Henry Gahn, of Fahlun, discovered a mineral in the mine of Bjelke, in Vermeland, in Sweden, which was first described by Assessor Gahn, and received the name of *pyrodmalite*, because, when heated, it gives out the odour of muriatic acid. Karsten changed the name to *pyrosmalite*, an alteration likewise adopted by Berzelius. Karsten states this mineral as a muriate of iron slightly mixed with silica, an opinion adopted by Mr. Jameson, though the properties of the substance are quite incompatible with that supposition, and though I had published Hisinger's analysis of it in the *Annals of Philosophy*, vol. ii. p. 467, together with some remarks, in order to show that it could not be a muriate of iron. The correct analysis of Hisinger, as given by him in the fourth volume of the Afhandlingar, p. 317, is as follows:—

| | |
|---|---|
| Silica | 35·850 |
| Black oxide of iron | 21·810 |
| Protoxide of manganese | 21·140 |
| Submuriate of iron | 14·095 |
| Lime | 1·210 |
| Water, and loss | 5·895 |
| | 100·000 |

It is, therefore, a bisiliciate of iron combined with a bilisiliciate of manganese, and containing a little submuriate of iron mechanically mixed with it. The analysis which I published in the *Annals of Philosophy* was quite sufficient to show that Karsten's opinion was ill founded. Mr. Jameson, therefore, ought not to have adopted it.

I think it would have been better if the barytic and strontian minerals had been kept, as formerly, in distinct families. If a chemical arrangement of minerals be possible, it is surely the best, and the one which must ultimately be adopted. Hitherto all attempts at such a classification have failed. But the possibility of the arrangement is now beginning to appear. The placing of silica among the acids is a great step towards accomplishing the task; and Professor Berzelius's arrangement, with which I shall hereafter present my readers, has great merit, and is indeed wonderful for a first attempt. Wherever chemical distinctions, therefore, present themselves spontaneously, we should attend to them. But barytes and strontian being two different earths, it is obvious that the salts composed of them differ very essentially from each other.

I must here notice another inadvertence, which excited in me a good deal of surprise. In vol. ii. p. 291, there occurs the following paragraph:—"The peculiar earth which characterises this mineral (*carbonate of strontian*) was discovered by Dr. Hope; and its various properties were made known to the public in his excellent memoir on strontites, inserted in the Transactions of the Royal Society of Edinburgh for the year 1790." This assertion had made its way into the first edition of Professor Jameson's book (vol. i. p. 601), from which it has been transcribed into this edition without any suspicion of error. The real facts, however, are very different, and I consider it as a piece of justice to Dr. Hope himself to lay them before the public, as nothing can be more disagreeable to an honest and liberal minded man than to have merit ascribed to him to which he is not entitled. Carbonate of strontian became known to mineralogists about the year 1787. In 1789 or 1790 a set of experiments were made upon it by Mr. Cruikshanks, at that time assistant to Dr. Crawford. These experiments were repeated by Dr. Crawford himself, and the result of them published by him in 1790 in the second volume of the Medical Communications. He drew as a conclusion from them that the mineral contained a new earth; and he sent specimens of it to Mr. Kirwan, that his experiments might be repeated, and his opinion confirmed or refuted. In the

Miner's Journal for 1791 a description of its properties, together with chemical experiments on it, was inserted by Mr. Sulzer. About the same time Blumenbach ascertained that it might be swallowed by animals with impunity. In Crell's Chemical Annals for September, 1793, Klaproth published his well-known paper, entitled, Chemische Untersuchung des Strontianits in Vergleichung mit dem Witherit,* which was concluded in the same journal in the February number for 1794. In that paper there is a full detail of its properties, and a description of the salts which it forms; and the paper concludes with this consequence, that strontian is a *selbtsständige, einfach erde*; that is, a *peculiar simple earth.*

Dr. Hope's paper was read to the Royal Society of Edinburgh on Nov. 4, 1793. An abstract of it was soon after printed and circulated; and this abstract was published in 1794, in the fourth volume of the Society's Transactions; so that Dr. Hope's paper was not read till more than a month after Klaproth's paper had been published. A paper by Mr. Kirwan on the same subject, and in which he draws the same conclusions as Dr. Hope, was read to the Royal Irish Academy on Jan. 9, 1794; and it was inserted in the fifth volume of the Memoirs of that Academy. I do not know in what year that volume was published, as there is no date upon the title-page. In that paper no notice is taken of Klaproth's or of Dr. Hope's papers; nor is it at all likely that they were known to Kirwan at the time when his paper was read. The same remark applies to Schmeisser's paper on strontianite, published in the Philosophical Transactions for 1794. He draws the same conclusions with Klaproth, Hope, and Kirwan; but appears to have been ignorant of what they had done. He gives us, however, the history of the experiments and conclusions of Dr. Crawford and Mr. Cruikshanks. It is needless to notice here the papers of Pelletier and Vauquelin, because they were not published till 1797.

From the above statement of facts, which must be well known to every chemist, it is quite clear that the original discovery of the peculiar nature of strontian must be ascribed to Crawford and Cruikshanks; while Klaproth, Hope, Kirwan, and perhaps also Schmeisser, contributed equally, and unknown to each other, in verifying this discovery. If this merit is to be determined by priority of publication, and perhaps also by the accuracy of the results, the greatest share belongs to Klaproth. Dr. Hope's paper was not published till 1798. It possesses much merit; but I consider what he gives respecting barytes as constituting the most original and valuable facts for which chemistry is indebted to Dr. Hope. Almost every thing which he gives concerning strontian had been anticipated by Klaproth. Dr. Hope's paper is much superior, both in accuracy and extent, to that of Kirwan. But Schmeisser contains some things which escaped all the other chemists. For example, he says

* I quote the title from the first volume of Klaproth's Beitrage, published in 1795. In Crell's Annalen the title is Chem. versuchè über die Strontianerde.

that the native carbonate of strontian contains a mixture of lime; a fact which I lately confirmed (*Annals of Philosophy*, vol. vii. p. 399), without being aware at the time that it had been already noticed by Schmeisser.

These inadvertences are the only ones I have observed in the book. In a work of such prodigious detail, and which has obviously been the result of vast reading, it is truly surprising to find so great a degree of accuracy. I think the arrangement of the iron ores might be a good deal improved. They constitute the most confused order in the whole system. Perhaps Mr. Jameson might have adopted D'Aubuisson's views with some advantage. Gadolinite, too, should have constituted a genus apart. As it contains no tantalum, it cannot with propriety be classed in the order which is devoted to that metal. I do not see any reason for putting it among the metals, though this has been done by most mineralogists. Professor Jameson does not appear to have been aware that the analyses of this mineral made by Ekeberg, Klaproth, and Vauquelin, are inaccurate; and that Berzelius has ascertained that cerium, or rather its oxide, enters into its composition to a considerable amount. The following is one of the four analyses of it which he has published, all of them approaching very near each other:—

| | |
|---|---|
| Silica | 25·80 |
| Yttria | 45·00 |
| Oxide of cerium | 16·69 |
| Oxide of iron | 10·26 |
| Loss by a red heat | 0·60 |
| | 98·35 |

Hence its symbol is $f^2 S + c e^2 S + 8 Y S$; or it consists of one atom of silicate of iron, one atom of silicate of cerium, and eight atoms of silicate of yttria.

I think it highly probable that the order of cobalt ores is also susceptible of improvement, though I have not examined these scarce minerals with sufficient care to enable me to decide upon the point. There seems to be little doubt, from Klaproth's analysis of glance cobalt, that it is an alloy of cobalt and arsenic: both on account of its simplicity of composition, and its greater abundance, it ought, I think, to have been placed first in order among the cobalt ores. Laugier published an analysis of grey and white cobalt ores in 1813 (Ann. de Chim. vol. lxxxv. p. 26), of which Professor Jameson does not seem to have been aware, as he does not quote them. It would appear from his experiments that grey cobalt ore is a compound of arseniuret of iron and arseniuret of cobalt. White cobalt ore consists of the same ingredients, with the addition of some sulphuret of iron, which in all probability is only mechanically mixed. I think, therefore, that white and grey cobalt ores should be considered rather as subspecies or varieties than as distinct species. I may mention here, by the bye, that the term *arsenical*

*cobalt ore*, given to white cobalt ore by Mr. Aikin, in his Manual of Mineralogy, ought rather to have been applied to glance cobalt, the true composition of which it denotes. White cobalt ore containing, besides arseniuret of cobalt, likewise arseniuret of iron and iron pyrites, cannot well be expressed by a chemical appellation. At any rate, before giving such a name, it would be necessary to determine by repeated analyses whether white cobalt ore be a chemical compound, or only a mechanical mixture. From the figure of its crystals I have a strong suspicion that it is merely a mixture of glance cobalt ore with arsenical pyrites. The specific gravity given by Karsten is rather against this supposition; but I have doubts how far Karsten's specific gravities are always to be trusted. I suspect that all the cobalt ochres might be reduced under two species, namely, arseniate of cobalt and oxide of cobalt.

Werner seems to me in some instances disposed to multiply species too far. Many of these redundancies have been very judiciously curtailed by Mr. Jameson in this edition. Thus zircon and hyacinth are made to constitute only one species. Corundum and adamantine spar are likewise conjoined. I may notice here, by the bye, an oversight into which Mr. Jameson has fallen in vol. i. p. 44. He says that corundum was known to Dr. Woodward as early as 1768. Dr. Woodward died in 1728. He mentions the mineral under the name of corivindum. In the year 1768 Mr. Berry, an Edinburgh lapidary, received a box of it from Madras. He showed it to Dr. Black, and it became known in consequence to mineralogists. This, I presume, is the fact which Mr. Jameson intended to state, but which he has accidentally written down wrong, and allowed to pass through the press without perceiving the error.

Perhaps our author might in some instances have gone further than he has gone in the suppression of Wernerian species, without any injury to the system. Thus I do not perceive what right miemite and gurhofite have to figure as distinct species. Dolomite, miemite (*bitter spar*), brown spar, and gurhofite, are all compounds or mechanical mixtures of carbonate of lime and carbonate of magnesia. I think I can perceive sufficient differences in the proportions of the constituents to constitute subspecies, but certainly not different species.

The system of mineralogy in this work is divided, as usual, into four grand classes; namely, earthy minerals, saline minerals, inflammable minerals, and metallic minerals. These names are not very appropriate; but the fault is not to be ascribed to our author, who has merely adopted terms hitherto in universal use. I am convinced, however, that it will be necessary to change these names, and even to modify the arrangement in several essential points, before long. The word *metal* has recently acquired a signification a great deal too extensive and vague. Every thing (with a very few exceptions indeed) is now a metal. I think this class of bodies must soon undergo a new arrangement; and several bodies at present called metals will be classed with other bodies. I consider

arsenic, for example, as bearing a much greater resemblance to the simple combustibles than to the metals. Like sulphur, it forms two acids with oxygen, and combines very readily with most metals. It possesses, indeed, the metallic lustre, and conducts electricity; but so does charcoal. It would be better, therefore, to place it among the simple combustibles than the metals. Native arsenic and orpiment would then go among the combustible minerals, and white arsenic might go among the *minerals soluble in water*, which would form a better name for the second class than *salts*; for not one-tenth part of the *salts* which exist in the mineral kingdom have been arranged by mineralogists in the class of salts, but those only which dissolve in water. The term *earthy minerals*, given to the first class, does not apply, as the diamond, which is placed under it, contains no earth whatever. The term *saline minerals*, given to the second class, does not apply, because it contains under it *sapoline*, which is not a salt, but an acid. The third class, *inflammable minerals*, is better named than the others; yet it contains *mellite*, which is a salt, and *graphite*, which is combustible, but not inflammable. The fourth class consists, indeed, entirely of metals; but that is owing to the present vague sense in which the word metal is taken; which would still apply to the class, even if the greater number of the first class were united with it. I think the term *ores* would be better than metallic minerals; and in that case several species at present arranged under it would come to be placed elsewhere.

The first class consists of about 161 species, about 30 of which are new, or at least are not to be found in the first edition of this work. I consider the improvements introduced into this class as the greatest of all. Indeed it was more defective and meagre in the first edition than the other classes, and therefore was susceptible of greater improvements. No pains have been spared to make it as complete as possible, and the vast quantity of important information collected together renders this class exceedingly valuable to the student of mineralogy. The curious details respecting the diamond, the different species of precious stones, agates, marbles, and alabaster, may be mentioned as striking instances. The descriptions are clear, full, simple, and distinct. The localities are stated with singular industry, and the uses in general pointed out with care. The portion of this class with the execution of which I feel least satisfied is the distribution of the zeolites. I think the Haüyan mode of arranging these minerals better than that followed by Werner. I am sensible, at the same time that, from the very defective descriptions of Haüy, it is a very difficult task indeed to assign their proper places to the different species and subspecies to which Werner has given different names. I am not, therefore, surprised that Mr. Jameson has not attempted it; though I wish he had, as I think he is better qualified than any other person to do justice to the subject.

The second class consists of 16 species, arranged under three orders, and nine genera. Only two new species are introduced;

but the arrangement of the whole is very much improved. I may notice, however, that the term *alum*, applied to the first species, is not the proper one, at least as far as my observations and reading have gone. It is a *sulphate of alumina*, and crystallizes quite in a different way from alum, which is a compound of sulphate of potash and sulphate of alumina. Erroneous names ought to be guarded against as much as possible, because they mislead. Mr. Jameson is not, indeed, answerable for this error, which has been adopted by most mineralogists, though not by all. It would have been better to have placed *sassoline*, or native boracic acid, in an order apart, than to have classed it with salts of soda, as it does not contain any of that alkali. This order might have been increased by *sulphuric acid*, which exists native in several springs, and by *arsenious acid*, which ought to be removed from the class of metals to that of *substances soluble in water*.

The third class, or *inflammable minerals*, contains 13 species, arranged under four families. Of these three species and two subspecies are new, or at least are not to be found in the first edition. I think the *honey stone* is very unhappily placed in this class. This fault has been committed by almost all mineralogists, and shows with how little care they have made their arrangements. The last species, *fossil copal*, is improperly named. I have given an account of its chemical properties in the *Annals of Philosophy*, vol. ii. p. 9. Any person who will take the trouble of reading that account, and of comparing it with the well-known properties of copal, will find that the Highgate mineral differs from that resin. Mr. Jameson took the name from Mr. Aikin, and I can easily see the reasons, or rather the authority, that misled that gentleman to adopt the name in question.

The fourth class, or *metallic minerals*, contains 139 species, of which 39 are new, or at least were not contained in the first edition. These are arranged under 22 orders. Considerable improvements have been introduced into the arrangement of these orders, though I think that still further improvements might be made with advantage. The order of arsenic should be expunged altogether; the first and third species, namely, native arsenic and orpiment, should be placed among the combustibles; the second species, or arsenical pyrites, should be placed under *iron*; the fourth species, or oxide of arsenic, should be arranged with the soluble bodies in the second class; and the fifth species, or pharmacolite, should be placed under lime. Tin does not stand well between zinc and bismuth. Indeed I conceive that the ores might be subdivided into four orders, thus—

1. Platinum, iridium, gold, silver, mercury, copper, nickel.
2. Lead, bismuth, tellurium, antimony, cobalt, molybdenum.
3. Iron, manganese, titanium.
4. Tungsten, uranium, tantalum, cerium, tin, zinc.

In this way those ores that have the greatest resemblance would be grouped together.

It is difficult to conceive a reason for placing iridium in the Order

of platinum. From the account of arseniate of lead, I perceive that Mr. Jameson is not acquainted with the fine specimens of that mineral found in the United Mines, near Redruth, in Cornwall, nor with the analysis of Mr. Gregor, published in the Philosophical Transactions for 1809. His result was as follows :—

| | |
|---|---|
| Oxide of lead | 69·76 |
| Arsenic acid | 26·40 |
| Muriatic acid | 1·58 |
| | 97·74 |

I put a high value upon Mr. Gregor's analyses on account of their uncommon accuracy; and therefore consider it as unfortunate that Mr. Jameson did not attend to the present. It applies to his second subspecies, *filamentous arseniate of lead*; but Mr. Jameson's description is very imperfect, from his not knowing the Cornish specimens.

Upon the whole I consider the present work as by far the most valuable mineralogical publication that has hitherto appeared in this country, and as doing infinite credit both to the industry and knowledge of the author.

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## Article XII.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

On Thursday, June 27, a paper by Sir Everard Home was read, giving a further account of the mechanism by means of which insects can walk contrary to gravity, as up perpendicular walls, or along the roofs of rooms. Since his first paper was read, Mr. Bower has drawn the apparatus of the feet of flies, determined by means of the microscope. It turns out different from what he formerly described. In the common bottle fly every toe is supplied with two suckers, very similar in appearance to the pieces of wet leather and string with which boys lift up stones. These suckers appear to be put in action by the voluntary muscles of the insect. In the horse fly each toe is supplied with three suckers. These suckers may be seen by means of a glass when a common fly is walking along the inside of a tumbler. The author concluded his paper with the description of certain cushions upon the feet of some insects to take off the effect of a sudden change from a state of rapid motion to a state of rest.

At the same meeting part of a paper by Dr. Bernardino Antonio Gomez, Physician to the Prince Regent of Portugal, on the Mode of Fumigating Infected Letters, was read. Letters from places sup-

posed to be visited by the plague are fumigated with vinegar after being cut in several places, before they are delivered to the persons to whom they are directed. It became a question in Portugal whether this mode of fumigation was sufficient. Government gave orders to substitute Morveau's mode by fumigating with chlorine. The author of the paper was doubtful how far this practice would be effectual, unless the letters were opened. He requested of Government to be allowed to investigate the point in the first place experimentally, and his request was granted. He found that letters or paper parcels exposed to the fumes of chlorine, even though completely closed by means of sealing-wax, are penetrated by it, and retain the smell for several days. He impregnated tow, cotton, silk, wool, and fur, with the vapour of putrid meat, inclosed a portion of each in a letter, and exposed the letters to the fume of chlorine for half an hour. The putrid odour was destroyed in the tow and cotton; the silk retained a little of it, and the wool and fur smelled still very strongly of putrefaction.

On Thursday, July 4, Dr. Gomez' paper was finished. He made comparative experiments on the disinfecting powers of vinegar, chlorine, and the fumes of sulphur. All of these substances destroyed completely the smell of putridity. With vinegar it was necessary to dip the letters in the liquid, and then dry them slowly by heat. It was necessary to continue the fumes of chlorine till the writing on the back of the letter was somewhat injured. But the fumes of burning sulphur mixed with nitre destroyed the smell completely, without in the least injuring the writing, and more promptly than the other two methods. He considers it, therefore, as the best method of disinfecting letters, to expose them to the fumes of burning sulphur mixed with saltpetre.

At the same meeting two mathematical papers were announced: the first containing a new Demonstration of the Binomial Theorem, by Mr. Light; the second, on the Methods of finding the Fluents of the Functions of Irrational Quantities.

The Society adjourned till Nov. 7.

## GEOLOGICAL SOCIETY.

*April* 19, 1816.—At this meeting a paper by Mr. Wynch, entitled, A Geological Sketch of the Eastern Part of Yorkshire, was read.

The district included in this description is bounded on the south by the Humber, and on the west by the great north road, its extreme length being about 60 miles, and its breadth 40 miles. The Tees, which is at the extreme north of this district, flows over beds of red and white calcareous sandstone, containing gypsum, which rest on the magnesian lime-stone, and are covered by the alum-shale. This latter rock forms the cliffs on the coast at Whitby, and the eastern moorlands, or Hamilton Hills. The upper part of this important deposit is fine-grained sand-stone, covering bituminous shale and coal, the latter of which is in some places, particularly

near Easingwold, worth working. To this succeeds a series of alternate beds of sand-stone and bituminous shale containing subordinate beds of iron-stone, which is partly smelted in the Tyne Iron Company's furnaces, and is partly calcined, and employed in the composition of Roman cement. The last member of this formation is the alum-shale, the thickness of which is unknown, but in some places certainly exceeding 400 feet. Vertebræ of the same species of amphibious animal which occurs in the Lids at Charmouth have been found in this shale; also echinites, ammonites, nautilites, and orthoceratites. Trunks of trees likewise occur, the bark and softer parts of which are converted into jet. Next above the alum shale occurs the oolite lime-stone, forming numerous beds, one of which, near Scarborough, is pea-iron ore, which has been largely employed by the Tyne Iron Company. The Yorkshire wolds consist entirely of hard chalk, which crops out on the banks of the Derwent, and stretches to the south as far as the Humber, being covered in places by alluvial soil.

An Introduction, by the Rev. W. Coneybeare, M.G.S, to Dr. Berger's paper on the Geological Features of the North-Eastern Counties of Ireland, was read.

The district here described is characterized by three distinct systems of mountains. The first of these, comprehending the Mourne mountains, the Fathom hills, the Newry, Ravendale, and Carlingford mountains, is characterized by the prevalence of granite, surrounded by grey-wacke-slate, and by the absence of mica-slate. In these respects it bears a perfect resemblance to the Lead Hills of Scotland, and in general to the moorlands, which form the common boundary of England and Scotland; and in fact appears to be a continuation of them. The second system comprehends the primitive chain of Londonderry, and consists for the most part of mica-slate with primitive lime-stone, having on its southern boundary beds of conglomerate. Hence in structure, as well as in position, it seems to be a continuation of the Grampians of Scotland. The third system occupies the country on the east and west of the valley of the Bann, and is characterized by its vast platforms of basalt, covering old red sand-stone and the newer rocks, and bearing a striking resemblance to the basalt district of Scotland, which lies between the two other systems already mentioned.

*May* 3.—A paper by Dr. Macculloch, Pres. Soc. on the Employment of the Barometer in Measuring Heights, was begun.

*May* 17.—Dr. Bright's paper, entitled, Observations on some Specimens from near Lake Ballaton, in Hungary, was read.

The country in the immediate neighbourhood of Lake Ballaton, and for a considerable distance to the north, is an extensively marshy plain, in the midst of which are seen a few detached mountains with broken rocky summits. One of these, called Csobanez, presents at its base horizontal beds of yellow sand, over which lie extensive scoriated masses, passing into a more compact amorphous rock, which by some mineralogists might be called green-stone, and

by others lava. This rock becomes more dense towards the summit of the hill, and has a strong tendency to a columnar structure. Another hill, called Badacsan, is composed chiefly of a dark-coloured rock resembling chirk-stone, which towards the summit becomes more grey, contains olivine, and assumes a rudely columnar structure. On the west side of the hill is a conglomerate perfectly resembling that which occurs on the ascent of Mount Heckla, in Iceland. The third hill, called Szeglegt, consists of the conglomerate already mentioned, traversed by a vertical vein 12 or 14 feet thick of basaltic green-stone. These three hills are considered by Dr. B. as fragments in situ of a volcanic mountain, the other part of which has been disintegrated and carried off during some of those convulsions which appear at different times to have modified the surface of the earth.

The reading of Dr. Macculloch's paper on the Value of the Barometer for Measuring Heights, which was begun on the 3d inst. was now concluded.

The circumstance which drew the attention of Dr. M. to the subject was the occurrence of some embarrassing discrepancies between two contemporaneous sets of observations made, the one by himself, at Glenroy, and the other by Lord Gray, at Kinfauns, near Perth. The known causes of error in observations on two or more barometers, are of two kinds, variable and constant. The former arise from the varying diameter of the tubes, from the different specific gravity of the mercury employed, from the capillary attraction exerted by the tube on the mercury arising from the imperfect boiling of the latter, and from the different elevation of the stations. The sum of these forms a quantity of error which may be very considerable; but when once known, and allowed for, they cease to influence the general result. The variable causes of error arise from difference of temperature when not corrected by thermometrical observations, and from a difference in the hour of making the observation, which latter is resolvable partly into difference of temperature, and partly into the diurnal oscillation of the barometric column, from causes which are not known. The sum, however, of these variable causes, does not amount to any very great quantity. Dr. M. then proceeds to examine the results given by two self-registering barometers, by the same maker, and stationed the one at the King's Library at Buckingham House, and the other at the house of the maker of the instrument, at Islington. In this case the errors arising from difference in the time of observation were done away, the horizontal distance between the two stations was small, not exceeding three miles; and yet the variations were found to be such as would, in one extreme, indicate the same altitude for both instruments, and in the other extreme a difference of 90 feet. In the second case, three barometers of the usual construction, and stationed in the Royal Observatory, Greenwich, in the Strand, and at Stoke Newington, were compared; and the differences occurring in the course of a year were such as would

occasion a difference of 200 feet in the elevation of the Greenwich Observatory above the Strand. Another set of observations, afforded by the barometers at the Greenwich Observatory at Edinburgh, at Kinfauns Castle, and at Gordon Castle, exhibits still more numerous and larger discrepancies, amounting in the extreme case to a variation of 700 feet in the relative elevations of Greenwich Observatory and Gordon Castle. Much of the variation recorded in all the above cases is in the opinion of Dr. M. to be here attributed to the direction and velocity of the winds, which, as is well known, are extremely partial, especially in our own island: and till the investigation of these atmospherical currents has been pursued more regularly, and to a greater extent than hitherto, the barometer will remain an imperfect instrument, however useful upon the whole.

*June* 7.—A paper by Samuel Solly, Esq. on the Probable Origin of Trap Rocks, was read.

Some geologists consider the trap rocks as ancient currents of lava; others regard them as having been ejected from the interior parts of the earth in a fluid state towards the surface, bending and rupturing the upper strata through which they have forced their way, and in many parts actually arriving at the surface. By other geologists, on the contrary, they are looked upon as deposits, partly mechanical, partly crystalline, from the waters of a universal ocean. A new theory is proposed by Mr. Solly. He supposes that the slaty carbonaceous clays and marls, such as the lyas formation and the various shales, have been subjected more or less to electro-chemical action: that this action, when very powerful, has excited the mass to actual combustion, and, when less intense, has produced proportionally smaller changes: that the results of these changes are the present rocks of trap formation, as they are called. This theory Mr. S. contends is more accordant with actual appearances, and better explains the diversity of geological position, and of mineralogical structure incident to these rocks, than any of the hypotheses at present received.

## ROYAL INSTITUTE OF FRANCE.

### *Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year* 1815.

(*Continued from* p. 69.)

### Mathematical Part.—*By M. le Chevalier Delambre, Perpetual Secretary.*

#### ANALYSIS.

*Memoir on the Ebbing and Flowing of the Sea,* by M. le Comte Laplace.—The phenomena of the tides have at all times attracted the attention of observers and philosophers. Indeed it was impossible that motions so regular and imposing could escape the inha-

bitants of the sea shore, and of navigators who durst not venture to any distance from the shore. Both of these sets of observers collected the circumstances of the alternate motions of the sea, while the philosophers studied their causes. Among the ancients, Posidonius appears to have been one of the most assiduous and successful observers of this great problem. His works are lost; but the extracts from them given by Pliny and Strabo prove that this philosopher was very well acquainted with the general phenomena, and that he had pointed out the principal causes of them, though he had no idea how their causes operated. What we read in Pliny on this subject is almost all that was possible for human sagacity, before Newton unveiled the great law of the universe. Strabo is not quite so exact, nor so particular. This may lead us to suspect that subsequent researches by some unknown person had added to the facts with which Posidonius and Athenodorus were acquainted. Be this as it may, we see in Strabo that all the changes of ebb and flow return very nearly twice during the course of every day; that the time of high and low water varies, and depends upon the rising and setting of the moon, and on its passage over the meridian. He tells us, likewise, that the tides are not equally high at all seasons; but he appears to believe, with Posidonius, that they go on diminishing from the solstice to the equinox, and increasing from the equinox to the other solstice. He distinguishes three sorts of periods, one diurnal, another menstrual, and a third annual. Pliny speaks of a period of 100 lunar months. He had some idea of the effect of the declinations of the moon. An uncertain person, called Seleucus, supposed a zodiacal inequality, in consequence of which the phenomena were more equal towards the equinoxes, and more unequal towards the solstices. This inequality appeared, both in the height of the tides, and in the rapidity with which the sea advances towards and leaves the shore. Strabo adds, that being at Cadiz at the time of the solstice he had seen nothing extraordinary in the tide of the full moon, while at the full moon of the same month he had observed a considerable difference in the height of the tide, and in the way in which it entered the Bætis. His statement contains circumstances in which we may suspect a good deal of exaggeration. But he acknowledges himself that an insulated observation proves nothing, and that it is better to confide in those who have it in their power to make a regular series of observations on the phenomena.

But though he thus pointed out the advantage of a regular series of observations, though the method followed in Egypt to measure the increase of the Nile might have served as a model for measuring the height of the tides, we have no proof that any regular set of observations was ever made. Perhaps only the high tides were marked; but if registers of them were kept, which is very doubtful, they have been all lost, as well as the works of Eratosthenes, Posidonious, Athenodorus, and Seleucus.

What the ancients did not do, or what they did in an incomplete

manner, the Academy of Sciences got accomplished at the commencement of the last century. The harbour of Brest was made choice of, which seemed to have been made on purpose for such observations. The tides there are considerable. The irregularities of the motion are much weakened at the bottom of the harbour where the heights are measured. These observations embrace a period of six years. Lalande published the greatest part of them in the fourth volume of his Astronomy. M. Laplace calculated them according to his theory in the fourth book of the Mechanique Celeste, and these calculations gave him the arbitrary constant quantities relative to that harbour. But do these constant quantities remain always the same, and do they not at length undergo some alteration, in consequence of the changes which nature and art may produce in the bottom of the sea, in the harbour and on the adjacent coasts? This was a point which deserved to be investigated, and which induced M. Laplace to request Government to order a series of observations to be made during an entire period of the movement of the nodes of the lunar orbit. This new series began in 1806, and has been continued till now without interruption; and M. Laplace in his memoir points out some circumstances which will render the subsequent part of it still more precise and conclusive. It already embraces nearly half a revolution of the lunar nodes. This is more than was done a century ago; so that already a comparison may be made between the two periods.

In each equinox and in each solstice M. Laplace has chosen the three syzygies and the three quadratures which are nearest to it. The way in which these observations are combined makes the effect of variations in the distance, and in the motions of the sun and moon, to disappear. It likewise causes several other inequalities to disappear, which mutually destroy each other. The sums of the excess of high water above low water are owing entirely to the great inequality. The law of these sums is determined by this consideration, that their variation is nearly proportional to the square of their distance in time from the maximum. This gives that maximum its mean distance in hours from the tides of the syzygies, and the coefficient of the square of the time in the law of the variation. Each of the eight years furnishes a particular value of this coefficient. The agreement between all these values proves the regularity of the observations; and from the laws established by the author respecting the probability of results, drawn from a great number of observations, we are enabled to judge how far this definite result approaches the truth.

In the quadratures, the method of calculation is the same, only we must restrict to a smaller interval the law of the variation proportional to the square of the time; because when we set out from the minimum the increment is more rapid than the decrement is when we set out from the maximum.

In all these comparisons the influence of the declinations of the heavenly bodies on the tides, and on the law of their variation in

the syzygies and quadratures appears clearly. This point is one of those which the ancients were least acquainted with. Pliny speaks of it, but he appears to confound the declination and the distance from the earth, and to believe that it is less when the declination is southerly. Pliny falls into this mistake because he had no theory, either mathematical or physical. M. Laplace carefully distinguishes them. After having determined the effect of the declinations by those tides in which it ought to be most sensible, he examines the effect of the distances by means of 18 equinoctial tides towards the perigee or the apogee of the moon. "Thus by combining the observations so as to make each element appear that we wish to know, we are enabled to unfold the laws of the phenomena." This rule, well known to astronomers, who have always followed it as much as possible, and facilitated in our days by the analysis, has become one of the most powerful means in the hands of modern astronomy.

"From this examination it follows that the actual height of the tides in the harbour of Brest surpasses by about a 45th the heights determined by former observations. A part of this difference may proceed from the distance of the points at which these observations were made. Another part may be ascribed to errors in the observations. But these two causes do not appear to me sufficient to produce the whole difference, which would indicate with great probability a secular change in the action of the sun and moon on the tides at Brest, if we were certain of the accuracy of the graduation of the ancient scale, taking under consideration its inclination to the horizon. But the uncertainty that exists in this respect prevents us from giving an opinion respecting this change, which ought to fix the attention of future observers. Philosophers will be surprised at the agreement of the ancient and modern observations with each other, and with the theory, with respect to variations in the height of the tides, depending on the declinations and the distances of the heavenly bodies from the earth, and on the laws of their increment and decrement in proportion to their distance from their maximum and minimum. I had not considered these laws in the Mechanique Celeste relative to the variations of distance of the moon from the earth. Here I consider them, and I find the same agreement between observation and theory."

Pliny states that the highest and lowest tides do not happen precisely at the epochs that he mentioned a little above, but somewhat later. He adds that they take place two equinoctial hours after the passage over the meridian. And the reason which he gives is, that all the effects produced by the heavenly bodies require always a certain time to be accomplished. Bernoulli ascribed this retardation to the inertia of the waters, and perhaps also to the time which the moon takes in transmitting her action to the earth. But M. Laplace has shown that, attending to the inertia of the waters, the highest tides would coincide with the sizygies, if the sea regularly covered the whole earth. He has ascertained, from the whole of the celestial

phenomena, that attraction transmits itself with a velocity incomparably more rapid than that of light itself. The true cause of the retardation is the rapidity of the motion of the planet in its orbit, combined with the local circumstance of the harbour. The most remarkable thing in this explanation is, that the same cause may increase the ratio which exists between the actions of the sun and moon on the sea. This particularity might appear merely paradoxical, if it were not founded upon a rigorous calculus. To recognise it, and establish it more certainly from observation, M Laplace has contrived a method, of which we can only give a general idea; for in order to understand it we must have before our eyes the formulas and the calculations of the author.

The formula which expresses the action of the sun is composed of two terms. At the equinox the tide is the sum of these two terms; at the solstices it is the difference. Observations made at these two points will let us know the ratio of the two terms, and the increment produced by the difference of their movements. This increment is almost insensible for the sun; but it is very sensible for the moon, whose motion is 13 times more rapid, and whose action on the sea is three times greater.

By employing this method in the Mechanique Celeste, the author had found that this increment was at least one tenth, and he observed that an element so delicate, a result so singular, required to be determined by a greater number of comparisons. By the new observations, employed in twice the number, this increment is found to be above an eighth. Another method, founded on a comparison of the tides towards the apogee and perigee of the moon, leads to a result quite similar. So that M. Laplace considers it as certain that the increment of the action of the heavenly bodies on the tides in the harbour of Brest does not admit of any doubt. But the remark is so important that he has thought it necessary to strengthen it by all possible means. Till a long series of observations establish the fact better, and furnish the true measure of the increment, the only way of coming to conclusions was to apply to it the general formulas of probability. From the preceding calculations, the increment would be equal to 0·1335 of the action of the moon on the ocean. These formulas show that it is 21400 to 1 that the local circumstances in the harbour of Brest increase the ratio of the action of the moon to that of the sun; but it is 14 to 1 that the value found is not wrong by one half. Thus, concluding with the author that the increment is certain, we are likewise obliged to conclude with him that we must wait for new observations to be pretty certain that we are not mistaken with respect to such small quantities.

It is the ratio of the actions of the sun and moon on the sea that gives the coefficients of the terrestrial nutation, of the inequality of the precession of the equinoxes, and of the lunar equation of the motions of the sun. But it is obvious that we must, in the first place, disengage this ratio from the effects of local circumstances.

Newton and Daniel Bernoulli could not take this correction into consideration, because they did not suspect it. By attending to it, M. Laplace has found that the mass of the moon is equal to $\frac{1}{68 \cdot 7}$ of that of the earth, from which results 9·65″ for the coefficient of the nutation. This is precisely what Meyer found by his theory, to which he added, the observations were not in the least contrary. The observations of Maskelyne give only 9·6″. But the 20th of a second is a quantity which never can be obtained from observations, and very often not from calculation. From his formulas of probability, M. Laplace has found that it is 21400 to 1 that the nutation is not below 9·31″, nor above 9·94″. Astronomers have never supposed it below 9″, and they have never carried it so high as 10″. It follows, likewise, that the inequality of the precession is in longitude 18·04″, and the coefficient of the lunar equation of the tables of the sun 7·56″. This only differs six tenths of a second from the coefficient which we have found from the discussion of a great number of observations of the sun. But in assigning the value 7·50″, we never supposed that we could answer for a difference so slight.

M. Laplace has supposed in his calculations the mean parallax of the sun equal to 8·59″, which he had concluded from his lunar theory, compared with a great number of observations calculated by M. Burckhardt. M. Ferrer has just confirmed that parallax by a new discussion of the transit of Venus in 1769. We obtained only 8·56″ for that parallax from the same transit; but our object was not so much to add to the certainty of that element, as to point out new methods for facilitating the calculus, which is always very long. Accordingly we adopted without discussion the longitudes and latitudes of the different stations. We in general took a mean of the observations, often very discordant, which had been made at the same station by different observers; while M. Ferrel has corrected these longitudes and latitudes by new observations. The discussion of the different observations would have probably led to a selection different from what we made. In such a choice we may select the observations which agree best with those made in other places; by doing which the results will agree better with each other. We shall very readily agree that the probability is in favour of the result of M. Ferrer; but probability is not certainty. The observation of transits is difficult, because they are rare, because the motion is very slow, and because the diameter of Venus is very small. They cannot be depended upon nearer than 20″ of time. We have only 21′ in time to determine the parallax. Hence it would only be certain to a 60th part if we had only one observation in which the error was as great as it might be. But the mean of a great number of observations may diminish the error. We may then with great probability affirm that the parallax of the sun is neither above 8·70″ nor below 8·50″. But we cannot from the transit determine whether it should be 8·56″ or 8·60″. Perhaps we

may arrive at greater precision by another way; but that of the transits will always be the most direct one, and the one which needs least calculation. But it requires long journeys, and it occurs seldom; therefore no other method should be neglected.

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## Article XIII.

### SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *New Aerolite.*

On Oct. 3, 1815, a large stone fell from the heavens about four leagues south-east from Langres, in France. It fell from a dark blue cloud, with a considerable noise, and appeared as if it had been broken into small pieces by an internal explosion. It differed a good deal in its appearance from common aerolites. It was very soft, felt smoother and lighter, and had no action on the magnet. Being analysed by Vauquelin, he found it destitute of nickel and of sulphur, and all the iron in it in the state of red oxide. It contained chromium in the metallic state, or nearly so. Its constituents were as follows:—

| | |
|---|---|
| Silica | 33·9 |
| Oxide of iron | 31·0 |
| Magnesia | 32·0 |
| Chromium | 2·0 |
| | 98·9 |
| Loss | 1·1 |
| | 100·0 |

It appears, then, that this aerolite constitutes a new species.—(See Ann. de Chim. et Phys. vol. i. p. 45.)

#### II. *Tunbridge Wells Water.*

Dr. Scudamore has just published an analysis of the celebrated mineral spring at Tunbridge Wells, which seems to have been conducted with great precision.

The specific gravity of this water, at the temperature of 50°, at which it always rises from the spring, was 1·0007. It is transparent, has a strong chalybeate taste, and becomes muddy when left for some hours exposed to the air. A gallon of the water was found to contain—

| | |
|---|---|
| Of gaseous matter | 13·3 cub. in. |
| Of saline matter | 7·68 grs. |

The gaseous matter consisted of

| | Cub. In. |
|---|---|
| Carbonic acid | 8·05 |
| Oxygen | 0·50 |
| Azote | 4·75 |
| | 13·3 |

The saline matter consisted of

| | Grains. |
|---|---|
| Common salt | 2·46 |
| Muriate of lime | 0·39 |
| Muriate of magnesia | 0·29 |
| Sulphate of lime | 1·41 |
| Carbonate of lime | 0·27 |
| Oxide of iron | 2·29 |
| Trace of manganese and insoluble matter | 0·44 |
| Loss | 0·13 |
| | 7·68 |

From this analysis there would seem to be a small excess of carbonic acid in the liquid more than is sufficient to keep the oxide of iron and lime in solution. I conceive that an atom of oxide of iron and an atom of lime require each two atoms of carbonic acid to keep them in solution. The eight cubic inches of carbonic acid weigh 3·7 grains. 2·8 grains of this will be required to keep the oxide of iron in solution, and 0·12 grain to keep in solution the carbonate of lime; so that there is an excess of 0·9 grain, or very nearly two cubic inches of carbonic acid.

## III. *Highland Dress.*

Most of my readers will be surprised to be informed that the philibeg, or short kilt, so commonly worn in the highlands of Scotland, was not the original dress of these mountaineers. They originally wore a kind of tartan pantaloon, called *trews*. Mr. Thomas Rawlinson, an Englishman, who began an iron manufactory in Inverness-shire, about the year 1724, was the person who first introduced the kilt. This dress was found so convenient that it was very soon generally adopted. See Culloden Papers, p. 103, or the Edinburgh Magazine for 1785, where the fact is stated in a letter from Evan Baillie, of Oberiachan.

## IV. *Crystallization of Lime.*

Gay-Lussac has lately ascertained that pure lime crystallizes in regular six-sided prisms. His method of proceeding was very ingenious. He put a quantity of lime-water into an open glass vessel, which he enclosed, together with another open vessel, containing strong sulphuric acid, in the exhausted receiver of an air-pump. When the sulphuric acid became too weak to act powerfully on the

aqueous vapour, it was renewed. By this means the lime-water was made to evaporate pretty rapidly, and fragments of regular six-sided prisms were deposited in abundance. He gives a short account of these experiments in the Annales de Chimie and Physique for March, just published.

## V. *On Dioptase.*

(To Dr. Thomson.)

SIR,

The notice you have kindly taken, in the last number of your *Annals*, of my communication of the 2d of April last, at the same time calls for my acknowledgments and gives the opportunity of extending my view of the subject then before me.

By referring you to Berzelius, who considers the dioptase as a siliciate of copper merely, and to the analysis of Lowitz, perhaps unknown to Berzelius, I certainly did not consider your new ore of copper as a dioptase when I stated they had much resemblance to each other. Being possessed of both, I have had frequent occasions to compare them; and by showing how the double combination of copper with carbonic acid and silex was formed, I considered that I had sufficiently maintained the distinction.

In support of that distinction, I propose to submit to you the application of the electro-chemical theory to the analysis of Lowitz, which I adopt as a correct analysis, by which I hope to afford you another instance from a modern analysis of the truth of that theory, and its utility as a test to the accuracy of analyses, at the same time aiding the discovery of the real composition of minerals. In my last communication I endeavoured to show that your new ore of copper is a trisiliciate united to a carbonate of the same basis, or a double acid, in which one atom of copper united with three atoms of silex and one atom of carbonic acid in the same mineral. I shall now attempt to show from the analysis alluded to that the dioptase is a trisiliciate of copper united with a bihydrate of the same basis.

The dioptase, as analyzed by Lowitz, 2 Lucas, p. 352, is—

| | |
|---|---|
| Oxide of copper | 55 |
| Water | 12 |
| Silex | 33 |

Now if, as before, I am allowed to suppose that the copper is equally divided between the silex and the water, the constitution of the mineral will be—

| | | | Oxygen. | |
|---|---|---|---|---|
| Oxide of copper united to silex | 27·5 | contains | 5·46 | 1 |
| Oxide of copper united to water | 27·5 | | 5·46 | 1 |
| Water | 12·0 | | 10·58 | 2 |
| Silex | 33·0 | | 16·38 | 3 |
| | 100· | | | |

Hence the mineral is

One atom of copper combined with three atoms of silex, as is also the case in your new ore of copper; and

One atom of copper combined to two atoms of water; or

$$Cu\ H^2 + Cu\ S^3 -.$$

The minerals agreeing in their combinations of the base with the common acid, we have to admire the order of nature that the union of the base with another acid, for so I must consider water to operate in this instance, has not varied the proportion.

I may here be allowed to quote the observation of Haüy, tom. 4, p. 360, that the aqueous principle which has been hitherto regarded as necessary to the regular assemblage of the particles of matter, appears in this instance of the essence of the constitution of the mineral.

I am, Sir, your obedient humble servant,

*June* 3, 1816. An Electro-Chemical Theorist.

## VI. *Remarks on Arsenious Acid, and on Nitrate of Silver as a Test of that Substance.* By Mr. R. Phillips.

(To Dr. Thomson.)

DEAR SIR,

In your account of the Improvements in Physical Science for the Year 1815 (*Annals of Philosophy*, vol. vii. p. 33) you mention a memoir by M. Fisher on the properties of arsenic, in which he states that "white oxide of arsenic is insoluble in water. Its solution takes place only when it is changed into an acid by combining with a greater proportion of oxygen, which it absorbs at the expense of the undissolved portion. Hence the reason why the undissolved portion loses its white colour, and becomes of a dirty yellow. This change takes place at all temperatures between that of the common temperature of the atmosphere and boiling water."

Having considerable doubts of the accuracy of these statements, I boiled a fragment of white oxide of arsenic, weighing about a drachm, in successive portions of distilled water. From the commencement to the close of the operation no discolouration whatever of the oxide occurred, and it was dissolved by the water without leaving an atom insoluble. Each portion of the solution, treated with ammonia and nitrate of silver, gave the well known yellow precipitate which indicates arsenious acid.

As M. Fisher's experiments are, therefore, totally inaccurate, it is perhaps scarcely worth while to inquire whether he supposes that the portion of oxide which combines with the larger quantity of oxygen is arsenic acid, or an intermediate oxide. That no arsenic acid is formed during ebullition is evident, because the precipitate which the solution yields with silver is yellow, instead of possessing the brick-red colour described by Klaproth as characteristic of arseniate of silver. (Essays, vol. ii. p. 151.)

In the *Annals of Philosophy*, vol. vii. p. 236, you also mention

that the experiments of a student at Guy's Hospital had occasioned some doubt whether the yellow colour of arsenite of silver is sufficiently characteristic of that substance to prevent it being confounded with phosphate of silver.

I have made several comparative experiments on the subject, and I am warranted by the results in asserting that it is impossible in many cases to distinguish arsenite from phosphate of silver. I do not mean to deny that by special management those who are previously acquainted with the nature of the substances on which they are experimenting may produce slight shades of difference; but whilst engaged in investigating the subject, I repeatedly obtained, without any care as to proportions in either case, precipitates of arsenite of silver and phosphate of silver, which so perfectly resembled each other that to distinguish them was impracticable; consequently it seems to me that silver can no longer be admitted as a test of the presence of arsenious acid, without the corroboration of additional evidence.

I remain yours truly,

*London, May* 25, 1816. R. PHILLIPS.

## VII. *On the Fossil Bones found by Spallanzani in the Island of Cerigo.*

(To Dr. Thomson.)

SIR,

In perusing Cuvier's justly esteemed Essay on the Theory of the earth, I was struck by an assertion of that celebrated naturalist respecting the fossil bones described by Spalanzani as existing in the island of Cerigo, and which the Italian philosopher has attributed to the human species. M. Cuvier affirms that the fossil bones brought from Cerigo by Spalanzani have none of them ever formed part of a human skeleton, as he had examined every individual fragment of those deposited at Pavia by the Italian naturalist; and Professor Jameson, in a mineralogical note on Mr. Black's translation of Cuvier's essay, says that the error of supposing such bones to be human had been detected by Blumenbach.

I have not the presumption to pass any remarks on the circumstance of such men as Spalanzani, Cuvier, and Blumenbach, being so entirely at issue on a question of this nature. As the first mentioned philosopher, however, has long ceased to be a party to the discussion of affairs in this world, and consequently cannot rescue his reputation as a skilful naturalist from mortal attacks, by entering the lists even with such an antagonist as M. Cuvier, I did, I assure you, experience no small pleasure in having a circumstance brought under my observation, which in my opinion went a great length to confirm my belief of Spalanzani's accuracy in this instance, without at all interfering with the justice of Cuvier's beautiful theory.

In the year 1811 I saw in the island of Cephalonia, *at no great distance from Cerigo*, a great many catacombs, resembling those of Syracuse and Malta, which had been dug at remote periods in a

lime stone rock, similar to that of the grotto of St. Paul's, in Malta, and in these tombs human bodies had been interred. From time immemorial these sepulchres had remained closed, till the British officer commanding in Cephalonia in 1810 had caused them to be opened, and discovered many human skeletons, several of which were in a fossil state. I procured several of the bones, and still have some of them in my possession.

The cause of these bones having become fossil may easily be explained, by the circumstance of the roof of the catacombs having at different times given way, and thus afforded a passage to water from the surface of the earth into the interior of the tombs. This water had become impregnated with calcareous matter from the rock, with which it had thus come in contact; and this stream passing over the bones had in its turn insinuated the limey particles, which it held in solution, into their substance. In this way some of the bones had become so intimately joined with the adjacent rock on which they reposed as to have the appearance of having been imbedded in the strata at a time when the whole had been in a fluid state.

Should the above communication appear deserving of a place in your *Annals*, you will oblige me by inserting it.

I remain, Sir, your very humble servant,

*June* 14, 1816. R. Y.

## VIII. *New Hygrometer.*

Mr. Daniel Wilson, of Dublin, has contrived a new hygrometer, which promises to be more delicate, and much more accurate, than any instrument of the kind hitherto thought of. He takes the urinary bladder of a rat, which is a small, stout, spherical body, and ties it firmly to the lower extremity of a thermometer tube. The bladder is then filled with mercury; so that when the bladder is exposed to a perfectly moist atmosphere, the mercury stands near the bottom of the tube. This point is marked zero. The instrument is now suspended in a glass vessel, together with a quantity of strong sulphuric acid, so as to render the atmosphere with which it is surrounded as dry as possible. The dimensions of the bladder diminish somewhat, in consequence of which the mercury rises in the tube. The point at which it remains stationary is marked 100°, and the distance between 0 and 100 is divided into 100 equal parts, or degrees; so that 0 on this instrument denotes extreme moisture, and 100 denotes extreme dryness. Perhaps it would be advisable to reverse the scale, by placing 0 at the point of extreme dryness, and 100 at the point of extreme moisture. This instrument is so delicate that the approach of the hand makes it sink several degrees. Mr. Wilson has made comparative experiments with these instruments for more than a year; during which time they did not alter their nature, and they corresponded quite correctly with each other at the end of the time. The inventor has taken out a patent for his instrument, and there is reason to hope that it will soon be on sale.

IX. *Answer to Mr. Atkinson's Observations in the Annals of Philosophy, vol. vi. p.* 309: *and on the Resolution of Biquadratic Equations.* By Mr. Lockhart.

(To Dr. Thomson.)

SIR,

Circumstances have prevented me from giving earlier consideration to Mr. Atkinson's letter published last October in the *Annals.* It is there asserted, and made the premises of subsequent reasoning, that the quantity $t^2 - \frac{b}{3}$ is negative in reducible equations. I shall put the assertion to the test of such an equation, for instance $x^3 - 2x = 4$, where $b = 2$, $t = 1 + \sqrt{-1}$, and $t^2 - \frac{b}{3} = 2\sqrt{-1} - \frac{2}{3}$ which is not a negative quantity, but an imaginary or fictitious quantity. This quantity, $t^2 - \frac{b}{3}$, as connected with the equation $x^3 - b\dot{x} = c$, is either positive, or zero, or imaginary, *never negative.* It is unnecessary to discuss this subject, because the algebraist can determine by an easy practice of a few minutes, on such numerical equations as he may choose to select, whether the quantity asserted to be negative, is so or not, or whether Mr. Atkinson or I have more correctly conceived the nature of the roots.

As to the number of cube roots belonging to numerical quantities, I am of opinion that it is infinite. This is no new opinion. A learned friend has lately informed me that it was discussed in the Leipzig Acts about 40 years since. I do not perceive that Mr. Atkinson has proved or disproved any thing relating to this subject, which the concluding part of his letter might induce your readers to suppose had been done; much less has he dismissed from the question those imaginary quantities which perplex the science of algebra, and which on that account I much wish discarded from it for ever.

And now, Sir, I beg leave to introduce a new subject to your notice. There is a passage of frequent recurrence in the inimitable work Ferrarius Redivivus, of Mr. Baron Maseres, in the following words: "which, however, I do not know how to prove." The learned Baron is then resolving biquadratic equations according to the method of Descartes. I would refer you particularly to p. 338, where the equation treated on is $x^4 - rx = s$. The employment of the three letters, $e$, $f$, and $g$, causes much perplexity in the demonstration, which does not satisfy me, nor many other algebraists. While meditating on this subject, I discovered a method not only of resolving all binomial biquadratic equations, employing only two letters, but also the symbolical resolution of equations of all degrees of the forms $x^n \pm px = s$, connected with results singularly interesting to science. These I will send you in a succeeding letter. The equation $x^4 - rx = s$ is resolved in the following manner:—

The equations $x^2 - a x = b$, $x^4 - \overline{a^3 + 2 a b}\, x = a^2 b + b^2$ have the common roots, namely, $\frac{a}{2} \pm \sqrt{\left(\frac{a^2}{4} + b\right)}$. To find the values of $a$ and $b$, put $a^3 + 2 a b = r$.

$$a^2 b + b^2 = s$$

$$b = \frac{r - a^3}{2 a},\ b^2 = \frac{r^2 - 2 r a^3 + a^6}{4 a^2},\ a^2 b = \frac{r a - a^4}{2}.$$

Hence $a^6 + 4 s a^2 = r^2$, which is the cubic of Descartes. The value of $a$ being thus obtained, $b$ is known from the simple equation $a^3 + 2 a b = r$; and by means of the equation $x^2 - a x = b$, the affirmative and negative values of $x$ are found. The impossible roots are contained in the equation $x^2 + a x = - a^2 - b$.

I am, Sir, your obedient servant,

*Field Head, June* 6, 1816. James Lockhart.

## X. *Moon-stone.*

There is a mineral of a white colour, and beautifully opalescent, which is found in rolled pieces in the island of Ceylon. It has been long known, and highly valued, by jewellers under the name of *moon-stone*. Mineralogists have always arranged it along with felspar; but it does not appear that its true mineral characters have ever been ascertained, or that any person has subjected it to chemical analysis. Mr. Mawe, of the Strand, to whom this circumstance had been mentioned by Count Bournon during his late visit to London, had the liberality to sacrifice some very fine polished specimens, which were worth a considerable sum, in order to enable me to decide the point. I have accordingly examined this stone, and the result leaves no doubt that it is in reality perfectly pure felspar. When broken, it appeared foliated with a double rectangular cleavage, a property which is well known to characterize felspar. Dr. Wollaston took the specific gravity of a fragment which did not weigh much more than half a grain, and found it to be 2·6, which is as near the specific gravity of adularia, namely, 2·564, as could be expected with so small a fragment. The chemical analysis led to nothing new. It exhibited silica, alumina, and potash, as in common felspar; but my experiments were made on so small a scale that I do not choose to state the proportions which I found, for fear of misleading others.

## XI. *Lepidolite.*

Mr. Holme, of Peter House, Cambridge, found lepidolite in abundance in beds of primitive lime-stone at Dalmally, and other parts of Inverness-shire. When the lime is burnt, the lepidolite still remains unaltered, and may be obtained in a separate state by dissolving the lime-stone in a weak acid. I got a specimen from him of lime-stone containing many small scales of lepidolite, which had been burnt or reduced to quick-lime about two years ago. I presume, however, that the calcination had been very imperfect;

for the specimen still retained a good deal of compactness, and was not in that friable state which a piece of quick-lime left to the air till it is again saturated with carbonic acid assumes. The highlands, at least where the lepidolite occurs, are at a considerable distance from coal. Hence fuel is an object of considerable expense, and of course the farmers economise it as much as possible. Indeed I have seen peats employed for the purpose of burning lime in that country.

## XII. *Carburet of Phosphorus.*

In the year 1799 M. Proust, to whose sagacity we are indebted for the discovery of so many important chemical facts, announced the existence of a compound of carbon and phosphorus, and described some of its properties. (Ann. de Chim. vol. xxxv. p. 44.) It is the substance which remains behind when new made phosphorus in a liquid state is made to pass through shamois leather. He says, likewise, that the red substance which remains behind when phosphorus is converted into phosphorous acid by slow combustion is carburet of phosphorus. It does not appear that these assertions of Proust have been constated by subsequent experimenters. Nor am I aware that the existence of carburet of phosphorus is recognised by chemists at present. I have had occasion to examine the red substance which remains behind after the combustion of phosphorus more than once. In general it is an oxide of phosphorus, though I twice obtained charcoal when I exposed a quantity of this red matter to a strong heat in a glass tube. On other occasions I found it oxide of phosphorus, without any sensible quantity of charcoal. This accounts for the great difference between the results of the experiments of Vogel and Thenard made upon this red substance; one of these gentlemen examined the pure oxide; the other, the oxide contaminated with charcoal.

Some time ago I accidentally discovered a real carburet of phosphorus; and I can now procure it at pleasure in any quantity. I shall give an account of the process by which it may be obtained hereafter, when I have cleared up one or two points which at present perplex the theory of the process. What Proust considered as *carburet of phosphorus* was not that substance in a state of purity, but either mixed or combined with oxide of phosphorus.

Carburet of phosphorus has a dirty lemon-yellow colour. It is a soft powder, without either taste or smell. It undergoes no change though kept in the open air. It does not melt when heated, nor is it altered in the least, though kept in a temperature considerably higher than that of boiling water. But at a heat a good deal below redness it burns, and at a red heat it gradually gives out its phosphorus; but the charcoal remains behind in the state of a black matter, being prevented from burning by a coating of phosphoric acid with which it is covered. When the powder is thrown over the fire, in small quantities at a time, it burns in beautiful flashes, very much like what happens when Howard's fulminating mercury is treated in the same manner.

By my analysis it is composed of one atom of carbon and one atom of phosphorus. But my analysis was made upon too small a scale to be correct, unless the carburet be entirely free from water, which I have not yet determined in a satisfactory manner. If I find water, on repeating the analysis on a greater scale, I shall be led to expect to find two atoms of carbon for one atom of phosphorus. My present analysis on one grain gave

| | |
|---|---|
| Carbon | 0·38 |
| Phosphorus | 0·62 |
| | 1·00 |

## Article XIV.

### *New Patents.*

James Younie, of Theobald's-road, Middlesex, Ironmonger; for an invention for the prevention or cure of smokey chimneys. March 23, 1816.

Abraham Rogers, of Sheft, Halifax, coal-merchant; for a method of effecting a saving in the consumption of coal or fuel, by an improvement in the mode of setting or heating boilers of steam-engines, and other bodies of different descriptions; also for heating and warming stoves, drying-houses, manufactories, and other buildings; and for burning different descriptions of glasses. March 23, 1816.

Henry Osborne, of Bordesly, near Birmingham; for a method or principle of producing various cylinders. March 23, 1816.

William Lewis, of Brimscomb, dyer; for a machine for fulling woollen or other cloths, that require such process. April 5, 1816.

Joseph Turner, of Layton, mechanic; for an improved rotary engine, and an application thereof to useful purposes. April 8, 1816.

William Atkinson, of Bentinck-street, Mary-le-bonne, architect; for a method or methods of forming blocks with bricks and cement in the form of ashlar stone, for building, so as to have the appearance of stone. April 9, 1816.

John Woodhouse, of Bromsgrove, civil engineer; for a method of forming the ground for roads and pavements, and also for paving and repairing old pavements and roads. April 9, 1816.

William Stenson, of Coleford, engineer; for an improved engine, to be worked by steam, or any other power. April 9, 1816.

William Lassalle, of Bristol; for a new method or contrivance for an improvement in the construction of a gig, and of cards, so called in the clothing and other manufactories, or other machines or instruments used and employed in such manufactories, for the same or similar purposes. April 23, 1816.

## Article XV.

# METEOROLOGICAL TABLE.

| 1816. | Wind. | Barometer. Max. | Min. | Med. | Thermometer. Max. | Min. | Med. | Evap. | Rain. |
|---|---|---|---|---|---|---|---|---|---|
| June 17 | S W | 29·92 | 29·82 | 29·870 | 67 | 48 | 57·5 | — | |
| 18 | S W | 29·88 | 29·79 | 29·835 | 71 | 53 | 62·0 | ·36 | |
| 19 | W | 29·96 | 29·88 | 29·920 | 69 | 47 | 58·0 | — | |
| 20 | N | 29·96 | 29·95 | 29·955 | 74 | 55 | 64·5 | ·25 | |
| 21 | N E | 29·95 | 29·95 | 29·950 | 71 | 51 | 61·0 | — | |
| 22 | N E | 29·95 | 29·94 | 29·945 | 78 | 53 | 65·5 | ·21 | — |
| 23 | S W | 29·85 | 29·82 | 29·835 | 69 | 50 | 59·5 | — | 1·06 |
| 24 | N W | 29·97 | 29·85 | 29·910 | 63 | 48 | 55·5 | — | |
| 25 | N E | 29·97 | 29·86 | 29·915 | 73 | 56 | 64·5 | ·37 | |
| 26 | Var. | 29·60 | 29·54 | 29·570 | 70 | 54 | 62·0 | — | 2·05 |
| 27 | N E | 30·00 | 29·60 | 29·800 | 63 | 50 | 56·5 | — | |
| 28 | N W | 30·07 | 30·00 | 30·035 | 69 | 47 | 58·0 | — | |
| 29 | Var. | 30·08 | 29·90 | 29·990 | 78 | 58 | 68·0 | ·39 | |
| 30 | Var. | 29·90 | 29·76 | 29·830 | 76 | 53 | 64·5 | ·16 | ·19 |
| July 1 | N W | 29·80 | 29·75 | 29·775 | 63 | 51 | 57·0 | — | |
| 2 | S W | 29·84 | 29·80 | 29·820 | 73 | 53 | 63·0 | — | — |
| 3 | N W | 29·86 | 29·84 | 29·850 | 64 | 50 | 57·0 | — | ·13 |
| 4 | Var. | 29·75 | 29·72 | 29·735 | 65 | 50 | 57·5 | ·44 | ·18 |
| 5 | Var. | 29·88 | 29·75 | 29·815 | 66 | 46 | 56·0 | — | |
| 6 | Var. | 29·89 | 29·75 | 29·820 | 69 | 56 | 62·5 | — | — |
| 7 | S | 29·75 | 29·70 | 29·725 | 69 | 52 | 60·5 | — | — |
| 8 | S | 29·70 | 29·69 | 29·695 | 70 | 52 | 61·0 | ·45 | ·26 |
| 9 | S W | 29·69 | 29·66 | 29·675 | 70 | 51 | 60·5 | — | — |
| 10 | S W | 29·66 | 29·63 | 29·645 | 73 | 51 | 62·0 | — | ·05 |
| 11 | N W | 29·72 | 29·66 | 29·690 | 66 | 54 | 60·0 | — | — |
| 12 | N W | 29·90 | 29·72 | 29·810 | 65 | 48 | 56·5 | ·63 | — |
| 13 | N W | 29·94 | 29·90 | 29·920 | 67 | 49 | 58·0 | — | — |
| 14 | S W | 29·90 | 29·69 | 29·795 | 65 | 58 | 61·5 | — | ·32 |
| 15 | S W | 29·69 | 29·68 | 29·685 | 71 | 52 | 61·5 | — | ·12 |
| 16 | Var. | 29·69 | 29·66 | 29·675 | 63 | 52 | 57·5 | ·36 | ·77 |
| | | 30·07 | 29·54 | 29·816 | 78 | 46 | 60·3 | 3·62 | 5·13 |

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.

*June* 23.—Cloudy morning: showery day: evening cold. 24. Cloudy morning: a strong cold wind from the N. W. 26. The early part of the morning was fine: wind N. E.: changed to the S. W. between 10 and 11 o'clock, and began to rain, which continued without intermission all day: in the evening and night, was extremely heavy. 27. Morning very much overcast: the rain fallen from nine o'clock yesterday morning to nine o'clock this morning amounts to 2·05 inches, a very unusual quantity for the neighbourhood of London. 29, 30. Foggy mornings: overcast.

*July* 1.—A *Stratus* on the marshes at night. 2. A little rain about 10 o'clock, p. m. 4. Showery day: some hail about three o'clock, p. m.: wind variable, chiefly S. W. 5. Showery morning: the day was fine. 6 Morning overcast: heavy dew. 8. Showery morning: fine day: a heavy shower of rain between nine and ten o'clock, p. m. 10. Showery morning: fine afternoon. 12. Cloudy morning: squally afternoon. 13. A heavy shower of rain about ten o'clock, p. m. 14. A gentle rain nearly all the day. 15. Rainy morning: showery day. 16. Very rainy day.

## RESULTS.

Winds variable: for the most part Westerly.

| | | |
|---|---|---|
| Barometer: | Greatest height | 30·07 inches |
| | Least | 29·54 |
| | Mean of the period | 29·816 |
| Thermometer: | Greatest height | 78° |
| | Least | 46 |
| | Mean of the period | 60·3 |

Evaporation, 3·62 inches. Rain, 5·13 inches.

Laboratory, Stratford, Essex, *July* 18, 1816. JOHN GIBSON.

*** During the absence of Mr. Howard, the Editor has been favoured with a continuation of the Journal by his friend Mr. Gibson.

BRITISH MUSEUM
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NATURAL HISTORY


GEOLOGY OF THE COUNTRY ROUND BIRMINGHAM.

Engraved for W. Thomsons Annals.—for Baldwin, Cradock & Joy, Paternoster Row Sept. 1. 1816.

ANNALS

OF

PHILOSOPHY.

SEPTEMBER, 1816.

## ARTICLE I.

*Geological Sketch of the Country round Birmingham.*
By Thomas Thomson, M.D. F.R.S.

THAT part of Staffordshire which borders upon Birmingham has been long distinguished by its collieries, and contains the thickest bed of coal known to exist in any part of Great Britain. Mr. Keir, of Birmingham, who wrought one of these collieries, drew up an account of the structure of that neighbourhood about 20 years ago, which was published in the first volume of Shaw's History of Staffordshire. I spent about a month in Birmingham during last April and May, and took the opportunity of comparing Mr. Keir's descriptions with the places described, and likewise of visiting several spots which he does not notice. I was thus enabled to form a general notion of the structure of that part of England, and I shall endeavour to state here as concisely as possible the result of my observations. They were confined to a circle, the radius of which was about 15 miles, and of which Birmingham constituted the centre.

Birmingham, as is well known, lies nearly in the centre of England; and its neighbourhood, especially towards the north and north-west, for an extent of at least 16 miles, seems to me to be the most populous part of England which I have seen, always excepting London and its immediate vicinity. When we stand upon the hill on which Dudley Castle is built, the whole country, as far as the eye can reach, appears one continued town. This is owing partly to the great number of iron furnaces which exist in the neighbourhood of Dudley, and partly to the manufacture of nails, more of which are made in this country than in all the rest of Great Britain

put together. The nailors live in detached cottages scattered up and down the country, having each a small smithy attached to it, in which the man, his wife, and children, are all busily employed in making nails.

Birmingham is elevated about 500 feet above the level of the sea, and is, therefore, perhaps the very highest part of the great tract of level country which constitutes so considerable a portion of the south of England. Its elevation has been well ascertained by means of the numerous canals which proceed from it in all directions, and afford a level both to the east and west coast. It must be observed, however, that the height above the level of the sea, as determined by canal locks, is not to be implicitly depended on; for according to the data furnished by the canals, the Irish Sea is 50 feet above the level of the German Ocean. From the position of the Irish Sea, and the great height of its tides, we might expect its level to be rather higher than that of the German Ocean; but 50 feet is certainly far beyond the truth. Barbeacon, a conspicuous spot about eight miles north of Birmingham, is 750 feet above the level of the Thames at Brentford. Mr. Creighton determined its height above Birmingham by levelling. He found that the height of this spot, as given by the authors of the trigonometrical survey, deviates no less than 150 feet from the truth. The highest peak of the Rowley Hills, about eight miles west from Birmingham, is 900 feet above the level of the Thames at Brentford.

The formations of which this part of England is composed are four in number. The following are their names, beginning with the lowest, and terminating with the highest:—

1. Lime-stone.
2. Coal formation.
3. Basalt.
4. Sand and sand-stone.

1. The coal formation in Fife, Mid-Lothian, Clackmannan, Northumberland, Durham, and Lancashire, appears to lie immediately over the *old red sand-stone.* But this does not appear to be the case in the southern part of Staffordshire. The only rock which has been detected *under* the coal formation is a lime-stone, which forms a range of low hills situated between Wolverhampton and Dudley. It appears also at Walsal and Dawend, where it is very extensively quarried, and likewise near Sutton Colfield, a few miles to the east of Walsal. The probability is, that this bed of lime-stone stretches under the whole of the coal in this district, though it has been observed only in the places which I have just mentioned, because in these it protrudes itself through the coal formation. It would not be surprising if this bed of lime-stone lay over the old red sand-stone; though this is an opinion that will not be soon verified in this part of the country; and I am not aware of any part in the neighbourhood where the lime crops out in such a manner as to show us the kind of rock which it covers.

There are three lime-stone hills lying between Wolverhampton and Dudley; namely, Sedgely Hill, Wren's Nest Hill, and Dudley Castle Hill. They occupy an extent which does not seem to exceed three miles in length, and perhaps half a mile in breadth. They are round-backed hills, nearly flat at top, and the highest point of them is only 900 feet above the level of the sea. They are distinctly stratified. The beds dip at an angle which cannot be less than 45°; and, what is curious, they dip two ways. Those beds that form the east side of these hills dip east, and those that form the west side dip west; so that they meet together at the top of the hill, forming a sloping ridge, like the roof of a house. But the upper part of this ridge has been long ago worn away by the weather; for the outer beds, which can alone be seen, break off before they come in contact with each other. If these beds ever met at the top, the hills at that time must have been about one-third higher than they are at present.

The lime-stone of which these hills are composed is compact, of a bluish-grey colour, with a very slight shade of red. Some of the beds are of a crystalline texture. These are much whiter than the common beds. The upper beds are much mixed with clay, and have a slaty texture. Some of these beds which I analyzed contained more than half their weight of clay. The purest specimens were composed of

| | |
|---|---|
| Carbonate of lime ..... | 92·6 |
| Insoluble matter..... | 7·4 |
| | 100·0 |

This lime-stone, which is very hard, and not in the least translucent, abounds in petrifactions. The most common of these are bivalves, usually of small size, and constituting various species of the *terebratula* of conchologists. I saw also abundance of a very common madreporite, which has not yet received any specific name. Entrochi also are frequent ingredients; but the most extraordinary fossil is the one commonly known by the name of Dudley fossil. A figure of this fossil is given by Parkinson in the third volume of his Organic Remains of a Former World, Pl. XVII. Figs. 11 and 14. It is also figured in the Philosophical Transactions for 1753 by Da Costa (vol. xlviii. p. 286). Eight or nine different species of this fossil have been observed; but it differs so much from all other known animals, that naturalists have been much puzzled how to class it. Linnæus gave it the name of *entomolithes paradoxus*; Da Costa called it *pediculus marinus*; Martin conceives it to be an *oniscus*; and Parkinson contrives a new generic name, *trilobites*, by which he conceives it ought provisionally to be distinguished.

These petrifactions, being all the remains of marine animals, demonstrate that this lime-stone rock was formed in the sea. The dip of the beds on the west side of these hills is greater than on the

east side. This at least is strikingly the case at Sedgely Hill, which is the furthest north of the range.

There can be no doubt that the coal formation lies over the lime-stone at Dudley; for the different coal beds have been traced to the lime-stone hills, against the sides of which they terminate, bending up very sensibly against these sides. On the west side of these hills, where the dip of the lime-stone is greater, the coal formation disappears much sooner than on the east side. Mr. Keir considers this fact as a proof that the lime-stone hills have been forced up through the coal formation in this place, and have occasioned its elevation so as to bring it within the reach of human industry. But this ingenious speculation, though plausible at first sight, will scarcely bear a rigid examination. If we suppose the lime-stone hills to have existed in their present position before the coal beds were deposited, we see a reason why the coal beds should incline against their sides, and should be higher there than at a distance. They would be thinner as they rose against the sides of the hills, and of course would suffer a proportionally less contraction on drying. Hence the apparent twist in the coal beds. If the lime-stone had been forced up through the coal beds, it would be difficult to conceive how its regular stratification could have continued. It is more natural to believe that the lime-stone was originally deposited in the very position which it now occupies, and that the coal beds were deposited at a subsequent period.

2. The coal formation makes its appearance a little to the north of Stourbridge and Hales Owen, and coal pits exist in Beaudesert Park, which is about 16 miles further north. It is not known whether the coal beds continue without interruption to the northern parts of Staffordshire, where the potteries are established, and where coal is found in great abundance. The breadth does not much exceed four miles. At the southern extremity, Stourbridge may be considered the boundary on the west, and Hales Owen at the east. Now the distance between these two towns in a straight line does not, I think, exceed four miles. As we advance north, the eastern edge advances gradually eastwards, and at its northern extremity is nearly as far east as Birmingham. But the western edge experiences nearly the same easting; so that the direction of the coal formation here is north by east.

Some years ago Lord Dudley cut an underground canal to his lime-stone quarries near Dudley, in the course of which undertaking all the coal beds between the lime-stone and the ten yard coal were cut through. All the coal-pits in this country go as low as the ten yard coal, in which their great workings always exist. Hence all the different beds which constitute the coal formation in this place have been cut through, and are known. The following table exhibits the names and thicknesses of these different beds as determined by Lord Dudley's canal, and by a coal-work at Tividale, in the parish of Rowley, wrought by Mr. Keir. This table I have

taken from Mr. Keir's paper above-mentioned, making such alterations in it as will serve to render it more intelligible to the reader. I begin with the lowest bed, which lies immediately over the limestone, and terminate with that bed which constitutes the immediate surface of the earth:—

| Names of the Beds. | Local Names of Ditto. | Thickness. Yds. | Ft. | In. |
|---|---|---|---|---|
| 1. Slate-clay ..... | Wild measures ..... | 30 | 0 | 0 |
| 2. Lime-stone ..... | Lime-stone ..... | 10 | 0 | 0 |
| 3. Slate-clay ..... | Wild measures ..... | 76 | 2 | 0 |
| 4. Coal ..... | 1. *Coal* ..... | 0 | 2 | 0 |
| 5. Slate-clay ..... | Wild measures ..... | 40 | 0 | 0 |
| 6. Coal ..... | 2. *Coal* ..... | 5 | 0 | 0 |
| 7. Slate-clay ..... | Black measures ..... | 2 | 2 | 0 |
| 8. Coal ..... | 3. *Good coal* ..... | 3 | 1 | 0 |
| 9. Gravel? ..... | Rough spoil ..... | 2 | 0 | 0 |
| 10. Coal ..... | 4. *Good coal* ..... | 3 | 0 | 0 |
| 11. Slate-clay ..... | Wild measures ..... | 9 | 0 | 0 |
| 12. Slate-clay ..... | Pot clay ..... | 2 | 0 | 0 |
| 13. Coal ..... | 5. *Heathing coal* ..... | 2 | 0 | 0 |
| 14. Slate-clay ..... | Clunch and iron-stone .... | 7 | 0 | 0 |
| 15. Coal ..... | 6. *Main coal* ..... | 10 | 1 | 6 |
| 16. Bituminous shale .. | Black batt ..... | 0 | 0 | 7 |
| 17. Slate-clay ..... | Catch earth ..... | 0 | 2 | 9 |
| 18. Coal ..... | 7. Chance *coal* ..... | 0 | 0 | 10 |
| 19. Bituminous shale .. | Black batt ..... | 2 | 0 | 0 |
| 20. Slate-clay ..... | Clunch and iron-stone .... | 0 | 2 | 9 |
| 21. Sand-stone ..... | Rock or rock binds ..... | 2 | 2 | 10 |
| 22. Slate-clay ..... | Clunch binds ..... | 1 | 1 | 0 |
| 23. Coal ..... | 8. Chance *coal* ..... | 0 | 0 | 9 |
| 24. Slate-clay ..... | Clunch parting ..... | 0 | 0 | 10 |
| 25. Sand-stone ..... | Strong rock ..... | 1 | 1 | 0 |
| 26. Sand-stone ..... | Rock with laminæ of coal.. | 1 | 1 | 0 |
| 27. Sand-stone ..... | Strong rock ..... | 1 | 1 | 0 |
| 28. Sand-stone ..... | Rock binds ..... | 5 | 1 | 0 |
| 29. Slate-clay ..... | Clunch with iron-stone .. | 4 | 2 | 0 |
| 30. Sand-stone ..... | Rock binds ..... | 5 | 2 | 0 |
| 31. Slate-clay ..... | Clunch with iron-stone .. | 0 | 2 | 9 |
| 32. Slate-clay ..... | Clunch binds ..... | 8 | 2 | 0 |
| 33. Slate-clay ..... | Penny earth with iron-stone | 2 | 1 | 0 |
| 34. Coal ..... | 9. *Coal* ..... | 0 | 1 | 3 |
| 35. Slate-clay ..... | Black clunch ..... | 2 | 1 | 0 |
| 36. Coal ..... | 10. Broach *coal* ..... | 1 | 0 | 9 |
| 37. Slate-clay ..... | Kind clunch ..... | 0 | 1 | 0 |
| | *Carried forward* | 248 | 2 | 7 |

| Names of the Beds. | Local Names of Ditto. | Thickness. | | |
|---|---|---|---|---|
| | | Yds. | Ft. | In. |
| | *Brought over* | 248 | 2 | 7 |
| 38. Sand-stone ..... | Rock binds ..... | 2 | 1 | 0 |
| 39. Shale? ..... | Parting emitting fire-damp | 0 | 0 | 3 |
| 40. Sand-stone ..... | Rock binds ..... | 0 | 2 | 0 |
| 41. Sand-stone ..... | Rock ..... | 0 | 2 | 0 |
| 42. Slate-clay ..... | Fine clunch ..... | 4 | 0 | 0 |
| 43. Slate-clay ..... | Fire clay ..... | 1 | 1 | 0 |
| 44. Coal ..... | 11. *Coal* called two foot coal .. | 0 | 1 | 6 |
| 45. Clay ..... | Soft clunch ..... | 2 | 2 | 9 |
| 46. Slate-clay ..... | Clunch binds ..... | 4 | 0 | 0 |
| 47. Slate-clay ..... | Kind clunch with iron-stone | 3 | 2 | 2 |
| 48. Sand-stone ..... | Black rocky stuff ..... | 2 | 2 | 0 |
| 49. Clay with coal .... | Smutt ..... | 0 | 0 | 3 |
| 50. Sand-stone ..... | Rocky black stuff ..... | 0 | 1 | 0 |
| 51. Slate-clay ..... | Wild stuff ..... | 5 | 1 | 8 |
| 52. Slate-clay ..... | Binds with balls of grey rock | 3 | 2 | 0 |
| 53. Slate-clay ..... | Red wild stuff ..... | 2 | 1 | 6 |
| 54. Sand-stone ..... | Greenish rock ..... | 1 | 1 | 0 |
| 55. Slate-clay ..... | Red wild stuff ..... | 13 | 2 | 6 |
| 56. Slate-clay ..... | Grey clunch ..... | 2 | 1 | 3 |
| 57. Slate-clay ..... | White clunch ..... | 1 | 0 | 3 |
| 58. Clay mixed with coal | Smutt ..... | 0 | 0 | 10 |
| 59. Slate-clay ..... | Clunch with iron-stone in it | 2 | 2 | 3 |
| 60. Sand-stone ..... | Rock with coal interspersed | 1 | 2 | 0 |
| 61. Slate-clay ..... | Red coloured roach ..... | 1 | 2 | 0 |
| 62. Clay ..... | Blue clay ..... | 0 | 1 | 0 |
| 63. Slate-clay ..... | Brown coloured roach .... | 2 | 0 | 0 |
| 64. Red clay ..... | Brick clay ..... | 1 | 2 | 6 |
| 65. Soil ..... | Soil ..... | 0 | 1 | 0 |
| | Total thickness | 313 | 1 | 3 |

From this table we see that the beds distinguished by different names in this coal formation amount to 65, and that its whole thickness is 313 yards, 1 foot, and 3 inches, or about 156 fathoms. The main coal, which is the great object of the colliers in that country, is about 60½ fathoms below the surface in the neighbourhood of Dudley. The beds of coal are 11 in number, five above and five below the main coal. The first bed occurs at the depth of 55 yards, or 27½ fathoms, below the surface; but none of the beds above the main coal are considered as worth working. The beds below the main coal are of very considerable thickness. None of them are wrought in the neighbourhood of Dudley; but on the north side of Bilston, and in Cannock Chace, are the beds which

supply the country with fuel. The main coal, or ten yard coal, consists, in fact, of 13 different beds, some of them lying close to each other, and others separated from each other by very thin beds of slate-clay, called *partings*. The following table exhibits the names and thickness of these different beds, as stated by Mr. Keir, in the Tividale Colliery. I have compared them with some other collieries, and found them nearly the same :—

| | Yds. | Ft. | In. |
|---|---|---|---|
| 1. Roof floor, or top floor | 1 | 1 | 0 |
| Parting of four inches. | | | |
| 2. Top slipper, or spires | 0 | 2 | 2 |
| 3. Jays | 0 | 2 | 0 |
| White-stone, called patchel, one inch. | | | |
| 4. Lambs | 0 | 1 | 0 |
| 5. Tow, or Tough, or Kitts, or Heath | 0 | 1 | 6 |
| 6. Benches | 0 | 1 | 6 |
| 7. Brassils, or Corns | 0 | 1 | 6 |
| Foot coal parting (sometimes only). | | | |
| 8. Foot coal, or bottom slipper, or fire coal | 0 | 1 | 8 |
| John coal parting one inch. | | | |
| 9. John coal, or slips, or veins | 1 | 0 | 0 |
| Hard-stone, 10 inches, sometimes less. | | | |
| 10. Stone coal, or long coal | 1 | 1 | 0 |
| 11. Sawyer, or springs | 0 | 1 | 6 |
| 12. Slipper | 0 | 2 | 6 |
| Humphrey parting. | | | |
| 13. Humphrey's, or Bottombench, or Kid | 0 | 2 | 3 |
| Total thickness of coal | 9 | 1 | 7 |

About five yards of this main coal, namely, the lambs, the brassils, upper part of John coal, bottom part of stone coal, and sawyer, consist of coal of the best quality, which is employed in private houses. The quality of the remainder is inferior. On that account it is used only in the iron furnaces, which abound in this part of the kingdom. The coal is of the species of slate-coal. It does not cake; and burns away more rapidly than Newcastle coal, leaving behind it a white ash. But it makes a more agreeable fire, and does not require to be stirred.

The coal beds dip towards the south, and rise towards the north; so that at Bilston the main coal crops out, and disappears altogether. A very curious phenomenon takes place at Bloomfield Colliery, to the south of Bilston. The two upper beds of the main coal, called the roof floor and top slipper, separate from the rest, and are distinguished by the name of the *flying reed*. This separation grows wider, and at Bradley Colliery amounts to 12 feet, four beds of shale, slate-clay, and iron-stone, being interposed. These two upper beds crop out, while the rest of the main coal goes on to Bilston, and is only eight yards thick.

To give the reader some idea of the degree of regularity which the different beds exhibit in this district, I shall give a table of the different beds bored through at Bradley colliery, near Bilston, beginning, as before, with the lowest, and terminating with the surface bed :—

| Names of Beds. | Local Names of Ditto. | Thickness. | | |
|---|---|---|---|---|
| | | Yds. | Ft. | In. |
| 1. Coal | *Heathing coal* | | | |
| 2. Slate-clay | Clunch | 3 | 1 | 0 |
| 3. Shale | Table batt | 0 | 2 | 0 |
| 4. Coal | *Coal* | 0 | 0 | 6 |
| 5. Shale | Hard batt | 0 | 1 | 0 |
| 6. Clay-iron-stone | Iron-stone | 1 | 0 | 0 |
| 7. Slate-clay | White clay | 0 | 2 | 0 |
| 8. Slate-clay | Blue clay | 0 | 0 | 6 |
| 9. Clay | Short earth | 0 | 1 | 6 |
| 10. Coal | *Main coal* | 8 | 1 | 3 |
| 11. Shale | Black batt | 0 | 2 | 6 |
| 12. Clay-iron-stone | Iron-stone | 0 | 0 | 8 |
| 13. Slate-clay | Blue binds | 1 | 2 | 0 |
| 14. Shale | Batt | 1 | 1 | 0 |
| 15. Coal | *Flying reed* | 1 | 2 | 0 |
| 16. Shale | Batt | 0 | 2 | 0 |
| 17. Slate-clay | Blue clunch | 3 | 0 | 0 |
| 18. Slate-clay | Do. containing four thin iron-stone beds | 4 | 0 | 0 |
| 19. Sand-stone | Grey rock | 0 | 1 | 0 |
| 20. Slate-clay | Clunch | 0 | 1 | 6 |
| 21. Sand-stone? | Peldon | 0 | 2 | 0 |
| 22. Sand-stone | Grey rock | 1 | 0 | 0 |
| 23. Slate-clay | Blue clunch | 6 | 0 | 0 |
| 24. Sand-stone | Grey rock | 1 | 0 | 0 |
| 25. Slate-clay | Blue clunch | 8 | 0 | 0 |
| 26. Red sand | Sand | 10 | 0 | 0 |
| 27. Soil | Soil | 0 | 2 | 0 |
| | Total | 56 | 2 | 5 |

We see from this table that the greater number of the beds which cover the main coal at Tividale have cropped out and disappeared before the main coal got to Bradley. At Tividale the main coal is $60\frac{1}{2}$ fathoms below the surface; at Bradley it is only $20\frac{1}{3}$; making a difference of 40 fathoms. Thus we see that the dip south is pretty considerable, amounting probably to 1 foot in 90. Indeed, if we subtract the flying reed, and all the beds between it and the

main coal, amounting to about three fathoms, we should increase the dip somewhat.

The curious phenomenon of the flying reed seems to show very clearly that the different beds of which the main coal consists were deposited at different times, and at considerable intervals from each other. During one of these intervals the beds separating the flying reed from the rest of the main coal seem to have been deposited towards the north of the field, while no deposit whatever took place towards the south of the field.

The substances which occur in this coal are the same as those found in the coal of other coal-fields; namely, 1. Iron pyrites, which occurs chiefly in that bed of the main coal called Brassils, and which furnishes a coal of the best quality. 2. Galena in very small plates and strings; it occurs likewise in the Newcastle coal. 3. Gypsum and calcareous spar: both of these (chiefly the former) may be seen occasionally in thin plates encrusting pieces of coal. When the coal is in small fragments it is called *mucks* by the colliers. These small fragments are left in the mine, and constitute nearly one-third of the whole coal in the bed. The pillars left standing probably amount to another third; so that the miners in this country extract only one-third of the coals, and leave two-thirds in the mine. This wasteful mode of working is to be ascribed to the low price of coals. As far as I have had an opportunity of judging, and I have been in most of the coal countries of Great Britain, the price of coals at Birmingham is less than any where else except Glasgow.* The consequence is, that the small coal will not bear the expense of removal. It is, therefore, left in the pits in prodigious quantities, where it is speedily destroyed by the weather. It is a pity that this enormous waste, which must hereafter be dreadfully felt in that country, could not be prevented. The consumption of coals in this part of England is prodigious. All the neighbouring counties, to a considerable distance, are supplied by means of the numerous canals of which Birmingham constitutes the centre. Besides this, an immense quantity of coal is required for the iron works, which are established in the neighbourhood of Dudley to the amount of 68. These smelted an immense quantity of iron; probably more than the quantity manufactured in all the rest of Great Britain. But the low price to which iron has of late sunk (about 9*l.* or 10*l.* sterling per ton) has in a great measure destroyed this formerly lucrative manufactory. No less than 32 of the 68 furnaces have stopped, or *been blown out*, as the phrase is in Staffordshire. The Welsh iron manufacturers, it seems, produce a greater proportion of iron from their ore, and work with less coals than they can do in Staffordshire. They are able, in consequence, to undersell them. This opposition has been carried so far as to sink the price of iron

* I consider the wonderful rapidity with which Glasgow has advanced in population, manufactures, and trade, as owing in a great measure to this circumstance. The inhabitants pay less for their coals than is paid in every other part of Great Britain.

much lower than it seems possible to manufacture it at. Before the late peace it sold at 18*l.* per ton, which was almost double its present price.

Tracts of coal occur in this coal-field distinguished by a blacker colour, possessed of less lustre, and burning with less flame, than the common coal. Such tracts are called *blacks.* It contains less bitumen, and approaches nearer to coke than the rest of the coal. In cracks of the superincumbent beds there occur shining pieces of coal, like Kilkenny coal. According to Mr. Keir, it is imbedded in cubic cells, formed by thin planes of calcareous spar, intersecting each other at right angles.

This coal-field contains a less number of sand-stone beds than the coal formations in Scotland and the north of England. The slate-clay, called in that country *clunch,* is harder, has more lustre, and is composed of finer particles, than slate-clay in the coal formation generally is. The *batt* is a black slate or shale, which may be split into very thin fragments, and which in general contains much less bitumen than bituminous shale. It approaches more nearly to *drawing slate* in its appearance, only its slaty fragments are much thinner. The clay-iron-stone occurs in various beds, but is only wrought in two; namely, in the bed that occurs under the broach coal in the neighbourhood of Wednesbury, and in that which occurs under the main coal. This last is the bed usually wrought for iron ore. This ore is what mineralogists term *clay-iron-stone.* It is, in fact, a carbonated hydrate of iron, usually mixed with clay. Probably the proportion of clay is greater here than in Wales. This would account for the greater produce of the Welsh ones, and the smaller quantity of fuel which they consume. This iron ore, when taken out of the mine, is built up in heaps called *blooms,* four feet long, three feet wide, and 22 inches high. It is considered as weighing 35 cwt., each cwt. being 120 lb.: 1000 or 1200 such blooms are usually got from an acre of good mine.

In the Marquis of Anglesea's park, called Beaudesert, there is a mine of cannel coal, which is reserved for the exclusive use of the Marquis's family. This coal has a brownish-black colour, and much less lustre than common coal. The fracture is flat conchoidal, and quite smooth; but the cross fracture is more rough, and on that account has a blacker appearance. This coal is hard, and does not soil the fingers. Interspersed through it are numerous specks of a brown matter, very similar in appearance to Bovey coal.

Many *faults* occur in this coal-field. They are rents in the beds, which are usually filled with clay. Very frequently the height of the beds varies on the two sides of a fault. By a great fault which occurs near Bilston, the dip of the coal is reversed; that is to say, the coal beds on the south side of the fault dip south, and those on the north side dip north. But this is an unusual occurrence.

3. There are 11 small hills, constituting a range that extends for about three miles, beginning at the town of Dudley, and termi-

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W. Creighton

nating about half way between Hales Owen and Oldbury, a little beyond the village of Rowley. They appear at first sight to be a continuation of the lime-stone hills to the north of Dudley, as they proceed nearly in the same direction, and have the same elevation, though their size is not so great. But these hills are composed of very different constituents, and lie in a very different position with respect to the coal formation of this country. They consist of very pure basalt, which in the neighbourhood of Birmingham is called *Rowley rag*, because the village of Rowley is situated on one of these basalt hills, and this hill appears to the eye to be the highest of the whole range. The names of these hills, beginning at Dudley, and proceeding in the order of their position, are as follow:—Corney, Tansley, Bare, Cook's Rough, Ash or Cox's Rough, Turners, Pearl, Hailstone, Timmins, Rowley, Whitworth. These hills are all covered with soil; but quarries have been opened in several of them, and the basalt of which they are composed is employed for mending the roads. The streets of Birmingham are likewise paved with it.

This basalt has a greyish-black colour. Its fracture is small conchoidal, and nearly even, with here and there a little tendency to the splintery. Its lustre is glimmering, owing to very small black crystals being interspersed. They appear to be prisms. Their lustre would indicate them to be *augite*; but, as their colour is black, I rather consider them as *hornblende*. The basalt is opake, brittle, not easily frangible, breaks into fragments with sharp edges, and the paviors in Birmingham complain that they cannot break it into the shape adapted for paving the streets. It is hard enough to cut glass, and to strike fire with steel. It melts before the blow-pipe; and, when heated in an open fire, becomes magnetic, and loses three per cent. of its weight. This stone was analyzed in 1782 by Dr. Withering, who found its constituents as follows:—

| | |
|---|---|
| Silica | 47·5 |
| Alumina | 32·5 |
| Oxide of iron | 20·0 |
| | 100·0 * |

Dr. Withering seems to have missed the lime and soda, which no doubt exist in Rowley rag. But the analysis of minerals was at that time in so imperfect a state, that we have more reason to admire the accuracy which he actually attained, than to be surprised at the mistakes into which he fell.

The basalt in these hills has a very distinct columnar structure. This structure will be seen very well by inspecting Plate LV., which exhibits a view of a quarry as it existed on Pearl Hill in 1808. The view was taken by Mr. Creighton. Mr. James Watt got an

* Phil. Trans. 1782, p. 327.

engraving executed from this drawing; and he had the goodness to favour me with a copy, from which the plate is taken.

These basalt hills lie above the coal formation, which may be distinctly traced passing under them. Hence they must have been deposited at a subsequent period, and they must be quite distinct from the coal formation. As there are no other rocks in this place with which these basalt hills are connected, we have no means of determining of what other formation this basalt constitutes a part. These hills would be a very good field for speculation between the Vulcanists and Neptunists, as the arguments on both sides might be urged without the possibility of refutation.

4. The bed lying immediately under the soil round Birmingham is a red sand of very considerable thickness. In some places it constitutes a sand-stone fit for the purpose of building. In some it is a reddish clay, which is employed for making bricks. This bed is distinctly stratified. It covers the coal beds, and occupies the greater part of the county of Warwick, and all that part of Worcestershire which I examined. The portion of Staffordshire which lies to the east of the coal-field consists of the same red sand. At Hagley and Clent it forms a range of hills, which begin nearly south from Stourbridge, and run east, becoming lower as they proceed, and sinking into the level country before they get as far east as Birmingham. The hills in Worcestershire, nearly to the south of Clent, of which the Licky is the most conspicuous, are composed of the same sand-stone. But in the Licky hill it almost entirely loses the appearance of sand-stone, and puts on that of quartz. This quartz constitutes a hard rock, which breaks in irregular rhomboidal fragments, translucent on the edges, and striking fire with steel. It appears full of small holes, each surrounded with an ochry looking crust. A very careful inspection enables us to detect the grains of sand of which it is composed. The cement is quartz, and it appears to be more abundant than the grains of sand. I traced this quartzy mass passing into loose sand upon both sides of the hill, so that there cannot be a doubt that it was originally in the state of sand; though it would not be easy to say from what source the quartzy cement was derived.

The uppermost bed of this red sand formation consists of water-worn pebbles, many of them of the size and shape of a goose's egg. These pebbles consist chiefly of a flesh-coloured quartz, bearing a great resemblance to the quartzy sand-stone of the Licky. I observed likewise pebbles of chalcedony, of flinty slate, &c. These pebbles were distinctly stratified, the same kind of pebble often forming an entire thin bed, followed by other thin beds of other materials. I did not observe any animal remains in this formation, though I examined it carefully in six or eight different places. Here and there I observed in it thin beds of fine sand, coloured black with manganese; and in one place a thin bed of black oxide of manganese, tolerably pure, but too thin to be of any value.

## Article II.

*Observations upon the Alveus, or General Bed of the German Ocean and British Channel.* By Robert Stevenson, Civil Engineer.*

In the course of making professional inquiries regarding the impression which the tidal waters of the Frith of Forth are making upon some of the most valuable properties situated upon its banks, I was imperceptibly led to compare these with other observations that have occurred to me in a pretty extensive survey of the coast of Great Britain and Ireland. On this subject, involving not only an important question regarding the economical interests of the country at large, but also some points connected with the natural history of the globe, I shall lay before the Society what occurs to me, in hopes that it may at least have the effect of turning the attention of some more skilful observers to its further elucidation.

I am, therefore, in this introductory paper, to endeavour to prove that the tidal waters of our seas are acting upon the coast of this kingdom, and wasting its shores, by a constant and almost invariable progress. This is, perhaps, more or less obvious to every one; but I shall here bring it more distinctly under the notice of the Society, in so far as my intercourse with the different parts of the coast has afforded an opportunity of observing; and shall add such collateral remarks as may occur in the course of this inquiry. Having established the point in this manner with regard to the wasting of the shores or margin of the land next the sea, I shall in a future communication inquire into the cause of this wasting, and endeavour to account for it. Without supposing it to proceed from an increase of the waters of the ocean, or to depend upon any adventitious circumstances connected with the natural state of the tides, I propose to show that it proceeds from a change upon the level or depth of the *alveus*, or general bed, of the German Ocean and British Channel.

It would open a field of inquiry too widely extended to enter upon the evidence of the water of the ocean having in former ages occupied a much higher level than it now does. As already said, I am only at present to trace the encroaching or wasting effects of the sea upon the land. In doing this, I shall begin with the shores of the Frith of Forth, and then proceed northward along the eastern coast to the Moray Frith, Caithness, and the Orkney and Shetland Islands; next slightly notice the Lewis, and the western parts of Scotland; and then turn my attention to the eastern shores of England, and to the British and St. George's Channels. From the extent of coast just alluded to, it will be obvious that I can take but

* Read at a meeting of the Wernerian Natural History Society of Edinburgh, March 2, 1816.

a very slight and partial view of the effects of the sea upon the shores at particular bays and creeks, which might deserve further illustration.

The wasting operations of the sea are not confined to the more exposed parts of the coast, but are observable upon both sides of the Frith of Forth westward, or above Queensferry, where the shores are defended on all sides from the violent attacks of the sea in stormy weather. Here we find, even in this narrow part of the Frith, that the land is gradually washed away by the tidal waters, as, for example, at Lord Dundas's estate at Grangemouth, and all the southern shore by the estate of Kinniel and the Earl of Hopetoun's lands, to Queensferry, at which place the track of the public road is now literally within sea-mark, although at no great distance of time it was defended from the sea by a tract of land. The same remarks are strictly applicable to the shores on the northern side at Culross, and along the estates of Sir Robert Preston and Lord Elgin, and all the way to North Queensferry. From an inspection of charts of the coast, it will appear that these effects are not likely to have been produced from any particular exposure, as this part of the Frith is completely *land-locked*, and is otherwise well sheltered from storms. These appearances would, therefore, seem to imply a change upon the level of the ocean, occasioning an *overfilling* of the various *inlets* of the sea.

Below Queensferry, or to the eastward of it, these effects are perhaps still more remarkable. On the southern side of the Frith at Barnbougle Castle, the seat of the Earl of Roseberry, in former times there was a lawn of considerable extent on the eastern front, and on both sides of the castle. This lawn is now completely washed away by the sea, and it has long since been found necessary to erect a bulwark for the safety of the walls of the castle, which is rapidly approaching to an insulated state, so that the Noble Proprietor has in some measure been under the necessity of building a new mansion-house upon a more elevated situation. Tracing the same shore along the rocky boundary of Granton, Royston, to Wardy and Newhaven, we are no less struck with the powerful effects of this element. Between Newhaven and Leith, where the subsoil consists of strong clay overlaid by a deep tirring of alluvial matters, it is in the recollection of some old fishermen still living, that an extensive piece of link ground or downs existed in front of the lands of Anchorfield, and along these shores, on which they used formerly to dry their nets, and which is now entirely washed away. From the fishing village of Newhaven to Leith, the direct road was formerly along shore on the northern side of Leith Fort; but the road being now carried wholly away, and the sea having penetrated considerably into a field on the eastern side of the houses of Anchorfield, the carriage road takes a circuitous route by another way. A few striking proofs were some years since adduced of the waste of the land by the sea at the citadel of Leith, in a law process connected with the Wet Docks there; and there is reason

for believing that at some former period the land here had extended, probably as far to the northward as the Martello Tower or Beacon Rocks. Proceeding along the southern shores of the Frith of Forth from Leith to Berwick-upon-Tweed, many instances are afforded of the waste of the land by the sea. Between Portobello and the Links of Leith, for example, the public road is in immediate danger of being carried away, although but a few years since it was defended by a considerable portion of land. The shores near Musselburgh, at Morison's Haven, at Prestonpans, have suffered greatly from the sea; as also the Earl of Wemyss' lands of Gosford, Gullenness, and all the shores extending from Dirleton Common to North Berwick, the Earl of Haddington's lands of Tyningham, Dunbar, Broxmouth, Dunglass, to St. Abb's Head, Eyemouth, and the river Tweed. To enter into particulars as to the appearances of waste upon these shores, would be prolix, and perhaps uninteresting. But at all these places I have been eye-witness to the rapid waste of the land, and the progressive encroachment of the sea.

If we turn our attention to the *northern* shores of the Frith of Forth, we shall find instances of the same kind no less remarkable. Of these may be mentioned the shores at the estates of the Earls of Moray and Morton, and Mr. Fergusson of Raith, the damage done to numerous properties bounded by the sea, at the towns of Kirkaldy and Dysart, and the very remarkable and fantastic appearance of the rocks produced by the wasting effects of the sea along the shores in the neighbourhood of Wemyss Castle; and, indeed, all the towns from Methil to Fifeness, particularly the Elie, Wester-Anstruther and Cail, have suffered by the encroachments of the sea, which, in some instances in this quarter, has also taken away parts of the public roads, thrown down the inclosures of gardens and fields, laid waste the piers, and even undermined and carried away dwelling-houses. The point of land called Fifeness affords another proof of the desolating effects of the sea upon the land. The section of the coast here exhibits strata of a very soft and friable sand-stone, with iron-stone and shale. This section I have distinctly traced between the point of Fifeness and the Carr Rock, which lies about a mile and a half off Fifeness, the whole distance between it and the shore forming a series of shoals and half-tide rocks; and as this series of rocks, so easily worn away by the sea, can again be traced near Kingsbarns, at the opposite side of the bay, it seems extremely probable that at no very distant period in the history of the globe, this space between the Carr Rock and the land of Fifeness, may have consisted of firm ground. Along the shores of Balcomie and Cambo, belonging to the Earl of Kellie, and the estate of Pitmilly, considerable sums have been expended in building and rebuilding *dykes* to defend the land against the encroachments of the sea; and, indeed, many of the proprietors along the shores of the Frith of Forth, finding this an endless task, have for the present given it up as a hopeless case. At St. Andrews,

the famous castle of Cardinal Beatoun, which is said originally to have been at some distance from the sea, now almost overhangs it; and, indeed, this fine ruin must ere long fall a prey to the waves. From St. Andrews, northward to Eden Water and the river Tay, the coast presents a sandy beach, and is so liable to shift, that it is difficult to trace the changes it may have undergone. It is certain, however, that within the last century the sea has made such an impression upon the sands of Barrey, on the northern side of the Tay, that the light-houses at the entrance of that river, which were formerly erected at the southern extremity of Buttonness, have been from time to time removed about a mile and a quarter further northward, on account of the wasting and shifting of these sandy shores, and that the spot on which the outer light-house stood in the 17th century is now two or three fathoms *under water*, and is at least three quarters of a mile within *flood-mark*. These facts I state from information obligingly communicated to me by George Clark, Esq. Master of the Trinity House, Dundee, from the records of that corporation. From the Tay all the way along the coast of Forfar and Kincardine to Stonehaven, the shores exhibit rocks of secondary or newer formations, as sand-stone and breccia, &c. and here the effects of the sea are in many places very perceptible; particularly about half a mile to the westward of the town of Arbroath, where the public road bounds the sea shore, within the last 30 years, the trustees for the highways have been under the necessity of removing the road twice within the fields, and this operation it has now become again necessary to repeat, for the safety of the traveller. The shores of the estate of Seaton, in this neighbourhood, and the Earl of Northesk's estate of Aithie, including the promontory called the Redhead, exhibit the most unequivocal marks of decay from the same cause; and on a very slight inspection the continued progress of disintegration is deducible from the appearance of the shores at Montrose, the North Esk river, Johnshaven, Dunottar Castle, and the bay of Stonehaven. From thence along the shores of Aberdeen and Banffshire, with little exception, the coast consists either of extensive tracts of sand or of primitive rocks, as granite, porphyry, and serpentine. The shifting nature of the sands, which, when dry, have been blown inland, and have covered nearly the whole parish of Furvie, belonging to the Earl of Errol, necessarily prevents the effects of the sea from being so easily traced as upon the softer kinds of rocks, or on alluvial grounds; and although these rocky shores do not yield so readily to the impulse of the waves, yet even the granite itself cannot withstand the continued force of the sea, which here rolls its surges upon it, in north-easterly gales, with uninterrupted violence, all the way from the coasts of Lapland and Norway. We are not, therefore, so much surprised to find *incisions* made into the hardest rocks, exhibiting such extraordinary cavities as the Bullers of Buchan, and other striking appearances on this coast near Peterhead, as to observe its destructive effects upon the more sheltered

shores of the Frith of Forth, formerly described, or those of the Moray Frith, which we are now approaching.

After passing the river Spey, the rocks on the shores belong to the sand-stone or coal formation, and here again the wasting effects of the sea become more apparent. At the ancient town of Burghhead, an old fort or establishment of the Danes was built upon a sand-sone cliff, which, tradition says, had a very considerable tract of land beyond it; but it is now washed by the waves, and literally overhangs the sea. Between Burgh-head and Fort George, a tract of about 25 miles, the coast is one continued bank of sand, which has undergone very great changes from the blowing of extensive sand-banks, which has buried several hundred acres of the estate of Cubin, and covered many houses; nor have the ravages of the sea been less felt than those of the sand-flood in this quarter, as the old town of Findhorn was destroyed by the sea, and the site of it is now overflowed in every tide. At Fort George the encroachments of the sea are likely to produce considerable damage upon the walls of the fort, some of the projecting bastions, formerly at a distance from the sea, are now in danger of being undermined by the water; and it has been found necessary to construct a kind of *chevaux de frise*, to break the force of the waves before they reach the walls. The same remarks regarding the destructive effects of the sea are also applicable to the shores of the Frith of Dornoch, and more sheltered Frith of Cromarty, and the great basin above Fort George, and even of Loch Beauly. The coast of Caithness, the islands of Orkney, and the southern parts of Shetland, consist chiefly of sand-stone rocks, and from their great exposure to the sea, it is no wonder that they appear in many places to be rapidly wasting. In Orkney it deserves particularly to be remarked, that the Start Point of Sanday, which is now formed into an island every flood tide, was, even in the recollection of some old people still alive, one continuous tract of firm ground; but at present the channel between Sanday and the Start *Island*, as it is now called, is hardly left by the water in neap tides; and since a light-house was erected upon this Point about ten years ago, the channel appears to have worn down at least two feet. It would indeed be an endless task to enter into minutiæ regarding the waste observable upon the western coast of Scotland, including the shires of Sutherland, Ross, and Inverness, although defended from the heavier breach of the Atlantic Ocean, by the chain of islands, consisting of the Lewis, Harris, Uist, and Barra, extending about 120 miles in a north-eastern and south-western direction, and commonly called the *Long Island*, while the Argyleshire coast is sheltered by the Western Hebrides, including the great islands of Mull, Jura, and Isla; yet even in the most sheltered places of this coast, as we have seen of the friths of Forth and Moray, the sea in many places is rapidly wasting the shores. These effects, however, are not less obvious on those islands which are exposed to the direct breach of the Great Western Ocean, as, for example, in the Lewis and Uist Islands. In Uist particularly,

the sea has overrun considerable tracts of land, forming every tide extensive pools and many fordable channels. The extensive low link grounds, or downs, and all the sandy shores of these western islands, and also of Orkney and Shetland, consist almost wholly of broken or pounded shells, thrown up in the first instance by the sea, and afterwards blown by the winds upon the land.

Observations upon the wasting of the land by the encroachment of the sea might, with great propriety, be made upon the shores of Ireland, of which I have seen many instances on the western, northern, and eastern coasts, from Loch Swilly, in the county of Donegal, to the Tusker Rock, off the coast of Wexford. But without enlarging upon these shores, we shall now turn our attention to the coast of England, which, with the opposite shores of Holland and France, form the apices of the German Ocean and British Channel. From the more soft and yielding matters of which these shores are formed, particularly those of England, which are at the same time exposed to the violent attacks of the sea in storms from the north-east and south-western directions, the wasting effects of the sea are altogether so very remarkable, that it may in general be affirmed that these shores are in a progressive state of waste. Beginning with the north-eastern coast, examples of this will suggest themselves to the recollection of those who are acquainted with the shores of Northumberland, Durham, and Yorkshire, as at Holy Island, for example, and the shores near Bamborough Castle, where the sea has made considerable inroads upon the land, in the recollection of the present inhabitants of that neighbourhood. Tynemouth Castle, situated at the entrance of the river Tyne, which now in a manner overhangs the sea, had formerly a considerable extent of land beyond it; Tynemouth Head, being composed of a soft sand-stone, is gradually worn away by the action of the sea and the effects of the weather, and every season falls down in such quantities that the degradation is quite observable to the inhabitants of the town of Tynemouth. Upon the southern side of the entrance to the river Tyne, many acres of land have been washed away from the extensive ebb called the Middens; and the same has happened along the whole shores of the county of Durham, particularly between the rivers Tyne and Weir, where the coast is chiefly composed of a soft friable lime-stone; and indeed the land is obviously in a state of waste all the way to the Tees. On the southern side of the great sand-banks forming the mouth of the Tees, we enter upon the coast of Yorkshire, which extends to the estuary of the river Humber, being upwards of a hundred miles. This coast consists chiefly of sand-stone and chalk hills, and exposes a precipitous face to the sea, which is acting upon it, and in many places producing its rapid destruction. Of this many examples are familiar to those on the spot, particularly in the neighbourhood of Whitby and Scarborough. For a few miles both on the northern and southern side of Flamborough Head Light-house, the section of the coast is almost perpendicular, and consists of

chalk, intermixed with portions of clay. At the eastern extremity, or pitch of the head, the chalky cliff is about 70 feet in height. From this point the coast declines all the way to the town of Bridlington, and from thence to Dimbleington Cliff, near the entrance to the Humber, it is a low sandy shore. From what has been already stated of the effects of the sea upon the hard or more compact shores of Scotland, it is easy to imagine what its operation must be on the line of coast just described; accordingly, the inhabitants at Flamborough Head, and indeed all along the Yorkshire coast, are too often kept in mind of this by the removal of their *land-marks* and inclosures; and there are many traditions of churches, houses, and whole fields, having been overrun by the sea in the neighbourhood of Hornsea, Kilnsea, and the Spurn-point Light-houses on the northern side of the Humber. The widely extended mouth of this estuary, and the manner in which it is cumbered with sand-banks off the coast at Clea and Saltfleet, in Lincolnshire, and indeed the appearance of the coast all the way to Boston, shows that much of the land has been swallowed up or overrun by the sea; of which there are many striking proofs, both of ancient and modern occurrence.

The same remarks are also applicable to the great ebb, called the Wash, forming the entrance or navigation to the harbours of Boston and Lyne. Here it would appear that the sea has made a breach through the chalk-hills, which are observable on each side of the Wash, in the counties of Lincoln and Norfolk, where it is obvious that the land has at one time extended further into the sea, and is at present undergoing the process of actual waste. Perhaps evidence of this may also be drawn from the works of William of Malmsbury, who represents the whole of the fens of Lincoln to have been in a state of high cultivation in the *eleventh century*. But certainly a most unequivocal proof of this is afforded from the discovery of Sir Joseph Banks and Dr. Joseph Correa, mentioned in the 89th volume of the Philosophical Transactions, of the remains of a *submarine forest* on this coast, now several fathoms under water, where the roots, boles, and branches of trees, particularly of the birch, of large size, were discovered. From the account of the fishermen of this coast, these appearances are to be seen for many miles along the shore in the form of a range of small islets: and trees have been often found, the timber of which was so fresh as to be fit for economical purposes. The inhabitants of the country likewise represent that at one time the parish church stood greatly within the present sea-mark, and that the walls of houses of a former village have been seen at low ebbs; and they allege that even the *clock* of the present parish church is the same that was in the church the foundations of which are now overflowed. It seems, therefore, probable that the present state of the fen country arises from the encroachments of the sea, occasioned by the *silting* or filling up to a certain degree of the alveus or bed of the German Ocean, rather than from the gradual retreat or subsiding of the waters of the ocean;

and that the sea, notwithstanding some anomalous instances of recession which shall afterwards be noticed, is invariably trenching upon the land. In exploring and comparing the present with the ancient state of our shores, we cannot enough lament the inaccuracy of the older maps and charts of our coast; and every one must rejoice at the prospect this country has of soon possessing the Trigonometrical Survey of Great Britain, now in progress, under the direction of the Board of Ordnance. This great national work will enable future generations with accuracy to appretiate and compare the effects which we are now describing.

Proceeding southward, we next traverse the coasts of Suffolk and Essex, where numerous instances occur of ravages which the sea is making upon the shore. It has already been ascertained that the sand-banks of Yarmouth Roads have of late years considerably altered, and that the depth of water is, perhaps, upon the whole, rather lessened, and some pretty extensive additions have been made to the land at the junction of the rivers Alde and Butley, in the great gravelly beach which extends about eight or ten miles in length, varying in breadth from a few hundred feet to about a mile; and similar appearances are to be found on this coast, as at Harwich, near the confluence of the rivers Stour and Ipswich, where a considerable addition has been made to the land on the southern side of Landguardfort; yet these, and other examples of the same kind, are trifling in proportion to the astonishing effects of the sea in *destroying* the land in this very neighbourhood. Near Lowestoff, Dunwich, and Aldborough Castle, on the Suffolk coast, the sea is daily making impressions upon the land, which is apparent to the observation of every one acquainted in the slightest degree with that coast, and is at some places severely felt both by the proprietor and the tenant. At the Naze Tower, near Walton, and indeed all along the coast of Essex, the same appearances are no less obvious. Crossing the numerous sand-banks and shoals which greatly encumber the mouth of the River Thames to the Kentish coast, we are every where presented with instances of the degradation of the land by the encroachment of the sea; from Sheerness along the shore of the Isle of Sheppey, and from the entrance of the River Swale to Margate and Ramsgate, at various places, very large portions of the chalky cliffs are undermined, and giving way to the sea. At Sheppey Island, Thanet, and Sandwich, there are proofs of the land gaining somewhat upon the sea. Of this the Goodwin and other sand-banks may also be considered as examples; but these cases, arising from the shape of the coast, and the set of particular currents of the tide, are evidences of the *silting* up of the *alveus* or bed of the ocean, and shall be afterwards alluded to as so many proofs of the consequent tendency of the sea to overflow its banks. But, to continue, it may further be noticed, that the streets of Deal are often laid under water, and houses there have occasionally been washed down by the sea; and, indeed, its effects are very alarming all along this coast. At Romney Marshes, labourers are

constantly employed in attending and repairing the fences and sea-dikes of these low shores. On the precipitous shores from Deal to Dover, Folkstone, and Hythe, large portions of the chalk cliffs are frequently undermined and carried away; particularly at the South Foreland and cliffs of Dover, where I happened to witness the effects of the recent fall, some years ago, of an immense quantity of these extraordinary chalk cliffs, the ruins of which appeared to cover several acres of ground, and must have contained many thousands of tons. A fall of this kind, near Beachy Head, on the Sussex coast, is noticed in a paper by Mr. Webster in the Transactions of the Geological Society. The portion which gave way extended 300 feet in length, and was 70 or 80 feet in breadth. A clergyman who happened at the moment to be walking on the spot, observing the ground giving way, had just time to escape when the whole fell down with a dreadful crash. In the same manner, the opposite coast of France is understood to be acted upon; and the numerous islands lying off that coast and the coasts of Germany and Holland. I might also extend these observations along the shores of Hampshire, Dorset, Devon, Cornwall, particularly to the Isles of Wight and Portland, and the Scilly Islands; the wasting of the land, and the encroachment of the sea, are very remarkable, being always in proportion to the nature of the strata or rocks composing the coast, whether alluvial, chalk, lime-stone, sand-stone, or granite.

Nor are these effects of the sea confined to the shores of the German Ocean and the British Channel; for the wasting of the land is no less remarkable in St. George's Channel and the Irish Sea, including the coast of Ireland on the one side, and on the other the shores of Wales, Lancashire, Westmoreland, and the counties of Dumfries, Kirkcudbright, and Galloway, where neither the rocky coasts, and exposed situations of the Islands of Anglesea, Man, Copland, Craig of Ailsa, and the Islands of Cumbrae, nor the sheltered and alluvial shores of the Bristol Channel, are exempted; even the indentations of the coast at Dublin Bay, Liverpool, and Lancaster, and the more extensive friths of the Solway and the Clyde, are subject to the unvarying destructive effects of the sea upon the land. Without further examples, however, we may for the present venture to assume that the disintegrating and wearing effects of the waters of the ocean are *general.* Whether we contemplate its effects upon the land by the immediate and powerful impulse of the waves at the base of a rocky shore, or, with the elegant and profound illustrator of the Huttonian theory, trace it in the form of rain, rills, and torrents, in the higher regions, we shall find its effects all tending to one unvarying principle, producing the degradation of the land, and consequent tendency to filling up at the bottom of the sea; while, at the same time, from the magnitude and extent of the surface, and other occult causes, we are not aware of the elevation of its level in any sensible degree. That Almighty Being who hath said, 'Hitherto shalt thou come,

and no further,' has, with infinite wisdom, created, if I may so express it, a kind of compensating power to counterbalance the seeming conflict of the elements of earth and water; for while the ocean appears to be extending its surface, it seems also probable that the quantity of its waters upon the whole is lessened; that part of them undergoes a complete and permanent change of form after the process of evaporation; and that the earthy particles continually accumulating at the bottom of the sea, have a direct tendency not only to preserve a uniform level, but even in some instances to make the water overrun what we have been accustomed to consider its boundary. If we attentively inquire into the *generality* of the wasting effects of the sea upon the margin of the land, it will perhaps appear that the commonly received opinion of the sea just taking a portion of the land from one part, while it adds in like proportion to another part of the coast, will be found to come far short of the instances of detrition in all quarters of the globe.

Having now pointed out, from actual observation on about one-half of the coast of Ireland, and on all parts of the shores of Great Britain, from the Scilly Islands, its southern extremity, to the Naze of Unst, or northernmost point of Shetland, that the land, on the margin of the sheltered bays and friths of our coast, as well as on the most exposed promontories and open shores, is undergoing the process of waste and decay from the impulse and action of the sea, I shall in a future paper, with the indulgence of the Society, endeavour to show that the cause of this effect, particularly on the shores of the German Ocean and British Channel, is, in a good measure, owing to the immense quantity of debris which must be accumulating, at least to a certain depth, in the bottom of the ocean.*

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## Article III.

*A simple Theory of Electricity and Galvanism: being an Attempt to prove that the Subjects of the former are the mere Oxygen and Azote of the Air, and the Subjects of the latter the mere Oxygen and Hydrogen of Water.* By Alexander Walker.

(To Dr. Thomson.)

SIR,

In your observations on atmospherical electricity in the fourth volume of your System of Chemistry, it is justly observed, that

* This paper has been circulated with a view of obtaining additional facts regarding the wasting of the shores of Great Britain and those of the opposite Continent; and more especially to procure intelligence respecting the numerous examples of the formation of new land and banks under water from the deposition of gravel, sand, and alluvial matters, at the mouths of rivers, in bays and creeks along the shores, or in the open sea. Communications upon this subject, prior to the month of November, 1816, are requested to be transmitted to Mr. Stevenson at Edinburgh; or 6, Suffolk-street, Charing Cross, London.

galvanic phenomena demonstrate a much closer connexion between chemistry and electricity than has hitherto been suspected. Dr. Wollaston, also, notices a strong point of analogy between galvanism and electricity; namely, that they both seem to depend on oxidation. The difference, however, in their mode of excitement, and the much greater permanence of galvanic than of electric effects, would seem to indicate that there is yet an essential difference between them. Now as there is no galvanic action without water, nor any electric action without air, and as both these bodies contain oxygen, while each presents also another and a different ingredient, capable apparently of producing the modifications which subsist between the natural agents of which I now speak, it would, at first sight, seem not unreasonable to suspect that, universally diffused as are these powers, galvanism was dependent on the liquid and electricity on the fluid which throughout nature are the most universally prevalent. This latter conclusion I accordingly stated above five years ago, perhaps too hastily, and on data not altogether sufficient.

With accumulated data, however, and rather more accurate knowledge of physical science, I was two years ago induced, with regard to electricity, to assert not only the original opinion of Dufay that there are two distinct electrical fluids, but that these are neither more nor less than the oxygen and azote of the common atmosphere unchanged and unalloyed in their nature; and, with regard to galvanism, that there are also two distinct fluids, which are precisely the oxygen and hydrogen of water, equally unchanged and unalloyed.

This I shall here endeavour, if not to prove, at least to render probable; premising merely, that I can by no means agree with the anonymous writer who, in the number of the *Annals* for June, says, that the galvanic fluids consist of "a large portion of caloric, and two distinct and highly attenuated *bases*, that *partake* of an oxygenous and hydrogenous nature;" "that these newly discovered compounds are formed by *a very different union* with caloric from that which takes place in the formation of oxygen and hydrogen gases, as we have not the least evidence that the *greatest possible attenuation* we could obtain by the application of heat to these gases would impart to them any electrical energies;" that they are "highly attenuated *compound bodies*," which "hold a place between well known gaseous bodies and caloric;" that "they seem *a link between* well known gaseous bodies and caloric, by partaking of the constitutional character of the one, and the action and subtle nature of the other;" and that it is not improbable that "both the electric and galvanic fluids will, at some advanced period of these sciences, be considered merely as a *newly discovered class* of PECULIAR gaseous bodies."—Nothing has so effectually retarded the progress of science as the universal search for truth in complex causes and circumstances instead of simple ones; and we may always be certain that such "participations," "very different

unions," "greatest possible attenuations," "compoundnesses of bodies," and "linkings between them," when applied to the grandest and simplest powers in nature, indicate rather the state of our own minds than of the laws of Him, the noblest and most admirable characteristic of whose power is the extreme simplicity of its means of operation. I shall endeavour, then, to show that the electric and galvanic fluids are neither a *newly discovered*, nor a *peculiar class* of gaseous bodies, and still less a newly discovered class of *peculiar gaseous bodies*, as is there asserted.

1. *The Oxygen and Azote of the Atmosphere are the only Electrical Agents.*

This is rendered probable by the following considerations:—

That electrical action cannot pervade a perfect vacuum;

That even in rarified air electrical attraction is weakened;

That the electric light becomes more concentrated and more dense in proportion to the condensation of the air;

That when points are throwing off or receiving electricity, a current of air always seems to proceed from them, whether they are positively or negatively electrical;

That an excited electric or a conductor strongly electrified gives to any part of the body when presented to it such a sensation as if wind were blown upon it;

That electricity accelerates a stream of water running through a minute aperture;—that it drives water in a stream through a capillary tube from which without the aid of electricity the water would not even drop;—and that it produces various similar effects easily explicable by the same hypothesis.

2. *In Electrical Operations the Atmospheric, Oxygenous, and Azotic Particles, are respectively received by corresponding superficial Cavities of Bodies, and by their Friction are separated from each other.*

This is rendered probable by the following considerations:—

That the quantity of electricity produced depends greatly on the superficial extent of the electrical bodies;

That a slight alteration of surface, of pressure (which affects surface), or of temperature (which equally affects it), will dispose a body to acquire one electricity rather than another;

That a considerable degree of heat, which thus affects the surfaces of bodies, renders electrics conductors;

That as surfaces alone seem to be thus concerned in electricity, it seems to be the change of surfaces which causes the change in the kind of electricity, when different rubbers are used;

That as the quantity of electricity produced depends on the superficial extent of bodies, so does that of electricity conducted;

That in this respect the solid contents of a conductor are unimportant, since it conveys nearly or quite as much electricity if hollow as if solid.

That as electricity thus depends on the surfaces of bodies, and not on their solid contents, it must depend on that circumstance in which the former differ from the latter;

That the surface of a body (which is engaged in electrical action) can differ from its solid contents (which are unengaged in it) only in this respect, that the cavities between its particles are patent, and hence while the former are incapable, the latter are capable of receiving the particles of the atmospheric fluid;

That the superficial cavities of bodies are of different form and magnitude, while the particles of atmospheric oxygen and azote are also of different magnitude* at least; and that, therefore, some surfaces must be better adapted to receive the former, and others the latter;

That if a stratum of air be compressed between two such surfaces, the oxygenous particles must adhere to that which presents similar superficial cavities, and the azotic particles to that which presents superficial cavities corresponding to them;

That the friction of two surfaces thus compressed is precisely that which would produce a divulsion of the particles adhering to them;

That in perfect consistency with this, if two similar substances be rubbed together, the smaller or the rougher assumes one species of electricity, and the larger and smoother another; and that thus are identified the operation which I have just described, and the phenomenon of electrical excitement;

Moreover, the passage of the electric particles along the surfaces of bodies is sensible even to feeling; for the electricity which passes from an excited electric or a conductor strongly electrified gives to the face when directed upon it a sensation as if air were creeping along it, or as if a spider's web were drawn over it.

3. *This forced Separation of the Aerial Particles, and their tendency to re-unite, best illustrate the most surprising Effects of Electricity.*

One of the most surprising effects of electricity, and most striking circumstances in which it differs from galvanism, is, that of the great portion of air which its spark percurs. This, however, is easily explicable, if, as I have endeavoured to prove, the two species of electricity be merely the oxygen and azote of the air unchanged and unalloyed. For, in this case, it is not necessary to suppose that the same particles which enter at any part pass through

* The admirable labours of Dalton, Berzelius, and Thomson, throw great light on this important subject. That the magnitude as well as the weight of the particles of oxygen is different from that of hydrogen I am convinced even by the general consideration that, while many metaphysical and physical arguments might be adduced to prove that matter is one—the πρώτη ὕλη, so well described by the Greeks, it seems evident that, as throughout nature a difference of weight in bodies of equal magnitude is caused by the greater or smaller number of their internal vacuities, and as there can be no such vacuities in the simple particles of matter,—therefore the different weight of the simple particles of oxygen and hydrogen must depend on their different magnitude.

the whole intermediate column of air. It is on the contrary probable that a complete dislocation takes place of all the intermediate oxygenous particles in one direction, and of all the intermediate azotic particles in another; and that thus the velocity of the spark or shock far exceeds the velocity of the individual particles.

As some force or pressure is necessary to produce this dislocation of the aerial particles, it is in perfect consistency with the preceding views, that the greater the accumulation of electricity, the larger may be the intermediate column displaced. As, moreover, the same quantity of electricity becomes relatively greater the minuter the points from which it proceeds and on which it strikes, it is obvious why the sharper these points are, the greater is the extent of this dislocation and the course of the spark. It is also obvious why the blunter the points are around which a great quantity of electricity is accumulated, the broader is the column of air displaced, and the greater the light and the noise produced.

Now were not the component parts of air and the electric fluids precisely the same, without change or alloy, it is evident that this interchange of the oxygen and azote of the two points for that of the air, and the dislocation which ensues could not occur.

In applying this very simple theory to the explanation of lightning, it is only necessary to suppose that while the atmospheric oxygen predominates in one region of the air and the azote in another, these oppositely constituted masses of air are, by some of the numerous events to which the air is liable, brought more nearly into contact. In proportion, then, to the magnitude of these masses will be the extent of the dislocated column of air, and hence springs the velocity of these phenomena.

By this theory are the less effects of electricity easily explicable, as well as its greater ones. Thus two pith balls, when similarly electrified, separate in order to obtain from the air some portion of the opposite gas to that with which they are loaded; while oppositely electrified balls unite in order to combine their gases, and hence they are neutralized, their electricity disappears, and no further result ensues, because they thus only restore to the atmospheric air a portion which had been separated from it.

But thus to explain all the phenomena of electricity would be to compose a system of the science. Suffice it that I have shown that if the two species of electricity were either more or less than the oxygen and azote of the air, its phenomena would be less easily explicable.

### 4. *The Oxygen and Hydrogen of Water are the only Galvanic Agents.*

This is rendered probable by the following considerations:—

That as electricity is in no instance produced without air, so is galvanism in no instance produced without water;

That water is decomposed during the operation;

That the gases which are liberated from the galvanic battery are

of the same nature—oxygen and hydrogen, and in the same proportion as the water decomposed.

5. *In Galvanic Operations the Aqueous, Oxygenous, and Hydrogenous Particles, are respectively carried to the opposite Extremities of the Battery, and conducted along its Wires.*

This is rendered probable by the following considerations:—

That to galvanic action it is indispensable that the bodies employed should have some chemical action upon each other;

That the galvanic action is proportionate to the degree of chemical action;

That the voltaic battery ceases to act the moment the chemical action between its parts is exhausted;

That while these circumstances prove galvanic phenomena to be dependent on chemical action, that action is primarily the oxidation of one of the metals employed; for it is in proportion to the oxidation of the zinc in the battery during a given time, that galvanic power is produced, and in proportion as the liquid loses its power, and oxidation ceases, that galvanic action declines;

That whenever an oxidating influence is exerted at one of the metals, a deoxidating one is produced at the other; for when iron, which oxidates rapidly when forming a circle with silver and common water, is arranged with zinc and common water, it remains perfectly unaltered, whilst the zinc is rapidly acted upon; and such also is the case with zinc and copper;

That during this operation the water in the battery is decomposed;

That it is, therefore, its oxygen which oxidates, and its hydrogen which deoxidates, the zinc and copper respectively;

That its oxygen is attracted to what is called the positive, and its hydrogen to the negative, side of the battery;

That if a wire proceed from the positive, and another from the negative, end of a battery, and be approximated in a tube of distilled water, then a stream of oxygen proceeds from the surface of the wire connected with the positive, and a stream of hydrogen from the surface of the wire connected with the negative, end of the battery;

That this oxygen and hydrogen, when collected in two small and distinct tubes, are in the proportions required to recompose the water by combustion.

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The preceding chain of circumstances seems to me clearly to indicate that the water which may thus be recomposed, is precisely the water which was decomposed in the battery; and it will certainly be a strong confirmation of this indication if the mode in which the oxygen of the water is attracted toward one extremity, and the hydrogen toward the other, and if also the opposite currents which they thus form, can be satisfactorily explained.

Now, it is obvious that, in each compartment of a galvanic trough

in which is plunged a plate of zinc and of copper, the oxygen of the decomposed water is attracted by the zinc, while the hydrogen, repelled by it, is attracted by the copper. We also know that these galvanic fluids are readily conducted by metallic substances, and also by the surface of water. Indeed it is in this latter respect observed, that water is a more powerful conductor of galvanism than mercury, while it is rather the reverse with regard to electricity. Hence it would appear that while the oxygen of the water is attracted by the zinc, and the hydrogen by the copper, both are conducted by these metals to that portion of them which connects the contiguous plates of two neighbouring compartments, and to the surface of the water between the two plates of the same compartment. Now the aqueous surface of each compartment, and the metallic junctions of contiguous ones, thus form one general conducting surface along which the liberated oxygen and hydrogen might obviously creep in opposite currents if there existed but a power which should give each an impulse in the opposite direction.

That there is such a cause, I think a little consideration will show.—It must be observed that in the first compartment of the positive extremity a plate of zinc only is plunged. This will abstract from the water of that compartment a portion of its oxygen which, partially liberated and ascending the plate, will, as rapidly as formed, escape by the wire which is attached to it. It is evident, then, that there will be in the water of this compartment rather a superabundance of hydrogen than of oxygen; and as in the second compartment, in which is plunged a plate both of copper and of zinc, both oxygen and hydrogen will be liberated and ascend to its surface, it is obvious that, connected as this compartment is with the first, there will instantly ensue an attempt at an equalization in the distribution of the liberated fluids. Now as it is the oxygen of the first compartment which is deficient, it is of course the oxygen of the second which will be attracted thither, while its hydrogen will be repelled in the opposite direction. But the oxygen of the second compartment, thus conducted away, will now also be defective; and thither consequently will be attracted the oxygen of the third, while its hydrogen also is still oppositely repelled. And so on will successively be affected the fourth, fifth, and all the remaining compartments. Each in succession will yield up a portion of its oxygen to that which is nearer to the positive extremity of the battery whence that fluid is escaping, and each will successively have repelled from it a portion of its hydrogen toward the negative extremity of the battery whence that fluid escapes by the opposite wire. Thus, then, will be formed a current in each opposite direction; and yet these currents, instead of impeding, must each accelerate the other, as appears from the preceding statement.

Thus, I believe, is obviated the chief difficulty attending this explanation; and there seems no reason to doubt that the water which may be recomposed at the extremities of the wires is precisely that which is thus decomposed in the battery.

*6. This Decomposition and Recomposition of Water best illustrate the Effects of Galvanism.*

One, perhaps, of the most striking circumstances in which galvanism differs from electricity is in the much smaller portion of air which its spark percurs. Now as the extensive course of the electric spark is, as I have shown, ascribable to the homogeneity of the electric fluids and the constituent parts of common air—to their being nothing else than the oxygen and azote of the air unchanged and unalloyed—and to the dislocation of all the intermediate oxygenous particles in one direction, and of all the intermediate azotic particles in another:—as this is the case, and as there is no such homogeneity between the galvanic fluids and the parts of common air—as they are the oxygen and hydrogen of water, and therefore cannot correspond to and rest on the particles of the intermediate aerial column—and as consequently no dislocation and interchange of its particles can occur, it is obvious why the galvanic spark is not seen to percur so great a portion of air as the electric.

The preceding considerations also show why it is that oxygen and acids seem to be attracted to the positive wire, while hydrogen, alkalies, and metals, are attracted to the negative wire.—The accumulation, however, of illustrations would ill suit the extent which can be permitted to this paper. It is enough that I have shown that if the two species of galvanism were either more or less than the oxygen and hydrogen of water, its phenomena would be less easily explicable.

Thus, Sir, have I endeavoured to explain the nature of that connexion to which you have alluded as existing between chemistry on the one hand, and electricity and galvanism on the other, and to show that that connexion is of the simplest and most obvious kind; the two species of electricity being the mere oxygen and azote of the air, mechanically divided, and unchanged and unalloyed in their nature; and the two species of galvanism being the mere oxygen and hydrogen of water, chemically separated, and equally unchanged and unalloyed.

I am, Sir, with great respect,

Your obedient servant,

*October*, 1815. ALEXANDER WALKER.

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## ARTICLE IV.

*Cursory Observations on the Nature and Treatment of Remittent Fever.* By H. Robertson, M.D. Col. Med. et Acad. Ion. S.

THE remittent fever is that form of pyrexiæ that resembles both intermittent and continued fever, and is found to originate from the impression of a peculiar exhalation arising from stagnant water im-

pregnated with the decaying remains of animal and vegetable substances, and consequently is not in the first place a contagious or infectious disease, or one that can be communicated at a distance from the source of its exciting cause, however severely and epidemically it may prevail in certain situations.

The more distinct the remissions, and the more nearly remittent fever approaches to the form of intermittent, so much more favourable are its symptoms. On the contrary, the more nearly it resembles continued fever, so in proportion are its consequences to be dreaded. In the south of Europe, this is its most general form; where the only appearance of its peculiar nature is to be perceived from the exacerbation of the fever that commonly approaches a few hours after mid-day, and the obscure remission that takes place as the fever declines after midnight as the morning advances. This exacerbation is found to recur at quotidian, tertian, and quartan periods. The last form I have always found accompanying the severest attacks. The pulse is rarely found hard in remittent fever, though commonly full, strong, and rapid; but in some instances I have found it small, and not greatly exceeding its natural motion. Remittent fever is always ushered in by cold shivering, which the patients call a fit of the ague; but the successive stages of the fit are occasionally found, even from the first, to be irregular, and the one not in succession to the other, as in common ague. After this cold fit the patients sometimes are able to walk about for a short time, but never beyond the evening of the third day from the first attack, when the disease again comes on in its decided type. In many cases the remission is so complete in the first days of the fever as to give an idea that the disease has terminated; but in every such instance I have always found it the precursor of the most violent subsequent disease: however, relapses are most rare in this disease, though I have also frequently met with people who have suffered two, and even three attacks, during the same season. Convalescence is in many cases very tedious; especially in those where the disease has run on some days before attended to, or where from other causes a determination to any particular organ still exists, though in a less degree than during the access of the disease. From this cause vertigo, head-ach, slight suffusion of the tunica albuginea, and tremors, are not unfrequently the sequelæ of remittent fever. Sometimes the disease is accompanied by diarrhœa; and in such cases I have always found the fever milder. Costiveness is not a common occurrence in this fever, the bowels being generally regular; the tongue is always foul, often parched from the first, with pain at the scrobiculus cordis, and sometimes nausea, with great thirst. But the head is the organ that primarily and particularly suffers in this disease, by pain, giddiness on changing posture, epiphora, sneezing, beating of the carotids, and frequently a sense of heaviness, with a degree of stupor, though at the same time there are a continual desire of changing posture, sighing, and seemingly great anxiety.

At the same time I have to observe that the most dangerous cases of this fever which I have seen were those who did not complain of head-ach, but in whom the giddiness was great, with a small quick pulse, and cold extremities. The duration of remittent fever may probably be justly said to be about seven days. No doubt there are cases much exceeding that period, and others that do not even reach it. This, however, is the medium of its duration, according to my experience. But I have met with nothing like a crisis in this fever; and any appearances of putridity, as petechiæ, or glandular abscesses, are no less uncommon. I have frequently seen patients who have been recovering from remittent fever become affected with regular fits of ague.

During the prevalence of remittent fever every acute disorder seems to partake of the nature of that disease; we find, therefore, many anomalous cases, that, although admitting of the same treatment, are not to be classed under this variety of disease. In this way catarrhs, with head-ach, epiphora, &c. in the first coming on, attended with a quick pulse, diarrhœas, especially such as occur in young and delicate subjects, in like manner yield to the same plan of cure that is found successful in the treatment of remittent fever. This affection of the bowels is almost always imputed to something improper in the food, to teething, or to some other not very obvious cause; but in proof that the fever in such patients is not owing to the first of those causes, we very well know that the febrile symptoms and diarrhœa remain even after all offending matters have been evacuated. We have also daily experience to show that no permanent febrile disease is ever excited by this cause; and we have only to look into any anatomical work, and examine the evolution of the teeth in the process of dentition, to be confirmed that disease from this cause must be a rare occurrence. I have accordingly always found that whenever remittent fever is a common disease, catarrhs, diarrhœas, inflammation of the lungs and abdomen are frequently met with; and in one case I saw the disease alternate with a severe pain of the femur.

As to the nature of the disease in question, I have no hesitation in giving my opinion of it as a fever of a highly phlogistic diathesis; an opinion that I conceive to be confirmed, not only by the symptoms of the disease in its whole course, but also by an inspection of the bodies of those who have died of this fever. Moreover, in its most aggravated forms, I consider remittent fever in this point of view, and that the symptoms generally taken for those of debility can be much more satisfactorily explained by referring them to the excessive stimulant powers of the exciting cause of the disease, producing thereby irregularity in the nervous functions; and that even remittent fever partakes of the phlogistic diathesis in its protracted state, when the phenomena that commonly supervene in these circumstances evidently arise from the excessive action of the sanguiferous system of the brain. It is to the excessive action of the heart and arteries, superadded to the previously stimulated state of

the brain from the exciting cause, that I imagine the sense of dullness or heaviness at the commencement is to be imputed, and which proceeds to coma as the disease increases. This is a point most essential to be ascertained in practice; as upon the correctness of our judgment here a most important part of the cure depends. In the view which I have taken of the nature of remittent fever, we may at once see the impropriety of the stimulant plan of treatment, so far as relates to the use of internal means, whether or not these be in the shape of diet, or medicines. The torpid state of the functions in almost every advanced case of remittent fever seems to be owing to an accumulation of serum in the ventricles of the brain, produced in consequence of a great determination of blood towards that organ, as explained above. In these circumstances stimulants must increase the affection; and I do not recollect having seen even a single instance of such treatment prove successful, while on the other hand the brain being thus considerably compressed, a general application of the evacuating plan may not be admissible; our resource in that case, therefore, is more in local remedies, at the same time keeping the tension of the vascular system in a certain state of vigour by means of mild diet, &c.

It has been long observed that the natives of any place are much less liable to be affected with the diseases peculiar to the situation than strangers, or those newly arrived; and it may likewise be added, that when they are attacked with any endemic affection, it is rarely so severe as it is found to prevail among strangers, and those not accustomed to the climate. This I have had good opportunities of seeing confirmed in the South of Europe, in the case of remittent fever, which rarely affects the natives so severely as it does our own countrymen, and the natives of northern climates, although in every respect using the same diet, and following the same manner of life. I speak of the Spaniards, the Portuguese, Italians, &c. employed in our service. This I have heard frequently imputed to our being less temperate than these; and unquestionably habitual intemperance does afford a most unfavourable disposition to every inflammatory affection; but in this instance I do not consider the observation sufficiently explained by that remark, even were it always correct; besides, I have frequently observed the most temperate of the military labouring under the most severe attacks; though this class are not so generally affected, or so difficultly recovered, as those whose irregularities are greater. Certainly much depends in this matter upon our own peculiar manners and habits. Though the natives of the north of Europe are not reckoned so irritable as those nearer the equator, unquestionably, however, the effect of any impression communicated to these is much more lasting than in the others; therefore a given impulse on the nervous system being more permanent, must become more powerful in its consequences; for, as I have already remarked, it seems to be from the accelerated action of the sanguiferous system increasing the effects of the original impression upon the brain, that the severity and

danger of the disease is most frequently owing. Remittent fever is, therefore, according to the view that I have taken of it, a disease whose characteristic symptoms during its whole course, however protracted, depend entirely upon the influence of the same cause, or the consequences which this produces on the system in its immediate impression; the principal phenomenon of the disease in either state depending on an increased action of the vessels of the brain.

But although I do not admit that there are different stages of this fever, as those of debility, putridity, &c., there is not a doubt but that there are different degrees of the disease in point of severity—from that of simple intermittent to that of the most violent continued fever.

Yellowness is a very common occurrence in this disease; and although I do think that it is almost always the consequence of a violent attack, I do not consider the yellowness itself as adding to the danger of the complaint. In such dissections as I have seen of those who died with this appearance in remittent fever, in no case have I ever discovered any peculiar affection of the liver, so as to be able to account for it. May not the yellowness, therefore, be in consequence of the increased action both of the arterial and absorbent vessels of that organ, whereby the increased bilious secretion which must thereby ensue is more rapidly taken up and conveyed to the circulation? There is one observation which I have made that seems to support this idea; namely, that the yellowness most generally comes on rapidly, and in the earliest period of the fever, and in cases where the affection of the head is most remarkable, though there are, no doubt, exceptions to this observation; but I have rarely seen it come on when the pulse is small or weak; neither do I think that it entirely depends on the season, or state of the weather, although it certainly does appear oftener when the season is hottest; and consequently the more southerly the climate, so much the more frequently is it met with; circumstances which tend, I imagine, to confirm the opinion that I have suggested of its cause. But to whatever circumstance or cause this may be owing, I have found that while the fever lasts, and during its abatement, the yellowness gradually wears off; but as soon as the disease terminates, the yellowness suffers but little diminution for a considerable time; and, in many cases, it is not till long after the patient has regained his usual health that the skin becomes of the natural hue. I have tried a variety of medicines to effectuate this purpose, but without being able to point out any that fully answers our intentions. I believe the nitric acid, with infusion of quassia, and occasional doses of calomel, to be as good as any.

The different and opposite modes of practice for the cure of remittent fever originate, of course, from the opposite opinions which are entertained of the nature of the cause of that disease in its effects upon the system. Those who maintain that this cause operates upon the system as a sedative, or power destroying the energy

of the function upon which it immediately acts, consider all the appearances which are met with on dissection of those who have died of the fever as supporting this opinion. These appearances are most commonly found to be a distended state of the venous vessels of the brain, and a serous effusion into the cavities of that organ. No doubt other organs are occasionally found to have been affected, especially in such as have previously laboured under any visceral affection.

The supporters of the above-mentioned opinion of the cause of remittent fever imagine that it more immediately affects the nervous system by its sedative power, the brain being the principal origin of this system. The distention of its blood-vessels found on dissection proceeds from the remora in the propulsion of the contents of the veins in consequence of a proportionate debility in these vessels themselves; but, in objecting to this opinion, we must observe, that not only are the larger trunks found in a state of distention, but there is always found the appearance of the minute vessels being affected also in the same manner, and in a much greater degree than what is met with except in those who have laboured under an inflamed state of the brain, and to which they bear every resemblance; a fact that indicates, I imagine, a very opposite state of the organ to that which I can suppose to be the effects of the operation of a sedative power; for admitting for an instant that such powers or properties do exist in certain bodies in their effects upon the functions, I apprehend that, in proportion to the sedative impression, a diminished energy in the blood-vessels must be the consequence, and unless the impression is instantaneous, and equally powerful upon the whole cerebral functions, thereby, as it were, arresting suddenly the fluids in the vessels, that instead of a turgid state of the vessels of that organ, we should rather expect to find it unusually pale, and the blood-vessels in a collapsed state; for the progress of the disease shows that the influence of the noxious agent is comparatively gradual in its action; and consequently, even upon the supposition that the remote cause is of a sedative nature, the blood would readily find its course by some other route, gradually as the debility of the vessels supervenes. The quantity of blood sent to the head must always be in proportion to the power of the blood-vessels to propel it. Moreover, dissection does not show that the arteries of the brain are in a distended state, which undoubtedly must have been the case if this appearance is the consequence of disease originating from the impression of a sedative power, as in that way both sets of vessels must have been equally affected.

But, laying aside these arguments, it is the duty of those who maintain the doctrine of sedatives to show, either by direct or analogous facts, that such powers do exist. I am not aware that any can be adduced of the first; and those generally supposed to be of the second class seem to militate considerably against that doctrine. In the first place, the circumstances of the inoculation either of the plague, small-pox, or any other febrile disease, do not warrant any

such inference of the nature of the matter producing these diseases. The same may be said of the poison of venomous animals, electricity, carbonic acid gas, all the mineral poisons, alcohol, and vegetable substances called narcotics; which, although in certain proportions they instantly destroy animation, nevertheless, in less quantities, or in a diluted state, they are known to possess the most decided stimulant properties; and I cannot imagine that the same matters should possess properties so decidedly different, however modified, or in whatever circumstances they are communicated to the living body.

Without, therefore, entering more minutely into an investigation of this subject, I shall briefly state my opinion of the proximate cause of remittent fever, which will readily afford an explanation of the method that I have followed in its treatment.

I consider that the miasma producing remittent fever acts upon the nervous system as a stimulant, the principal effects of which are manifested upon the brain, to which it gradually communicates its peculiar impression through the medium of the circulation; that the irregularity that appears in the nervous functions in the course of the disease arises from the violence of the power of the exciting cause. Hence originates the irregularity and increased energy of the circulating powers; that of course this excitement will appear greatest in the organ most immediately and principally affected, viz. the brain, or in such viscera as are connected with that organ in a healthy state by sympathy, as the stomach, the liver, &c. or in such organs as may have been previously affected with disease. In this way I imagine that the foul tongue, nausea, and other affections of the stomach and bowels, met with in all degrees of remittent are secondary symptoms, and depend entirely upon the primary impression of the exciting cause on the brain. Moreover, that from the moment the miasma or exciting cause is imbibed into the system, I consider its effects to commence, though gradually developing itself according to the virulence of the exciting cause, the idiosyncrasy, and habits, of the patient; but from the first instant that its effects become evident, in a greater or lesser degree, and not after a lapse of so many days, weeks, or months, does it explode, according to modern phraseology on this subject.

But although I consider the matter producing remittent to be essentially the same, I believe, nevertheless, that a more aggravated form of disease is occasioned by a more concentrated state of that poison. Hence the different degrees of severity of remittent fever at different periods of the year, and in different climates, which seem to form a gradation of disease in the following scale: intermittent, catarrhus attended with epiphora, sternutatio, &c., remittent, yellow fever. And from a recent occurrence in this island (Corfu), independently of what is to be inferred from the writings of Sydenham, Russel, Diemerbroeck, and Alpinus, it would appear that the plague itself originates from the same cause. See also Heberden's observations. It was long ago remarked by

Sydenham that erysipelas bore a great resemblance to the plague; and from the frequency of scarlatina in every climate where remittent fever is an endemic disease, it is also to be presumed that there exists, at least, a very great similarity in the causes of these affections.

This being my opinion of the effect of the remote cause of remittent fever, the plan of cure in all its varieties will be very easily understood. As we have no other mode of abating the energy of the nervous system than by diminishing the vigour of the different functions, we most effectually answer the intention by diminishing the force of the circulating fluids, causing a more equal distribution of them by derivation, and carefully avoiding every thing that tends to excite the energy of the sanguiferous system, either by the articles of diet, medicine, or regimen.

I am convinced that the practice of blood-letting in the cure of remittent fever has frequently fallen into disrepute, not only from the injudicious way it has been had recourse to, but more particularly from its failure in giving relief, in consequence, I imagine, of the simultaneous administration of other remedies, or modes of practice tending to frustrate its advantages. One of the most obvious consequences of blood-letting, either in the healthy or diseased state, is an increase of the sensibility and irritability of the system. From this cause we find that less doses of medicines are required to procure certain effects after that operation than before it, and consequently that whatever tends even slightly to irritate or stimulate the system when in health, is in these circumstances often followed by the most violent effects.

Mercury, which is a stimulant, and one that is more permanent in its effects than any other known, is, therefore, in my opinion, decidedly improper in every febrile affection; and I speak from experience when I say that it is most pernicious in the treatment of remittent fever. If blood is taken away, therefore, at the same time that mercury is administered to the patient, we cannot wonder that the good effects of the first operation are defeated, or rather that it is thereby rendered prejudicial to the patient; as thereby the stimulant effects of the mercury become more forcible, which, added to the already morbidly increased irritability of the minute vessels from the effects of the exciting cause, must often, if not always, aggravate the disease. The mercurial or stimulant treatment is, therefore, a much safer practice when exhibited without any previous evacuation of blood. In this case some of the patients, by the strength of constitution, may overcome both the effects of the original exciting cause, and also the irritation produced by the mercury: whereas in the mixed practice few escape, except in slighter cases of the original affection. In proof of this I have only to adduce the proportion of mortality to the recoveries in those cases of epidemia that have occurred in these climates of late years; a mortality equally fatal to that of any plague on record, being very recently as three to five who recovered.

Calomel is the only remedy of this class that is used; and in whatever way it is given, or with whatever substances it may be mixed, even with antimonial medicines, when it acts as a purgative, it leaves such an impression upon the extreme vessels increasing their irritability, that I have never seen the use of it persisted in for two successive days, in severe cases of disease, that it was not followed by a fatal event, or tedious recovery, and in these too it seemed to give origin to visceral obstructions, and other untoward consequences. Indeed, upon any rational principle of cure, there cannot be any propriety in using such medicines at the commencement of remittent, in however small doses they may be given, or upon whatever function they are supposed to operate; for even when calomel acts as a purgative, there is a sufficient quantity of it always absorbed, which very speedily produces an impression that defeats the good effects of its more obvious operation. See an important paper on this subject in the Annual Register for 1802, vol. xliv. p. 814. In that paper the merits of the different modes of practice are impartially stated, it having been drawn up from the observations of a Gentleman not of the medical profession. And here it appears that the success of the plan, which I have proposed, is much greater than that of the mercurial practice; as by the above-mentioned paper the number who died under the mercurial treatment was 75 to 79; on the other hand, 56 who were treated by blood-letting speedily recovered.

The same reasoning in regard to calomel is, in a certain degree, applicable to the use of antimony in the commencement of this fever; for although its stimulant powers are not so permanent as those of calomel, and it is, therefore, less objectionable, nevertheless until the energy of the circulation is somewhat abated by blood-letting, saline purgatives, and other parts of the antiphlogistic treatment, it is not an advisable remedy; but after the tension of the sanguiferous system is diminished, I have found antimony to be a most useful auxiliary in the cure of remittent fever, but given in much less quantities, and at longer intervals than what are usually recommended. I rarely exceed one or two grains of antimonial powder for a dose, and this repeated every hour for four, five, or six times, drinking lemonade or barley water during its use. In this way it commonly affects the bowels, and at the same time produces a moisture on the skin, which has the effect almost always to relieve the patient; but whenever it occasions severe nausea or vomiting, I have always found it hurtful, and am, therefore, careful in preventing either of these occurrences, by withholding the further use of the antimony when these are likely to be produced by it. The preparations of antimony which I prefer are the antimonial powder and the tartrate of antimony, the last always in solution rarely exceeding $\frac{1}{10}$ of a grain for a dose. Nausea and vomiting, as determining to the head, and increasing the irritability of the stomach, are peculiarly hurtful in this fever; therefore emetics are, except under peculiar circumstances, never to be used. The affection of the

stomach, being in almost every case a secondary affection, generally disappears as the head becomes relieved.

Cold bathing, or rather the dashing of cold water over the patient, I have always found at best a doubtful remedy, and often a dangerous attempt, particularly in the commencement of remittent fever, by its effect in determining to the head. Spunging with tepid water I have often seen usefully employed in every period of the disease. From what has been already advanced on this subject, it may naturally be expected that stimulants of every kind must be hurtful in every period of the disease; and I have accordingly found that internal stimulants are pernicious at every period of remittent fever; even bark, and in weak decoction, is scarcely excepted from this remark; for this medicine never seemed to me to be of the smallest utility till the fever had put on the decided intermittent form; whenever it was given previously in substance, it evidently did harm.

Sudorifics, as coming within the class of stimulants, are not advisable in the early stages of the fever; though, as the disease advances, when this process can be effected without forcibly increasing the action of the minute vessels, it is productive of great benefit, by relieving the head. Upon the same principle, small blisters on the neck, head, and ancles, or synapisms to the soles, are often advantageously had recourse to. But here I have to observe, that I have frequently found the good effects of blistering defeated by the general irritation produced from their being too large; for all medicines of this class do good only by the local irritation which they occasion.

Upon the whole, I have found the remittent fever most successfully treated by a free depletion at the commencement, and the gradual adoption of the stimulants we have in the articles of diet towards the termination, with occasional remedies for particular symptoms during the course of it; and I venture to pronounce this practice favourable, on finding that the proportion of fatal cases to those of recovery is not more than 1 to 24, including even those who from previous disease, and inveterate bad habits, could not be expected to recover from any febrile attack, and that too in an epidemic of great malignity.

Purging by mild neutral salts, or infusion of senna, pulp of cassia, &c. is extremely beneficial at the commencement of remittent fever. These should be frequently repeated, so as to keep up a diarrhœa for a day or two; and for this intention, when the bowels are thus disposed, small doses of pulv. antim. are found sufficient to keep up the affection; for, next to blood-letting, no class of medicines are so useful here as cathartics.

I do not recommend blood-letting in the commencement of remittent fever, from the idea of counteracting the actual inflammation of any particular organ, though this does also frequently occur; but it may be no less usefully employed to counteract the effects of a determination towards some viscus, which, although not in what is called a state of inflammation, is in a condition very nearly ap-

proaching to it, and which, if not corrected, proves equally fatal to existence. This I conceive to be the most general condition of the organ principally affected in remittent fever.

It is astonishing to see how very soon patients recover their usual strength after the loss of several pounds of blood for the cure of this disease. Indeed, I am convinced, both from private and hospital practice, that my patients got soonest well who had been most profusely evacuated at the commencement of the disorder. I am of opinion, therefore, that the dread that is commonly apprehended on account of the debility that is supposed to succeed such practice is merely imaginary, very opposite effects often being the immediate consequence of that operation. I remember having a patient (Serjeant Dogherty, of the 10th Foot), who by accident (the bursting of the temporal artery) lost at once at least 8 lb. of blood, besides the evacuations in the course of the disease, and he was able to return to his duty in about ten days. A circumstance nearly similar happened to a soldier of the 20th Dragoons, with, in a certain degree, a similar result. In protracted cases of remittent fever it is often puzzling to determine on the propriety of blood-letting. In such cases I have always found that this practice may be determined upon by supporting the patients erect in bed, and tying up both arms in the same manner as when the common operation of phlebotomy is to be performed, carefully avoiding the compression of the arteries. If upon this the head is declared to be relieved, or the patient otherwise expresses himself better, blood may be taken away with advantage.

Upon this principle I have taken blood from patients with great advantage whose pulse was extremely weak at the wrist, but always in these circumstances it rises after the operation. In this state cupping glasses applied to the occiput, or temples, leeches, and local stimulants, as blisters, together with diaphoretics, assist in the recovery of the patient.

The above is an outline of the appearances of remittent fever, as it has fallen under my observation in different parts of the Mediterranean, and in different seasons, together with the method which I have found successful in the treatment of it. I offer these for consideration, not from the idea that they are altogether new, but in the hope that they may assist in leading to a more certain mode of curing the most frequent, and, as I apprehend, in its varieties, the most dreadful disease that afflicts mankind in every climate.

*Corfu, Dec.* 24, 1815.

## Article V.

*A Comparison of the Old and New Theories respecting the Nature of Oxymuriatic Acid, to enable us to judge which of the two deserves the Preference.* By Jacob Berzelius, M.D. Professor of Medicine and Pharmacy, and Fellow of the Royal Academy of Sciences at Stockholm.

(*Continued from* vol. vii. p. 441.)

8. *Chlorine forms, with Hydrogen, Muriatic Acid; and with Oxygen, Chloric Acid. It is, therefore, analogous to Sulphur, which forms Acids with Hydrogen and Oxygen.*

It appears to me always strange when the new doctrine has recourse to analogies, because it seems to have laid it down as a duty not to lay any stress upon them. However, we shall examine this analogy, so favourable to the new doctrine, more closely.

Sulphur, tellurium, phosphorus, charcoal, arsenic, unite with hydrogen, and form peculiar bodies, which all have a certain analogy with each other. The first two, it is true, possess the characters of acids, which the rest want. But these acid properties do not in the least diminish the general analogy which subsists among these compounds of hydrogen. Sulphur, arsenic, phosphorus, charcoal, and tellurium, combine likewise with oxygen, and form acids, which have a marked analogy with each other, as well in their outward characters as in their chemical properties. I ask now with which of these classes of bodies has muriatic acid an analogy? Hardly a chemist would be found (the supporters of the new doctrine not excepted) who would hesitate a moment to place muriatic acid along with the acids that contain oxygen, their properties agree so well together, namely, their acidity, their taste, their smell, their effect when concentrated in corroding and blackening animal and vegetable bodies, the degrees of their affinity, &c. Nor could any reason be assigned, if we except the necessity of the new doctrine, for classing muriatic acid with sulphureted hydrogen or tellureted hydrogen rather than with sulphuric acid or phosphoric acid.

Even the weakest acids containing oxygen rank before the hydrogen acids, and this is owing to the nature of the case. But muriatic acid differs so much from the others, that it is capable of separating most of the other oxygen acids from their combinations, even those which are stronger than itself. This is contrary to every analogy with sulphureted hydrogen and tellureted hydrogen, and even difficultly accords with our ideas respecting acidity. Can it, then, be alleged with reason that the explanations of the new doctrine agree with the other parts of chemical theory, while the old doctrine is inconsistent with them? And has not the old doctrine

in its accordance with the rest of chemical science a manifest advantage?

The only thing which can here be urged against the old doctrine is the necessity of supposing the existence of a hyper-oxidated acid. This opinion, indeed, is not improbable; but we have no example of it among undisputed acids. But whoever considers this as a reason for adopting the new doctrine would be disposed to deny the possibility of every thing not yet discovered. Anhydrous sulphuric acid has only been lately discovered; who, then, can be sure that hereafter we may not form a combination of it with oxygen?

According to both doctrines, muriatic acid is a very strong acid. This means, according to the usual acceptation of words, that muriatic acid has a stronger affinity as an acid, or that it adheres to bases with more energy than other acids. It follows, as a consequence from this, that the compounds of muriatic acid are more difficult to decompose than the same bases united to other acids. But it is easy to see whether, according to the new doctrine, this be the case or not.

When a portion of concentrated sulphuric, nitric, muriatic, or phosphoric acid, acts upon the tongue or the skin, the taste and the feeling inform us that their action is analogous, and the same. Their action on vegetable colours and vegetable bodies has also the greatest resemblance. If we combine these acids with ammonia, we perceive again a completely analogous action. The acid characters disappear, and are replaced by a saline taste, because aqueous sal-ammoniac, which both doctrines recognize as a salt, is formed. If we unite these acids with potash or oxide of lead, we can detect nothing which is inconsistent with their analogous action; saline bodies are formed, possessing the usual characters of salts of potash or oxide of lead. The old doctrine considers muriate of potash as a salt, just as nitrate or sulphate of potash, neither of which possesses any chemically combined water. But the new doctrine takes quite a different view of the matter from the old. As the same product which is obtained by saturating muriatic acid with potash is procured likewise by uniting chlorine and potassium; and as the latter, when dissolved in water, and obtained again by spontaneous evaporation, upon being dried has the same weight as at first, it can neither contain potash nor muriatic acid, nor can it be a salt, but must be a chloride of potassium. The increase of temperature which takes place when muriatic acid is mixed with a ley of caustic potash, cannot, therefore, have the same cause with the heat produced by mixing diluted sulphuric acid or nitric acid with potash ley, but must proceed from this cause, that the hydrogen of the muriatic acid forms water with the oxygen of the potash, while chloride of potassium is composed. The saline taste of the chloride of potassium can give no information respecting this body, because analogy cannot be admitted as a proof. Quite the same explanation is given of chloride of lead, though it possesses all the characters of a salt of lead, namely, a sweet taste, is easily blackened by sul-

phureted hydrogen, &c. It is not, therefore, considered as an acid, nor does it contain oxygen. Chloride of potash and chloride of lead, therefore, are not salts; they are rather analogous to oxides or metallic sulphurets. I now ask, can any man hold out such opinions as correct chemical philosophy? And is this *the just logic of chemistry*, which the chemists of the new doctrine have been under the necessity of adopting?

The new doctrine shows itself, the more closely it is examined, the less consistent with the other parts of chemical theory, and therefore the less probable. I shall, on that account, dwell a little on the chloride of potassium. It is clear that chlorine must have a strong affinity for potassium. It is likewise known, by experiment, that chlorine has a stronger affinity for hydrogen than for oxygen, and potassium a stronger affinity for oxygen than for hydrogen. Hence in no chemical argument can it be denied that muriatic acid as an acid must have a strong affinity to potash as a basis. From all these well-grounded and sufficiently established suppositions, it must follow, as a necessary consequence, that when chloride of potassium is dissolved in water, all these affinities (that of chlorine for hydrogen, of potassium for oxygen, and of muriatic acid for potash) must act, the water must be decomposed with an increase of temperature, and muriate of potash formed.

But if we believe the supporters of the new doctrine, none of all these things takes place. It follows, therefore, that either our suppositions, or the opinion that chlorine is a simple substance, is false. But the premises founded on the new doctrine of affinity cannot be entirely without foundation, as is evident from this, that when sulphuret of potassium and telluret of potassium are put in contact with water, the liquid is decomposed with an increase of temperature, hydro-sulphuret of potash and hydro-telluret of potash being formed, which may be obtained in a dry state by evaporating them in vessels from which oxygen gas is excluded. They are not again decomposed by exposure to a red heat; at least this is not the case with hydro-sulphuret of potash; so that by that process no sulphuret of potassium is formed.

We will allow a supporter of the new doctrine to affirm that whenever chlorine of potassium is dissolved in water, muriate of potash is always formed, but which by the act of crystallization is again converted into chloride of potassium. As chlorine has a stronger affinity for hydrogen than for oxygen, and as potassium decomposes water with an elevation of temperature, it is obvious that when hydrogen combines with chlorine, and oxygen with potassium, an elevation of temperature must be produced. If we take a quantity of chloride of potassium, 4 oz. for example, and, putting into it the bulb of a thermometer, moisten it with water at the same temperature, and stir the moistened mass with the thermometer, we shall perceive the mercury in the instrument suddenly to sink. It appears, then, that a diminution of temperature, instead of an elevation, takes place, just as when the same experiment is

made with nitrate of potash. Chloride of potassium, then, combines with no water chemically, the liquid acts merely as a solvent, and cold is produced, because the particles of the chloride are separated further from each other by the interposition of the water. Now as in this case merely a solution takes place, without any evidence of chemical combinations, I do not think it consistent with the principles of sound chemical philosophy to draw conclusions contrary to what we perceive by our senses, assisted by philosophical instruments.

Thus the asserters of the new opinion always come upon consequences which are inconsistent with the common theory of chemistry; and, therefore, either that theory or the new doctrine respecting the nature of muriatic acid must be inaccurate. But we shall go on with our proofs.

If into a hot concentrated solution of muriate of potash we pour an acid somewhat weaker than muriatic acid, as phosphoric or arsenic acid, the liquid immediately gives evidence of the presence of disengaged muriatic acid. In this case, therefore, according to the new doctrine, a weaker acid has produced a decomposition of water, by means of which the potassium has left the chlorine to combine with oxygen, for which it has a weaker affinity, and form a phosphate or an arseniate. In this case the chlorine exhibits a very singular appearance. It forms a stronger acid at the moment when it is separated from its combination with the base; and although this new acid cannot combine with the potash, it is yet capable of preventing the complete saturation of the phosphoric acid with the potash. Now I ask again is this chemical logic?

It cannot be alleged that the acid properties of muriatic acid are only apparent, and proceed from its reducing power, and its uniting with chlorine. How could this agree with the greater affinity of hydrogen to chlorine than to oxygen, in consequence of which this acid must be less reducing than perhaps any other hydrogen acid. Phosphureted and arsenical hydrogen have a greater reducing power than sulphureted hydrogen, yet they are not acids.

While, therefore, the new doctrine, when compared with the whole of chemistry as a theoretical science, is inconclusive, and inconsistent with itself, all the phenomena, when viewed according to the old doctrine, are simple, consistent, and more than probable. This doctrine assuming that muriatic acid gas, like common sulphuric acid, is a compound of anhydrous acid with a quantity of water, which answers the purpose of a base. This anhydrous acid can unite with the different salifiable bases, and form salts capable of existing either with or without chemically combined water. Therefore muriate of potash as well as nitrate of potash, and muriate of lead as well as nitrate of lead, are salts.

### 9. *Considerations on neutral, simple, and triple Muriates.*

The embarrassment into which the supporters of the new doctrine are thrown when they speak of muriates may provoke a smile. They

neither agree with each other, nor with themselves, about what to consider as muriates, what chlorides. In reality they are unable to point out with accuracy any other muriates except those which cannot be obtained free from water, as muriate of ammonia, muriate of magnesia, muriate of alumina, and some other combinations of muriatic acid with earths and oxides which lose their acid when exposed to heat. That sal-ammoniac is a compound of muriatic acid and ammonia is evident from this, that it cannot be obtained free from water, any more than any other neutral simple ammoniacal salt. When oxymuriatic acid gas is mixed with ammoniacal gas, the requisite quantity of water is obtained by the decomposition of part of the ammonia at the expense of the oxymuriatic acid. When ammoniacal gas is mixed with muriatic acid gas, the salt absorbs all the water in the muriatic acid gas which becomes the water of crystallization of the sal-ammoniac. According to the new doctrine, the chlorine gas decomposes the ammoniacal gas in consequence of the affinity of chlorine for hydrogen, and the muriatic acid formed combines with the undecomposed ammonia, and forms anhydrous sal-ammoniac. If this be true, a very extraordinary anomaly occurs; muriate of ammonia, fluate of ammonia, and hydriodate of ammonia, are the only ammoniacal salts which contain no water; while all other neutral compounds of undisputed acids with ammonia contain water of crystallization, and cannot be obtained without it.

An English chemist, who considered the old doctrine as preferable to the new, endeavoured to demonstrate the inaccuracy of the latter in this manner. Dry ammoniacal gas was mixed with dry muriatic acid gas, and the neutral salt thus obtained gently heated. Water was obtained, which he considered as the former basis of the muriatic acid. But such a result agrees as little with the one doctrine as the other. It is easy to conceive, whatever opinion we adopt, that the water obtained might be owing to moisture which had unavoidably obtruded itself. This supporter of the old doctrine showed by this attempt that he was not quite master of the case. His opinions were opposed by some pretty eminent supporters of the new doctrine, and they served, as might have been expected, only to spread the new doctrine wider.

Muriates of magnesia, alumina, zirconia, &c. are likewise considered as true muriates, because they all, when heated, give out muriatic acid, and the earth remains behind in a state of purity. It would have been too absurd to have supposed that the bases of the acid and of the earth were acidified and basified at the instant of decomposition. Of these, muriate of magnesia alone can be considered as a chloride of magnesium. It is formed when chlorine is passed over red-hot magnesia, while at the same time the oxygen of the earth is disengaged. Water instantly decomposes this compound, and converts it into muriate of magnesia, which cannot be converted into chloride of magnesium by evaporation.

Here we ask why the muriates are confined to a small number, and to feeble bases, and why none of these, except magnesia, forms

a chloride? Nothing is more natural than to expect a difference between the chlorides and muriates analogous to that which exists between sulphuret of potassium and hydro-sulphuret of potash; for the change of the metal into an oxide, and of the chlorine into an acid, must surely produce some notable alteration in the physical characters. So far from this being the case, we can perceive no other difference between the bodies called chlorides and muriates, according to the new doctrine, than what exists between salts containing, and salts destitute of, water of crystallization. Every chloride exhibits the properties of a salt, as well as the muriates; and the chloride of potassium bears the same relation to muriate of alumina as the sulphate of potash does to the sulphate of alumina. It is quite evident, then, when we take a general view of the subject, that the chlorides and muriates belong to the same genus of bodies, and of consequence that they are all salts, just as much as the sulphates, nitrates, &c. Now this is the doctrine which the old opinion has so long taught.

Several muriates have the property of combining with each other, and forming triple salts. Thus sal-ammoniac combines with muriate of platinum, muriate of copper, &c. The first of these triple salts contains just as much water as belongs to the sal-ammoniac present. This compound, therefore, according to the new doctrine, cannot be a triple salt, but a peculiar compound of sal-ammoniac (without water) and chloride of platinum. The triple salt, composed of sal-ammoniac and muriate of copper, contains more water of crystallization; and, therefore, according to the new doctrine, is a real triple salt or muriate. Now I ask which of the doctrines is most correct; the old, which considers these salts as triple salts, with more or less water of crystallization; or the new, which considers the one as a triple salt, and the other as a compound of a peculiar nature, without assigning any other reason for the difference than the necessity of the doctrine?

Some anhydrous muriates have the property of uniting with ammonia, and forming triple subsalts, which retain the ammonia with so much the greater force the weaker the basis of the muriate is. This is the case with anhydrous muriate of lime and permuriate of tin (*spiritus Libavii*). Different sulphates have the same property. Sulphate of copper, for example, both combined with water and anhydrous, absorbs ammoniacal gas, assumes a blue colour, and is converted into a triple subsalt. When pulverized sulphuret of tin is mixed with 20 times its weight of oxide of mercury, and all the mercury is distilled over; or when tin is distilled to dryness with sulphuric acid, and heated to redness, anhydrous sulphate of tin is obtained, which absorbs ammonia. The muriates agree in this respect with the sulphates. But the new doctrine is very far from perceiving this agreement. According to it, chloride of potassium and chloride of tin are acids of a peculiar kind, in which the metal is the base, while the chlorine acts the part of oxygen. These acids

are capable only of uniting with ammonia, with which they form peculiar ammoniacal salts. Such extravagant hypotheses, founded entirely upon the necessities of the new doctrine, should have long ago, methinks, have raised some distrust of its accuracy; and I must suppose that those who have supported that doctrine have not been aware of the facts which I have here stated.

### 10. *Submuriates.*

Muriatic acid forms subsalts with various bases, in which, for the most part, the acid is combined with four times as much base as in the neutral salt, just as in subsulphates the acid is usually combined with three times as much base as in the neutral salts. Most, if not all, the submuriates contain water of crystallization, which is separated from the salt by a gentle heat. The new doctrine considers the subsalts containing water as true submuriates; but if the water be driven off by heat, the residuum is considered as a compound of one particle of chloride with three particles of oxide; so that, according to the new doctrine, the chloride can combine with oxides, and likewise with carbonates and muriates. But if the new doctrine should be brought to deny the existence of muriatic acid in submuriates (which is very possible in a theory in which, without any regard to the properties, all the explanations are deduced from the theory), in that case these bodies would be considered as combinations of a chloride and a hydrous oxide.

But what way soever these combinations are viewed they must always obey the laws of chemical mixtures. In this point of view I shall examine *submuriate of lead* and *submuriate of copper*, both containing water. According to the analyses of these salts, which I consider as pretty accurate, they are so composed that the muriatic acid is combined with four times as much base as in the neutral salts, and the metallic oxide, and chemically combined water, contain each the same proportion of oxygen. In the following tables of these combinations I take for the basis of the weight of each atom the grounds which I have stated in my treatise On the Cause of Chemical Proportions; namely, the weight of oxygen, $O = 100$; base of muriatic acid, $M = 139{\cdot}56$; lead, $Pb = 2597{\cdot}4$; copper, $Cu = 806{\cdot}45$; and hydrogen, $H = 6{\cdot}636$. Muriatic acid, oxide of lead, and oxide of copper, contain each two atoms of oxygen and one atom of metal. Water, on the contrary, contains two atoms of base and one atom of oxygen. According to the new doctrine, the weight of an atom of chlorine, $Ch = 439{\cdot}56$ (that is, $M + 3\,O$ of the old doctrine), and an atom of muriatic acid ($Ch + 2\,H$), is $452{\cdot}82$ (according to the old doctrine, $M\,O^2 + H^2\,O$).

### A.—SUBMURIATE OF LEAD.

*Old Doctrine.*

| | | | | |
|---|---|---|---|---|
| Muriatic acid | M | = | 139·56 | = 339·56 |
| | 2 O | = | 200·00 | |
| Oxide of lead | 2 Pb | = | 5194·80 | = 5594·80 |
| | 4 O | = | 400·00 | |
| Water | 8 H | = | 53·08 | = 453·08 |
| | 4 O | = | 400·00 | |
| | | | | 6387·44 |

*New Doctrine, α.*

| | | | | |
|---|---|---|---|---|
| Muriatic acid | Ch | = | 439·56 | = 452·83 |
| | 2 H | = | 13·27 | |
| Oxide of lead | 2 Pb | = | 5194·80 | = 5594·80 |
| | 4 O | = | 400·00 | |
| Water | 6 H | = | 39·81 | = 339·81 |
| | 3 O | = | 300·00 | |
| | | | | 6387·44 |

*New Doctrine, β.*

| | | | | |
|---|---|---|---|---|
| Chlorine | | | | = 439·56 |
| Lead | $\frac{1}{2}$Pb | | | = 1298·70 |
| Oxide of lead | $1\frac{1}{2}$Pb | = | 3896·10 | = 4196·10 |
| | 3 O | = | 300·00 | |
| Water | 8 H | = | 53·08 | = 453·08 |
| | 4 O | = | 400·00 | |
| | | | | 6387·44 |

### B.—SUBMURIATE OF COPPER.

*Old Doctrine.*

| | | | | |
|---|---|---|---|---|
| Muriatic acid | M | = | 139·56 | = 339·56 |
| | 2 O | = | 200·00 | |
| Oxide of copper | 2 Cu | = | 1612·90 | = 2012·90 |
| | 4 O | = | 400·00 | |
| Water | 8 H | = | 53·08 | = 453·08 |
| | 4 O | = | 400·00 | |
| | | | | 2805·54 |

*New Doctrine, α.*

| | | | | |
|---|---|---|---|---|
| Muriatic acid | Ch | = | 439·56 | = 452·83 |
| | 2 H | = | 13·27 | |
| Oxide of copper | 2 Cu | = | 1612·90 | = 2012·90 |
| | 4 O | = | 400·00 | |
| Water | 6 H | = | 39·81 | = 339·81 |
| | 3 O | = | 300·00 | |
| | | | | 2805·54 |

*New Doctrine, β.*

| | | | | |
|---|---|---|---|---|
| Chlorine | | | = | 439·560 |
| Copper | $\frac{1}{2}$Cu | | = | 403·225 |
| Oxide of copper | $1\frac{1}{2}$Cu | = 1209·675 | = | 1509·675 |
| | 3 O | = 300·000 | | |
| Water | 8 H | = 53·080 | = | 453·080 |
| | 4 O | = 400·000 | | |
| | | | | 2805·54 |

If we compare these tables with each other, we perceive that both doctrines correspond so far with the laws of chemical proportions that each body is composed of a whole number of elementary atoms; for it is obvious that if both examples under β be doubted the broken number of atoms disappears.

But I have endeavoured to show that this is not the only circumstance which concerns the doctrine of chemical proportions, and that if this law alone constituted the whole doctrine, it would be scarcely possible to establish its truth; for too great a number of combinations would be possible, and the difference of the properties of the substances by the increase of the number of single atoms would be too small to be accurately determined by analysis. But experiment shows that the difference between possible combinations of two oxides is very great, as we see is the case here between the neutral and submuriates and sulphates. This difference must have a cause—a cause which is explained by a second law which exists between the combinations of oxidized bodies. This law is as follows:—*In a combination of two or more oxidized bodies the oxygen in one of the oxides is a multiple by a whole number of the oxygen in that oxide which contains the smallest quantity of oxygen.* I believe that the great number of experiments and analyses which I have performed on the different combinations of oxidized bodies, in order to discover and establish this law, prove that the agreement of experiment with the rule is not an accidental circumstance, but demonstrates the universality and accuracy of the law. The combinations of the salts under consideration agree, according to the old doctrine, with this law, as is obvious from the preceding tables; for even if the oxygen of the muriatic acid be considered as problematical, still the proportions of oxygen in the metallic oxides and water, concerning which there is no dispute, are equal, and therefore correspondent with the law.

But the contrary takes place when we follow the new doctrine. The metallic oxide either contains 400 parts, and the water 300 parts, of oxygen; or the oxide 300 parts, and the water 400 parts. But 400 is not a multiple by a whole number of 300; and chlorine, when we consider it as a simple substance, cannot contain any oxygen. It follows, therefore, that the combinations of the aqueous submuriates, when viewed according to the new doctrine, do not agree with the law which regulates the combinations of oxidized

bodies. Therefore either the new doctrine or that law must be erroneous.

The supporter of the new doctrine, therefore, in order to avoid the complete refutation of his opinions, must affirm that this law is inaccurate. But its inaccuracy can be demonstrated no other way than by producing examples of combinations of oxides, the proportions of whose oxygen is known, and in which the quantity of oxygen in the one is not a multiple by a whole number of that in the other. That no such example has hitherto been produced by any supporter of the new doctrine, it is scarcely necessary to mention; and my experiments on azote and ammonia deserve, I conceive, so much confidence, that no nitrate in which the azote is considered as a simple substance can be brought forward as an example.

To state the matter as Sir Humphry Davy has done when he says—"Professor Berzelius has lately adduced some arguments, which he conceives are in favour of chlorine being a compound of oxygen from the laws of definite proportions; but I cannot regard these arguments as possessing any weight. By transferring the definite proportions of oxygen to the metals, which he has given to chlorine, the explanation becomes a simple expression of facts; "*and there is no general canon with respect to the multiples of the proportions in which different bodies combine.*" This I call attempting to decide the question by superiority of reputation. But, while I acknowledge superiority with respect, I shall never cease to oppose it with the force of scientific arguments.

From what has been stated, it is obvious that the new doctrine respecting the nature of muriatic acid is inconsistent with the law respecting the combination of oxidized bodies; that both cannot at the same time be true. I leave it to the supporters of the new doctrine to point out the inaccuracy of the above stated law, observing only in passing that whoever embraces both must not venture to hope for the reputation of a chemical philosopher.

*(To be continued.)*

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## Article VI.

### *Sketch of Mr. Howard's New Process for refining Sugar.* By Thomas Thomson, M.D. F.R.S.

I consider this process as by far the greatest improvement which has been made in sugar-refining since it began to be practised in this country. It will enable the manufacturer to produce a greater quantity of loaf sugar, and of better quality, from raw sugar, than has hitherto been done. It will, therefore, ultimately reduce the

price of that article. I shall here merely give such a general view of the processes as will be intelligible to the chemical reader, without entering into the minutiæ, which can only be learned by inspecting the manufactory itself, and by reading the specifications of Mr. Howard's two patents, both of which have been published.

1. The raw sugar is mixed with a little water, and heated on a flat copper steam-bath. It is then put into clay pots, and the treacle allowed to run off. To wash it out more completely, a saturated syrup is poured upon the sugar in the clay pots. The treacle thus separated amounts to about 10 lb. for every 1 cwt. of sugar. Common sugar-bakers separate about 30 lb. from the same quantity.

2. The sugar thus freed from treacle is dissolved in water by means of steam, having been previously mixed with a quantity of finings. The finings used are composed of alum dissolved in water, and mixed with as much quick-lime as will completely saturate the excess of acid in the alum, and no more; so that the white powder obtained shall produce no change in the colour of paper stained with turmeric. Two pounds of alum are employed for every 1 cwt. of sugar.

3. The solution of sugar, while still hot, is let into a filtering vessel, in order to separate the impurities by filtration. When the syrup passes into the filtering vessel, it is black and opake. But after filtration it is transparent, and of an amber colour. The filter is one of the most ingenious parts of the manufactory. It consists of strong Russia canvas fixed firmly on a thin copper frame, with holes at the bottom. There are 50 of these in the filtering vessel, because rapid filtration is necessary.

4. From the filtering vessel the syrup passes into the boilers, where it is to be boiled down to the requisite degree of concentration. In the ordinary way of boiling syrup, the temperature is so high that a considerable portion of the sugar is converted into treacle. Mr. Howard's vessels are globular, and of copper, and connected with an air-pump, which is wrought during the whole time that the boiling goes on. The consequence is, that a vacuum is formed within the boilers. This enables the boiling to take place at temperatures so low, that there is no risk of destroying any of the sugar. The vacuum is such as to support a column of mercury from one inch to four inches in height. There is a thermometer attached to each boiler, and likewise a mercurial guage, to give the degree of rarefaction. From inspecting these two, and comparing them, it is easy to know when the boiling has been carried far enough. But each boiler is provided with a very ingenious contrivance for taking out samples, in order to judge, in the usual way, by the viscidity of the syrup, whether it be sufficiently concentrated.

5. The concentrated syrup is let down into an open copper vessel in order to granulate. This is accomplished by raising its temperature by means of steam to 180°, and then letting it cool to 150°.

6. From the granulating vessel it is poured into clay moulds of the usual size for loaves. When cold, the liquid syrup is allowed to run out, and afterwards saturated syrup is poured upon the bottom of the loaf. By this means almost the whole of the yellow-coloured syrup is driven out of the loaf. A little only remains at the apex of the loaf, which is left longer than usual on purpose. This portion is cut off by turning the point of the loaf upon a kind of lathe invented for the purpose. The loaf is then fit for sale.

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## Article VII.

*A Plan for an invariable Standard of Measure, under the same Parallel of Latitude.* By Col. Beaufoy.

(To Dr. Thomson.)

MY DEAR SIR, *Bushey Heath, July* 22, 1816.

The equalization of weights and measures engaging at present the attention of Parliament; and the most certain method for determining the length of any given measure being yet undecided, I beg to trouble you with a scheme which perhaps may have the merit of inducing others to improve on the idea. Let a horizontal wheel 200 inches in circumference be divided into inches, tenths, and half-tenths of inches. This wheel makes one revolution in a second, each division representing the $\frac{5}{100}$ part of a second; and by means of a vernier similar to those used in reading off the divisions in barometers constructed for measuring the altitudes of mountains; each division of $\frac{5}{100}$ parts may be reduced to the small quantity of 1000; consequently the 1000th part of a second may be measured by this revolving wheel. Let the axis of this wheel measure rather more than 16 feet from the upper side of the wheel; at which distance, perpendicular to the axis, and parallel to the plane, of the wheel, let an arm project, to which fix a knife, the total length of the arm and knife somewhat exceeding the radius of the wheel; and the edge of the knife must be adjusted by a plummet to be exactly over zero; slightly touching the wheel over zero, place a small hammer, with a pencil or point fixed in it, for the purpose of making a mark on the wheel. Immediately over zero suspend a ball by a fine wire attached to the frame of the machine; the under side of the ball should hang exactly 16 feet above the hammer on the wheel. The wheel, by revolving, will cause the knife to cut the wire; the ball falling on the hammer, the pencil or point will mark on the wheel the number of divisions that have passed from the commencement of the ball's falling; and by the vernier may be ascertained the time the ball falls the 16000th part of one foot, on the supposition that the length of one foot is the object in view.

That the agitation of the air caused by the arm may not affect the ball, it is suspended in a cylindrical cup with a hole perforated through the top for the wire to pass; and, the knife fixed, the cup will divide the wire, and the ball fall unaffected by the wind. The barometer and the thermometer should be at a certain height when these experiments are made, that the resistance of the air to the falling body may be always the same. The precise velocity of the wheel may be accurately measured by making the arm strike a bell every revolution, and increasing or decreasing the velocity of the wheel by adding or diminishing the weights that give the rotatory motion to the wheel until the striking of the bell coincides with the beat of a clock whose pendulum vibrates seconds; and they can be made to agree within the 50th part of a second; for by attending to the beating of two clocks in my observatory, one of which kept siderial, and the other mean time, and noting the minute and second when they coincided in beat, and then waiting until a second time the two clocks beat together, and calculating from the known rate of the two clocks what was the real space of time elapsed between the first and second coincidence of beat, I found the ear would determine to the 50th part of a second. Therefore if the wheel makes 20 revolutions, the error of one revolution will amount to no more than the 1000th part of a second. If the wheel be long in acquiring a uniform velocity, let four pieces of thin metal be placed under the lower side of the wheel, which, by resisting the air, will sooner produce a uniform motion. When the motion is ascertained to be regular, the wire suspending the ball, that before was kept clear, must be brought in contact with the edge of the knife, by making the wire and the cylindrical cup, by means of a mill-headed screw, slide in a groove projecting from the framework of the apparatus; the screw will render the sliding motion smooth and regular. This machine shows the length the ball falls; 16 feet. The wheel revolving once in a second shows the time in parts of a second the ball takes in descending, which in the same parallel of latitude will not differ sensibly. It is evident if the ball took one second to descend, it would mark on zero; but, if less time, the mark will be made at some intermediate division; suppose at 199·2 inches. Then as 200 inches : 1″ :: 199·2 inches : 0·996, the time the ball took in falling. Consequently the distance a ball of certain dimensions falls in such a time is an invariable standard of measure. The wheel may be larger or smaller, and the ball hung at any height.

The simplest method of giving motion to the wheel is by winding two strings in contrary directions round the axis, and passing them over two pulleys, and then attaching the weights to give the requisite velocity; but as the motive powers will be increased by the unwinding of the strings, two other strings of the same size as those wound round the axis must be hung beneath the weights, of sufficient length to touch the ground when the weights are wound up.

These strings, by shortening in the same proportion as the others lengthen, prevent any increase of weight and acceleration of the wheel.

I remain, my dear Sir, very truly yours,

MARK BEAUFOY.

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## ARTICLE VIII.

*xplosion of Coal Gas in a Ship.* By J. H. H. Holmes, Esq.

(To Dr. Thomson.)

DEAR SIR, *Bishopwearmouth, Aug.* 4, 1816.

IT is a singular, and rather a melancholy circumstance, that this neighbourhood should be so frequently furnished with catastrophes from the explosion of coal gas; sometimes occurring in coal-mines, and carrying destruction before it; and at other times breaking out where the phenomenon could not be anticipated, and where precaution is inactive, because danger is not supposed to exist.

In your *Annals*, vol. viii. p. 72, an interesting account is given by Dr. Pemberton, of this place, relative to an explosion which occurred on board a brigantine lying in Sunderland harbour; and it was very justly presumed that it was occasioned by the light of a candle coming in contact with a quantity of foul air confined between the decks of the vessel, owing to the hatches being closely fastened down, and covered with a tarpaulin.

A circumstance so singular not having happened for a great length of time, it was not readily attributed to the mere effect of hydrogen gas, but was supposed by many to have been occasioned by the accidental explosion of some gunpowder.

The learned Doctor, however, seems to have been very correct in his ideas upon the subject, and to have attributed it to the right cause, as having arisen from the inflammation and explosion of carbureted hydrogen, in combination with atmospheric air. From some conversation I have had with the owner of that brig, it appears that the surface of the coals between the main deck and the cable tier under the forecastle were much burnt, or charred. This must have arisen from the gas burning merely under the effect of inflammation, until it reached the cable tier, where a larger quantity was accumulated, and mixed with atmospheric air; that it was produced by the ignition of foul air cannot now be disputed, as, I am sorry to say, a more distressing accident of a similar nature has just occurred.

About eleven o'clock last night the brig Flora, 188 tons, belonging to —— Davison, Esq. blew up nearly in the same situation as the former vessel, and laden with coals from Nesham's Main. She

had taken in her lading, battened down her hatches, and was just preparing to weigh for sea, with the tide then nearly at the flow. A boy having occasion to go below, the fore or skuttle hatch was opened, which must have let out the gas confined below to some extent, for another boy coming along the deck to the fore part of the vessel with a lighted candle in his hand, observed a yellowish circle playing round the flame for some time before he reached the fore chains; in a few seconds the phenomenon increased, and he immediately threw the candle overboard.

It does not appear that ignition was communicated to the foul air by this light, though it probably would have been had the boy proceeded any further. The most probable source from whence the explosion originated is from a decomposition of the pyrites contained in the coal, and which frequently produces spontaneous fire. The boy who was below says that a blue flame ran upwards from the coal, in a serpentine direction, immediately before the explosion took place.

I went on board this morning, with young Mr. Davison, nephew to the owner, and examined the injury sustained through this extraordinary circumstance. At the time it happened, there were only three boys forward, the remainder of the crew being abaft. The boy who carried the light was driven a considerable distance, and very much hurt; another, who stood on the forecastle, was blown overboard; and the boy who went below was much bruised, though less hurt than either of the others. All the deck above the cable tier is completely blown to pieces; three of the beams sprung, and one broke; the windlass, which is extremely heavy, was driven from its position, the bits tore up, and some of the blocks in the rigging split completely in two by splinters of plank. The windlass must have been forced three feet upwards, as the bolts were completely drawn, and the pauls were found quite erect, with the nail heads of the ironwork forced into the cross bar above. Two anchors lying on each side, starboard and larboard, of the forecastle, were blown overboard, and some pieces of plank drifted through the rigging over the stern of the vessel. The report was heard to a great distance; and many of the inhabitants imagined it to proceed from the shock of an earthquake. It will readily be seen how seriously these accidents are fraught with danger, and how soon a ship and her crew might be consigned to the waves, should an explosion occur when at sea in a gale of wind. It is, therefore, extremely desirable that explosions in this way should be remedied.

The Nesham Main coals are brought directly from the pit, by means of a waggon way, and are consequently but little exposed to the air compared with others which come down the river in keels; and when the main deck between the after ladder and the fore hatch is completely filled with coals, there can be no ventilating current whatever pass along between the decks; consequently the gas is left quietly to accumulate in the vacant parts, and particularly where the cable is coiled. This I think might be remedied by laying

a box or air trough of about 10 inches square on the deck under the coals, and carrying it from the after hatch, so as to open in the fore part of the vessel, where no coals are laid. The natural warmth produced by the gaseous quality of the coal would in some measure rarify this trough; and by this means a current of atmospheric air would pass through, and, by having the fore hatch occasionally lifted up, would drive off the foul air. The danger would also be obviated, if the hatches were left open (in dry weather) for a few days after the ships are laden, as by that time the moisture produced by decomposition would evaporate, and being supplied with a vent, the carbureted hydrogen would, from its specific lightness, ascend into the atmosphere. I consider this as a great proof of the superior quality of the coals from Nesham's Main; at the same time I wish to suggest the propriety of great caution, both in working the mine, and in the management of cargoes of them on board a ship.

I am, dear Sir, yours very truly,

J. H. H. Holmes.

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## Article IX.

### Analyses of Books.

*The Transactions of the Linnæan Society of London,* Vol. XI. Part II. 1816.

This half volume contains 22 papers, of which 10 relate to zoology, 10 to botany, and two to mineralogy.

For the following account of the zoological papers contained in the 11th volume of the Transactions of the Linnæan Society I am indebted to a friend.

*Mammalia.*—In p. 161 a new species of *cricitus* from Trinidad is described by Mr. I. V. Thompson under the title *mus anomalus,* an admirable name, since the author has "some doubts" whether it should not rather be referred to the genus *hystrix!* an animal named *mus castorides* by the Rev. E. I. Burrow. The description is vague; and as there is no figure, we cannot determine whether this be a new species or not, or to what genus it should be referred.

*Aves.*—Description, by Lieut.-Col. Hardwicke, of *corvus leucolophus,* a new species, inhabiting the mountainous forests above Hardware, and living together in flocks consisting of from 20 to 50. "Its note resembles the human voice in loud laughing." (P. 207.)—Some observations on the bill of the toucan, by Dr. Traill, who considers the network of which the interior of the bill is formed as subservient to the purposes of smell, although he assigns no reason in support of this opinion.

*Pisces.*—Notice, by Mr. I. Hoy, respecting *trichiurus lepturus* of

Linnæus, two specimens of which were found on the shore of the Moray-Frith, and had not before been observed in the British seas.

*Gasteropoda.*—Several rare and new species from the Devonshire coast are described by Montagu.—*Tritonia papillosa* of Lamark, p. 16, tab. iv. fig. 3.—A new species of Cuvier's genus *Pleurobrancus*, p. 185, tab. xii. fig. 3; which, with several discordant animals, is placed in a new genus, entitled *lamellaria.*—A new species, named *lamellaria tentaculata*, which is not referrible to any genus that has hitherto been established.—A new *scyllæa*, tab. xiv. fig. 2.—Three new shells, tab. xiii. figs. 3, 4, 5.

*Acephala.*—Figures and descriptions of *terebratula cranium* of Müller, and of a new genus of shells, the tooth of which is moveable, tab. xiii. fig. 1.

*Cirrhipedes.*—Of this class two species are figured and described, under the names *lepas membranacea* and *lepas aurita.*

*Crustacea.*—A new arrangement of these animals by Dr. Leach, who has described in this volume several new species, and has pointed out the characters of some genera that had not been observed by preceding writers. The number of his genera is 56.—Tab. i. represents *atelecyclus heterodon*, and *pisa gibbsii*, which were first described by Montagu under the names *cancer septemdentatus* and *biaculeatus.* *Pisa* is synonymous with the genus *arctopsis* of Lamarck. In tab. ii. fig. 5, an interesting crustaceous animal, *nebalia herbstii*, is given as a *monoculus*; and in the same plate, fig. 6, the genus *proto* occurs, with three small crustacea not yet referred to any natural genus, viz. figs. 2, 3, 7.

*Arachneides.**—These animals are distributed into five orders and 29 genera by Dr. Leach, as we have already shown in a former volume of the *Annals of Philosophy*, when giving an account of the proceedings of the Linnæan Society.

*Myriapoda.*—The arrangement of this class, too, which was first noticed as distinct by Dr. Leach, is here given in detail, with descriptions of several new species. (P. 376.)

*Insecta.*—A monograph on the genus *choleva*, by W. Spence, Esq. who has divided it into the following sections: 1. Antennæ subfiliform; thorax with the hinder angles obtuse. 2. Antennæ clavate; thorax with the hinder angles acute; elytra generally obsoletely striated: *a.* Thorax with the basal margin set out near the angles: *b.* Thorax with the basal margin strait. 3. Antennæ clavate; thorax with the hinder angles acute; elytra not striated. In this paper the species are admirably described; but in the last section the author has included a species of that very distinct genus *mylæchus latr.* The paper is preceded by some acute and learned general entomological observations. (P. 123.)

* In p. 7, tab. ii. fig. 4, is given a cheliferous animal under the name *phalangium acaroides.* The name is as incorrect as the figure, which is evidently composed out of two different species of *obisium*, both of which occur in Devonshire.

In p. 35 is given an essay on the British species of *melöe*, by Dr. Leach, who in p. 242 has given a supplement, in which he has described six exotic species, and arranged the whole into the following sections:—A. Antennæ in both sexes, filiform, short, thick: *a.* Antennæ with their tips entire: 1. Thorax quadrate: 2. Thorax on each side produced: *b.* Antennæ with their tips notched. B. Antennæ in both sexes filiform, long, and slender. C. Antennæ (especially of the male?) thicker towards their tips: *a.* Thorax short, transverse: *b.* Thorax elongate. D. Antennæ (of the male especially) thicker in the middle, most generally broken: 15 species are described, seven of which are British. They are all figured in plates 6, 7, and 18.

The last, and by far the most interesting entomological communication, is on a new order of insects, by the Rev. Wm. Kirby, who has named it *strepsiptera.* We regret that it is not in our power to enter into a minute analysis of this paper. We must, therefore, content ourselves by briefly stating that the parts considered as elytra by Mr. Kirby have no pretensions to that title, as they do not perform the functions of those parts, neither do they arise from the same part of the thorax; in short, they can be considered but as appendages to the thorax, in which light they were viewed by Rossi, the discoverer of the genus *xenos*, the type of the order, which contains another genus, named *stylops* by Mr. Kirby. Both these genera are in the larva state parasitical, living beneath the abdominal segments of certain hymenopterous insects of the genera *polistes* and *andrena* Fabr. The dissertation, as far as it relates to the order in question, is extremely well drawn up, and does infinite credit to its learned author. We cannot, however, subscribe to the rules for the artificial classification of insects. (P. 94, 95.) Plates 8 and 9 illustrate the characters of the order. The latter plate is decidedly the most beautiful that has been produced in this country, and was executed from a drawing by F. Bauer, Esq. An appendix to this paper is given in p. 233.

*Vermes.*—In plates 3, 4, 5, and 14, several species of this class are given by Montagu; but, without an inspection of the animals themselves, nothing can be said with any degree of confidence.

*Radiata.*—Montagu, p. 201, plate 14, fig. 4, has given an animal of the family *medusæ*, under the title *medusa pocillum*, but we do not know to what genus it belongs.

Under the head Extracts from the Minute Book, we find an account of a pig that lived 160 days without food, communicated by T. Mantell, Esq.

In p. 401 is a singular paper on a fossil occurring in the chalk strata near Lewes, under the title, Description of a Fossil Alcyonium, by Mr. G. Mantell, F.L.S. We can only observe that the thing described is no more an *alcyonium* than the variety of discordant objects figured and described as such in the last volume of the Transactions of the Geological Society. The type of *alcyonium* is *A. digitatum*, which is very common on all our coasts.

I can do little more than give the titles of the botanical papers, as few of them, from the nature of the subject of which they treat, are susceptible of abridgment.

1. *On the Deoxidation of the Leaves of the Cotyledon Calycina.* By Dr. Benjamin Heyne.—This is a very curious fact, though Dr. Heyne's theory is not quite so evident. This plant is cultivated commonly in the gardens in India. In the morning the leaves are as sour as those of the sorrel. As the day advances they become less so, and at noon are quite insipid. By the evening they have acquired a bitter taste. Dr. Heyne ascribes the acidity to the oxygen absorbed by the leaves of this plant during the night. This oxygen is again disengaged by the light, and the plant becomes insipid. I cannot perceive how such an absorption should have such an effect. The most probable thing is, that the acid formed during the night is exhaled by the heat of the day. If so, it will probably be found to differ from every known acid in its properties, and in the plant it cannot be combined with any base. If there be any person in India acquainted with the details of experimenting, the subject would deserve his investigation; some new and important fact would probably be brought to light.

2. *Description of a new British Rubus, with Corrections of the Descriptions of Rubus Corylifolius and Fruticosus, and a List of some of the more rare British Plants.* By George Anderson, Esq. F.L.S.—This new species was first noticed in the third volume of the Transactions of the Royal Society of Edinburgh, by Mr. Hall, under the name of rubus nessensis, because he found it on the banks of Loch Ness. Mr. Anderson calls it *rubus suberectus.* It seems to be pretty common in Great Britain.

3. *Some Observations on the Iris Susiana of Linnæus, and on the natural Order of Aquilaria.* By Sir James Edward Smith, P.L.S. &c.—Sir James thinks that this iris is the native of a colder climate than the neighbourhood of Susa, in Persia. It was brought from Constantinople; and the name *susam* appears to be the Turkish appellation for an *iris.* Hence, in his opinion, the origin of the trivial name. He thinks that the plants considered by botanists as forming the natural order of *aquilaria* belong to *euphorbiæ.*

4. *Description of a new Species of Psidium.* By A. B. Lambert, Esq. F.R.S. &c. This species, the *psidium polycarpon,* was first described by Dr. Anderson, of St. Vincents. It ripened in Mr. Lambert's stove at Boyton.

5. *Of the Developement of the Seminal Germ.* By the Rev. P. Keith, F.L.S.—In this paper Mr. Keith controverts the different hypotheses by which vegetable physiologists have endeavoured to explain why the radicles of plants proceed downwards, and their stems upwards. The most ingenious explanation is that of Mr. Knight, who ascribes it to the action of gravity. He showed that when seeds were made to germinate, subjected to a centrifugal force, the radicles obeyed that force, while the stems grew towards it. This is a striking analogy, and renders the assigned cause, gra-

vitation, very probable; though I do not think that Mr. Knight has succeeded in showing us how gravity can make the stems of plants grow upwards; nor do I think that the subsequent explanation of Mr. Campbell (*Annals of Philosophy*, vol. vii. p. 443) serves in the least to obviate the difficulty. I conceive that the experiment of Mr. Knight has rendered it probable that gravity is the agent; but we are still unable to show in what manner that power acts.

6. *Remarks on Dr. Roxburgh's Description of the Monandrous Plants of India.* By Wm. Roscoe, Esq.

7. *Observations on the Genus Teesdalia.* By Sir James Edward Smith, M.D.—Mr. Brown had constituted the *iberis nudicaulis* of Linnæus into a new genus, called *teesdalia.* The object of this paper is to show that the lepidium nudicaule of Linnæus belongs likewise to this genus. He proposes to call it *teesdalia regularis*; but he acknowledges it to be doubtful whether the two do not in fact belong to one and the same species.

8. *Remarks on the Bryum Marginatum and Bryum Lineare of Dickson.* By Sir James Edward Smith, M.D.—This constitutes a defence of Mr. Dickson against an unjust attack made upon him by Bridel.

9. *Description of several new Species of Plants from New Holland.* By Edward Rudge, Esq. F.R.S.—The species described in this paper are the dodonæa cuneata, asplenifolia; philotheca australis; darwinia fascicularis; pultenæa ferruginea, elliptica, polygalifolia; eriostemon salicifolia.

10. *Description of nine new Species of Plants from Caucasus.* By Chevalier de Steven, Counsellor of the University of Moscow. —The names of these plants are, veronica crista-galli; anchusá alpestris; androsace albana; cucubalus lacerus; silene cæspitosa; orobus formosus; serratula elegans, depressa; orchis mutabilis.

The only mineralogical paper, which has not been already noticed, is the following:—

*Observations on Arragonite, together with its Analysis.* By the Rev. John Holme, A.M. F.L.S.—Arragonite has long attracted a good deal of the attention of mineralogists. Its constituents, as far as could be detected by analysis, were found the same as those of calcareous spar; but the figure of its crystals, its specific gravity, and hardness, differ materially from those of that substance. It constitutes a species apart; and the difficulty was to account for this difference in the properties of two substances composed of the same constituents in the same proportions. Stromeyer discovered that arragonite contained about two per cent. of carbonate of strontian, and this discovery has been verified by Vauquelin, Bucholz, and other celebrated analysts. But it is difficult to see how so minute a quantity of carbonate of strontian can account for the difference in the structure of the crystals, in the hardness, and in the specific gravity. Mr. Holme has shown another difference to exist between arragonite and calcareous spar. The first contains combined water, while the second is destitute of that substance. When the water is

driven off, the arragonite falls to powder, and its specific gravity is diminished. According to Mr. Holme, arragonite is composed of

| | |
|---|---|
| Lime | 55·8 |
| Carbonic acid | 43·4 |
| Water | 0·8 |
| | 100·0 |

The objection to this mode of accounting for the different properties of arragonite and calcareous spar is the very small quantity of water found. An atom of carbonate of lime weighs about $6\frac{1}{4}$, and an atom of water 1·125. According to these weights, arragonite would be composed of 18 atoms of carbonate of lime and one atom of water. It is difficult to conceive how so small a proportion of water can unite with so great a proportion of carbonate of lime.

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## Article X.

### *Proceedings of Philosophical Societies.*

#### ROYAL INSTITUTE OF FRANCE.

*Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year* 1815.

Mathematical Part.—*By M. le Chevalier Delambre, Perpetual Secretary.*

#### ANALYSIS.

(*Continued from* p. 149.)

*Application of the Calculus of Probabilities to Natural Philosophy.* By M. Laplace.

"When we wish to know the laws of a phenomenon, and acquire great exactness, the observations are combined in such a manner as to eliminate the unknown elements, and a mean of them is taken. By the theory of probabilities, the most advantageous mean result, or that which gives the least hold to error, is determined. But this is not sufficient; it is necessary, likewise, to determine the probability that the error is confined within given limits. Without this, we have but an imperfect knowledge of the accuracy obtained. This is one of the things which I had chiefly in view in my Theorie Analytique des Probabilités, in which I have established formulas, which have the important advantage of being independent of the law of the probability of errors, and of containing only quantities given by the observations themselves, and by their analytical expression."

Here the author gives a clear and concise explanation of his

method, from which it would not be possible to take a single word, and for which we are under the necessity of referring to the Connaissance des Temps for 1818, which will be published before this notice. In that book a great number of useful and curious formulas will be found, with which it is impossible for us to enrich our extract. In the memoir on the ebbing and flowing of the tide, we have just seen an application of the method. This new memoir exhibits two others: one respecting the amount of the masses of Jupiter, Saturn, and Uranus; the other, respecting the law of the variation of gravity. By a laborious investigation of the motions of Jupiter and Saturn, M. Bouvard found the mass of Saturn equal to $\frac{1}{3512}$th part of that of the sun. This is the sum of the masses of Saturn, his satellites, and ring. From the formulas of probability M. Laplace has calculated that it is 11000 to 1 that the error of this result does not amount to $\frac{1}{100}$th part of its value; or that after a century of observations, added to those already obtained, and examined in the same manner, the result will not differ $\frac{1}{100}$th part from that of M. Bouvard. He finds, likewise, that there are several thousands to 1 that the error does not amount to $\frac{1}{50}$th part. Newton had found that mass equal to $\frac{1}{3012}$th part of the sun, which is about $\frac{1}{6}$th greater than that of M. Bouvard. It is several millions of thousands to 1 that Newton's number is an error; and we shall not be surprised at this, says M. Laplace, if we consider the extreme difficulty of observing the elongations of the satellites of Saturn, which served as the basis of Newton's calculation. The satellites of Jupiter present much smaller difficulties, and we find in fact that with respect to the mass of this planet, Newton came much nearer the truth; for the mass which he has assigned it is $\frac{1}{1067}$, and M. Bouvard has obtained $\frac{1}{1071}$ by means of quite different observations, which give us a million to one that the error of the result is not $\frac{1}{100}$th part, and 900 to 1 that it is not $\frac{1}{150}$.

M. Bouvard has endeavoured to obtain the mass of Uranus from the effects which it produces on the motion of Saturn. The result is, that its mass is $\frac{1}{17918}$th of that of the sun. The difficulty of observing the elongation of the satellites of Uranus is even much greater than that of the elongations of the satellites of Saturn. Therefore we may reasonably suspect a considerable error in the mass which results from the elongations observed by Herschel. In a new memoir, which will appear in the Philosophical Transactions for 1815, and which contains interesting details respecting these imperceptible satellites, Dr. Herschel has made several changes in the nodes, the inclinations, and the periods, which he had formerly assigned. Hence some change may likewise result in the mass. We do not know how much that is of which M. Laplace has spoken; and we ought to refrain from all calculation, and all explanation, till the author himself has published the work of which he has sent us a copy. To return to the result of M. Bouvard, it is 213 to 1 that the error is not $\frac{1}{20}$th, and 2456 to 1 that it is not $\frac{1}{4}$th. We

cannot expect greater precision till observations have been sufficiently multiplied; that is to say, at the end of about a century.

For the second application of his method M. Laplace has chosen the law of gravitation, and the experiments with the pendulum. The observations which appear to him to deserve the greatest confidence are 87 in number, and extend from N. lat. 67° to S. lat. 51°. The rate is very regular; yet a still greater precision would be desirable. The length of the isochronous pendulum resulting from it follows very nearly the simplest law of variation, that of the square of the sine of the latitude. The two hemispheres in this respect do not present a sensible difference, or at least which may not be ascribed to errors of observation. By a profound discussion of these observations, M. Mathieu has found that the length of the pendulum which swings seconds at the equator being taken as unity, the coefficient of the term proportional to the square of the sine of the latitude is $\frac{551}{100000}$. The formulas of probability applied to these observations give 2127 to 1 that the true coefficient lies between the limits of 5000ths and 6000ths.

If the earth is an ellipsoide of revolution, the coefficient 5000 corresponds to a flattening of $\frac{1}{272}$. "It is 4254 to 1 that the flattening is less. There are millions of thousands to 1 that this coefficient is less than that which corresponds to the homogeneity of the earth, and that the beds increase in density as they approach the centre. The great regularity of gravity at the surface proves that they are disposed symmetrically round that point. These two conditions, necessary consequences of the fluid state, could not exist with respect to the earth, unless it had at first been in that state, which a violent heat could alone give to the whole globe."

Next come the general formulas: the author arranges them for particular cases, when the number of unknown quantities is 2, 3, or 4. This part not being susceptible of extract, we refer to the Connaissance des Temps for 1818, published by the Board of Longitude.

*Exercises in the Integral Calculus.* Fifth Part. By M. Legendre.

We have in the notices of former years given an account of the first parts of this work. The fifth part, which has just appeared, leads us to expect that it will be followed by several others, in which the author will continue to develope the consequences of the principles which he has laid down, and to add to the number of equations of which the integration is possible. The inquiries which we announce to our readers are for the most part a continuation of those which are the object of the third and fourth parts. Some relate to the developement of functions into series; others relate in general to the means of facilitating and extending the applications of the integral calculus by the exact valuation or approximation of different sorts of definite integrals. As all the mathematical truths are connected with the subject, we shall not be surprised that the author

on his way meets with theorems or formulas already known, either because their close connexion has led him naturally to them, or because, struck with the importance of a new proposition, the author has turned aside to come to it, and to connect it to the general theory. Thus in paragraphs 3, 4, and 5, will be found several formulas which we owe to M. Poisson and to M. Cauchy. The 6th treats of some transcendentals expressed In continued fractions. We find in it an explanation of an error remarked in a result of Euler. We see that continued fractions ought to be employed only with great precautions, and we ought to be always certain that the quantity necessarily omitted in the term at which we stop will not produce a sensible effect upon the total value of the fraction. The author terminates by remarking, that it would be difficult to give an example where the use of continued fractions in the integral calculus gives any advantage over that of series, which represent the value term by term. In the 7th paragraph we find important theorems on returning series; methods which the author shows to agree with a formula of Lagrange, and which serve him to demonstrate the formulas which we owe to M. Burman. The 8th gives the means of summing a given number of terms in the developement of $(1 + a)^n$. The 9th contains a remarkable extension given to the methods of Lagrange to develope in converging series the arc whose tangent is given in a rational function of the sines and cosines of another indefinite arc. We see that this sort of resolution may always take place, whatever be the coefficients of the given function, and that from these developements we may deduce some remarkable theorems respecting definite integrals. Paragraphs 10 and 11 are occupied in developing more complicated expressions. This is the case, likewise, with paragraph 12, in which the author is taken up with a peculiar species of transcendentals, which possess several beautiful properties, and which have numerous applications in the theory of the perturbations of the planets. Here the examples are calculated to from 8 to 13 decimal places, to show more clearly the use of the formulas. When the demonstration of these formulas depends upon calculations of some length, which is the most common case, the author points out different ways, all of which lead to the same point. This attention, which is not to be slighted in the most simple problems, becomes so much the more indispensable, the more elevated the theories are, and the more difficult the calculations.

*Demonstration of a Theorem from which may be deduced all the Laws of ordinary and extraordinary Refraction.* By M. Ampere.

M. le Comte Laplace has reduced all the phenomena of ordinary and extraordinary refraction to a single principle, namely, that of the least action. He has shown that by means of this principle all the problems relative to this part of natural philosophy are reduced to simple questions of analysis, as soon as observation has made us acquainted with the velocity which light acquires in each medium.

The velocity depends on the direction in consequence of a very

simple law in crystals, in which the effect of these forces has been already determined. This velocity is a function of the angle which the direction of the refracted ray makes with a fixed axis. But M. Biot has found in mica two axes of polarization, which may lead to the supposition that in this substance, and in several others not yet examined, the law of the motion of light may be more complicated, and may depend upon two angles, which might determine the direction of the ray.

M. Ampere, in consequence, has conceived that it would be useful to demonstrate in general the identity of the principle of least action, and of a construction analogous to that which Huyghens has given for the case in which the law of the extraordinary refraction depends only on a single angle. In order to determine the direction of the ray, it is sufficient to know always the angles which it forms with two lines given in position. It results from this, that in whatever manner we have the expression of the law according to which the velocity depends upon the direction, we can always, by the known formulas of spherical trigonometry, calculate the value of this velocity in a function of the two angles, which determine the direction to two lines taken an pleasure, provided that the respective position of the axes of the crystal and of these lines be known.

For the object which the author has in view, it is sufficient for him to establish in general the possibility of expressing the velocity in that manner. It is useless for him to calculate the value of the function which represents it, because the considerations of which he has to make use, and the results which he has deduced from them, do not depend upon the determination of this function, either in the medium into which the ray passes, or in that from which it issues.

The simplicity of the demonstration results from the choice of the two variable quantities, which he has considered as independent. He has employed the formulas of transformation, and the notation used already in several of his memoirs on the integration of equations of partial differences. And he flatters himself that the new use which he has made of it will leave no doubt respecting the advantages which may be drawn from it in several applications of the analysis to questions of mechanics and physics.

We see that this memoir is purely analytical. We could not continue the extract of it without giving the formulas of the author, without which it would be impossible to make any use of the theorem which constitutes the principal object of the memoir.

From the same theorem M. Ampere deduces a construction applicable to all the cases in which we know the velocity of light in functions of the two angles which determine its direction, and by which we construct the refracted ray, when we have the direction of the incident ray.

He then shows the relation of his general construction to that which Huyghens has given in a particular case.

*Memoir on two Micrometers for measuring the Diameters of the Sun and Moon.* By M. Rochon.

We have already had more than one opportunity of speaking of these micrometers. Both the instruments and the memoir are in the hands of M. Arago, who is examining them. We shall take care to give an account of the result of these observations as soon as they are finished.

*Memoir on the Distribution of Heat in Solid Bodies.* By M. Poisson.

The Class in 1812 had proposed, as the subject of a prize, the question which forms the subject of this memoir. M. Fourrier obtained the prize in consequence of the new formulas which he presented, and the great number of experiments which appeared to demonstrate at once both the formulas and the physical suppositions on which the calculus was founded. The Class had expressed regret that the author had not removed the difficulties which his analysis still left, and that his demonstration had not all the rigour and all the generality which the importance of the question required. M. Poisson, who entertained the same opinion, and who had it in his power to examine at leisure the manuscript left at the secretary's office at the Institute, was induced to resume the whole mathematical theory of the distribution of heat in solid bodies. His analysis, though more rigorous, is likewise less simple, as might indeed have been expected. But the important point is, that it leaves no doubt with respect to the certainty of the results, and that it furnishes us with a general and complete solution of the problem. He conceives, also, that it may be applied to other questions in physics and mechanics, the solution of which had not been attempted, or which still left something further to be done.

The author begins by explaining the nature of the question, and detailing the difficulties which the calculus has to resolve. He enumerates the principles and facts established by experiment, which constitute the bases of the analytical reasonings, as well as of the suppositions which he ventures to adopt, and which he circumscribes within limits where they may be regarded as perfectly conformable to experiment. The data which he thus obtains lead him to consider three quantities, which must be carefully distinguished from each other, as constant for every species of body; namely, the capacity of the body for heat, its conducting power, and the coefficient of the temperature in the expression of the exterior radiation. These three quantities being given in numbers for the body which we examine, the general solution of the problem naturally divides itself into two parts. The 1st relates to the research of the differential equations on which the distribution of heat in the interior or at the surface of the body depends. The 2nd, which is purely analytical, comprehends the integration of these equations, and the determination of the arbitrary functions contained in their integrals from the initial state of the body, and the conditions relative to its surface.

A difficulty occurs at the outset respecting the manner in which the differential equation ought to be formed. To escape it, for it appears insurmountable, the author supposes, with Laplace, that the action of each elementary particle of the body extends beyond contact, and that it extends to all the elements contained in a given space, as small as you please. The law of this action may be expressed, as in the theory of refractions, and in that of capillary attraction, by a function of the distances, which becomes insensible or null as soon as the variable quantity acquires a sensible value. But in this hypothesis the variation of the temperature is given by an integration, the result of which is independent of the form of that function. The equation of the motion of heat preserves the same form that it had before, but the differential homogeneity is re-established, and the difficulty disappears.

This supposition of an action at a distance between the particles of a solid body differently heated, may appear strange to those who are not sufficiently initiated in this kind of calculus; it has even found unbelievers among mathematicians of a superior class. But it is not a mere mathematical supposition, made merely to cause an irregularity to disappear. It is conformable to every thing that we observe in nature, where we know that all the molecular actions extend beyond contact, and to finite distances, which indeed escape our senses. However, M. Poisson, in admitting this notion, which requires to be well ascertained, acknowledges that it is a point to be determined by experiment whether the radius of this sphere be as small as is supposed. If, on the contrary, its extent were sensible, some modifications in the calculus would be necessary, which the author promises to point out as a supplement to his memoir.

From this mode of viewing the communication of heat, M. Poisson has formed the equation of its motion in a body of any figure whatever, and has obtained that which M. Fourrier first gave in his memoir that gained the prize, the demonstration of which was not quite complete. Besides this equation, common to all the points of the body, there is another, which belongs only to the points of the surface supposed radiating. This general equation, as well as the first, is found in the memoir of M. Fourrier, who had formed it at first for the faces of a parallelopiped, for the surface of a cylinder, and that of a square, and had then extended it by induction to the surface of any body whatever.

These equations remained to be integrated. The first is an equation of partial differences of the second order. But it belongs to those which apply only to a single arbitrary function in their complete integral, as M. Poisson had already demonstrated. The method which M. Fourrier had followed was, as he observes himself, similar to that which Daniel Bernoulli had employed for vibrating cords. We know the objections which Euler, d'Alembert, and Lagrange, made to Bernoulli at the time. They may be started, likewise, against M. Fourrier. Hence his demonstration may appear insufficient. But this does not prevent all his results from

being correct, as M. Poisson has ascertained, and has expressly acknowledged in his memoir.

We could have wished to have given at least some idea of the analysis of the author, but we are under the necessity of referring to the memoir; and we may mention to our readers that they will find a copy of it in the Bulletin de la Societé Philomatique for the month of June, 1815.

*Memoirs on the Theory of Waves.* By M. Poisson.

This question was proposed by the Class as the subject of a prize to be decided in 1816. But when M. Poisson terminated his investigation, the prize was still undetermined; no candidates had put in their claims: yet he did not consider himself as at liberty to make known the result of his researches. He satisfied himself with announcing them to the Class, who received the memoir into their custody. A single memoir was sent for the prize, and M. Poisson found in it formulas similar to those of his own memoir for the case of infinite depths. The question, which he has treated anew, and more at length, is of such a nature as scarcely to admit of any abstract. We shall merely state the fundamental suppositions of the author.

He supposes that the water did not receive any percussion at the origin of movement, and that it was deranged from a state of equilibrium in the following manner. A piston of any form whatever is plunged to a small depth in the water. The fluid is allowed time to come to a state of rest. Then the immersed body is suddenly withdrawn. Waves are formed round the spot which it occupied; and the object of the paper is to determine their propagation, both at the surface, and in the inside of the fluid mass. M. Poisson has considered only the case in which the agitations of the water are sufficiently small to permit us to neglect the square, and the higher powers of the velocities and displacements of the molecules. Without this restriction, the problem would be too complicated to admit of solution. He supposes the depth of the water constant in all its extent; so that the bottom of the water is a horizontal plain, placed at a given distance below the surface. He treats, in succession, of the case of a fluid contained in a vertical canal, of a constant breadth and of an indefinite length, and of a fluid whose surface extends indefinitely in all directions.

*Levelling of the Mounts d'Or and Dôme.* By Mons. Ramond.

This is a continuation of the levelling of the environs of Clermont-Ferrand, of which an account was given in our abstract for 1808. The city of Clermont is the common origin from which all these operations commence. To measure with more accuracy the heights of objects at a great distance, it was requisite to obtain intermediate stations, the elevation of which was known with great precision. After having settled these by means of the barometer, M. Ramond was desirous to try them by trigonometrical operations.

M. Broussard, Chief of the Battalion of Engineers, who was then employed in the department of Puy-de-Dôme with a great geodesical operation, under the direction of the general depot of war, furnished this comparison. The greatest difference between methods so different was two metres, and this happened with respect to Mount d'Or, which the repeating circle could only determine indirectly, and by induction. For Puy-de-Dôme the difference amounts only to a metre, and for Puy-de-Sancy it is only two decimetres, in a height of 1843 metres.

After having thus established the intermediate stations, M. Ramond deduces from them the height of 80 mountains, and of the 200 most remarkable points in that country. This mathematical part, the only one which belongs to the sciences which are the object of this abstract, constitutes but the least curious part of the work. The author was particularly anxious to furnish accurate measurements for the purposes of natural history. He examines the surface from his levelling, with respect to the origin of the soil, and under every point of view which belongs to physical geography. Here terminates the fragment which has been transmitted to us; and we regret not to be able to give any idea of the numerous observations, the acute views, the ingenious conjectures, which cause us to wait with much anxiety for the reading of the whole memoir.

(*To be continued.*)

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## Article XI.

### SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Lectures.*

*Medical Theatre, St. Bartholomew's Hospital.*—The following Courses of Lectures will be delivered at this theatre during the ensuing winter:—On the Theory and Practice of Medicine; by Dr. Hue.—On Anatomy and Physiology; by Mr. Abernethy.—On Chemistry and Materia Medica; by Dr. Hue.—On the Theory and Practice of Surgery; by Mr. Abernethy.—On Midwifery; by Dr. Gooch: the Demonstrations of Anatomy, by Mr. Stanley, to commence on Tuesday, Oct. 1, at two o'clock.

*Theatre of Anatomy, Medicine, &c. Blenheim-street, Great Marlborough-street.*—The Autumnal Course of Lectures at this School will begin on the following days:—Anatomy, Physiology, and Surgery, by Mr. Brookes, daily at two, on Tuesday, Oct. 1, 1816. Dissections as usual.—Chemistry, Materia Medica, &c. daily at eight in the morning. Theory and Practice of Physic at nine, with examinations, by Dr. Ager, on Monday, Oct. 7.—

Three courses are given every year, each occupying nearly four months. Further particulars may be known from Mr. Brookes, at the Theatre; or from Dr. Ager, 69, Margaret-street, Cavendish-square.

*St. George's Medical, Chemical, and Chirurgical Schools.*—The Courses will commence the first week in October.—On the Laws of the Animal Economy, and the Practice of Physic; by Dr. G. Pearson.—On Therapeutics, with Materia Medica and Medical Jurisprudence; by Dr. G. Pearson and Mr. Brande.—On Chemistry; by Mr. Brande.—On the Theory and Practice of Surgery; by Mr. Brodie.—Sir E. Home will continue to give Lectures on Surgery gratuitously to the pupils of St. George's Hospital.

Mr. Clarke will commence his winter Courses of Lectures on Midwifery, and the Diseases of Women and Children, on Thursday, October 3. The lectures are read every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals, at the lecture room, No. 10, Saville-row, Burlington Gardens.

Mr. Wilson and Mr. Bell will commence their Lectures on Anatomy and Surgery, in Great Windmill-street, on Oct. 1, at two o'clock.—Mr. Bell will deliver a separate Course of Lectures on Surgery in the evening.—The Anatomical Demonstrations will be given by Mr. Shaw, who will direct the students in their dissections.

Dr. Clutterbuck will begin his Autumn Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Wednesday, Oct. 2, at ten o'clock in the morning, at his house, No. 1, in the Crescent, New Bridge-street, Blackfriars.

## II. *Charcoal.*

Dobereiner's method of obtaining what he considers as the metallic basis of charcoal is as follows:—Mix together one part of very finely pounded charcoal with one part of black oxide of manganese and two parts of iron filings. Put the mixture into a black lead crucible, which must be covered with a lid. Expose it for several hours to a white heat. By this process he obtains an alloy of manganese and iron, and a blackish grey substance in plates having a strong metallic lustre. This is metal of charcoal not quite free from iron and manganese. But these metals may be removed by digestion in aqua regia and muriatic acid. The following are the properties by which this substance is distinguished:—

Colour, black grey. Lustre, very strong, not capable of being melted. Does not burn till heated white hot. Its specific gravity is 3·5. It conducts electricity, and combines with different metals.

These properties, which are all that Dobereiner details in his Elements of Pharmaceutic Chemistry, just published, lead to the conclusion that the substance which he obtained is similar in its properties to carburet of manganese. It seems to me that this is the substance which he has in reality made. Though he does not

appear to be aware of the presence of manganese in his supposed metal.

### III. *Insects in Switzerland.*

In the *Annals of Philosophy* for March, vol. vii. p. 243, the appearance of a great number of caterpillars on the snow in Switzerland was mentioned. I have since seen it noticed in one of the Journals that these insects were the larvæ of the *cantharis fusca.*

### IV. *Colouring Matter of Blood.*

As soon as the presence of iron in blood was ascertained, it became fashionable for chemists to ascribe to it the cause of the red colour of the blood. Fourcroy and Vauquelin affirmed that the iron was in the state of a subphosphate held in solution by the serum, and capable of altering its colour when acted upon by air and several other bodies. This opinion was combated in 1797 by Dr. Wells, who endeavoured to prove that the colouring matter of the blood is not iron, but an animal substance. But though his arguments are simple, ingenious, and convincing, (Phil. Trans. 1797, p. 416), they do not seem to have drawn any attention from chemists or physiologists.

Berzelius, however, adopted a similar opinion to that of Dr. Wells; but I do not know when his experiments on the colouring matter of the blood were made. From his observations on the colour of blood in the first volume of his Lectures on Animal Chemistry, published in 1806, he seems to have been aware at that time that blood does not owe its colour to phosphate of iron, though he does not relate any experiments on the globules, which indeed would have been inconsistent with the nature of his work. In the Philosophical Transactions for 1812 Mr. Brande, who was unacquainted with what Berzelius had done on the subject, inserted a set of experiments on blood, in which he showed, in a very decisive manner, that the colouring matter of blood was an animal substance, capable even of being fixed upon cloth as a dye.

The same year the experiments of Berzelius became known in this country by his paper containing a general view of the Composition of Animal Fluids, published in the third volume of the Medico-Chirurgical Transactions. The conclusions of Berzelius and Brande have been lately confirmed by Vauquelin, who has published a set of experiments on the colouring matter of blood in the first number of the Annales de Chimie et Physique, p. 9. His method of obtaining this substance is as follows:—

Take the coagulum of blood well drained upon a searce, and digest it in four parts of sulphuric acid diluted with eight parts of water for five or six hours at the temperature of 160°. Filter the liquid while hot, and wash the residuum with as much hot water as you have employed of acid. Evaporate the liquid to half its bulk, and then add ammonia till only a slight excess of acid remains,

The colouring matter precipitates. When it has subsided, decant off the clear liquid, add pure water, let the colouring matter again subside, and again decant off the water. Proceed in this manner till the water ceases to precipitate nitrate of barytes. Then dry the colouring matter at a low heat.

This matter possesses the following properties:—

1. It has no sensible taste nor smell.

2. When dry it appears as black as jet, of which it has the fracture and lustre. When mixed with water it assumes a wine-red colour, but does not dissolve in that liquid.

3. It dissolves readily both in acids and alkalies, and communicates a purple colour to these liquids. Its solution in muriatic acid does not alter the transparency of muriate of barytes. Thus it shows the absence of sulphuric acid.

4. Gallic acid and prussiate of potash produce no change in the acid solutions of this substance, thus demonstrating the absence of iron. The infusion of nutgalls throws it down from its solution in acids, but does not alter its colour.

5. When heated, it neither alters its form nor colour. If the heat be increased, it gives out the usual smell of animal substances, and furnishes carbonate of ammonia and a purple oil, but scarcely any gas. The residue is a charry matter, neither soluble in acids nor alkalies.

6. When dissolved in dilute nitric acid, the colour is not altered. The solution is not altered by nitrate of silver, but nitrate of lead throws down a brown precipitate, and discolours it completely.

## V. *Analysis of Common Air.*

Dr. Prout, in his important paper on the Relations between the Specific Gravities of Bodies in their gaseous State and the Weights of their Atoms (*Annals of Philosophy*, vol. vi. p. 321), considers common air as a chemical compound of one volume oxygen and four volumes azote, or of one atom oxygen and two atoms azote. About the same time (November, 1815), the same opinion was maintained by Dobereiner, in a paper published in Schweigger's Journal. According to this opinion, common air ought to be composed of 20 measures of oxygen and 80 of azote, instead of 21 oxygen and 79 azote, which is the common estimate. I lately made some experiments, with as much care as I could, in order to put this opinion to the test. I consider the most accurate way of analysing common air to be to mix it with rather more than $\frac{2}{3}$ths of its bulk of pure hydrogen gas, and to burn the mixture by means of an electric spark. The third part of the volume which has disappeared gives the volume of oxygen present in the mixture. The following experiments leave no doubt that the common opinion is the true one. The air which I analyzed was collected in St. James's Park.

| Volumes of air. | Volumes of hydrogen. | Residue. | Diminution of bulk. | Oxygen in the air. |
|---|---|---|---|---|
| 100 | 50 | 87 | 63 | 21 |
| 100 | 50 | 87 | 63 | 21 |
| 100 | 60 | 97 | 63 | 21 |
| 100 | 60 | 97 | 63 | 21 |
| 100 | 40 | 80 | 60 | 20 |
| 100 | 40 | 81 | 59 | 19·7 |
| 100 | 40 | 83 | 57 | 19 |

In the first four experiments the quantity of hydrogen present was more than sufficient to consume the whole of the oxygen. In them we see that the volumes of oxygen indicated in 100 air are constantly 21. In the last three experiments the quantity of hydrogen not being sufficient to consume the whole of the oxygen, we perceive irregularities in the result, and the quantity of oxygen is uniformly less than in the first four experiments. This might have been expected; and shows us that common air cannot be correctly analyzed by means of hydrogen unless the quantity of that gas exceed $\frac{2}{5}$ths, as it did in the first four experiments.

If we attend to the changes that are continually going on in common air from respiration, combustion, &c. it must be obvious that the proportion of its constituents must be continually changing, though the variation is a great deal too small to be detected by the most delicate methods of investigation which we have it in our power to employ.

### VI. *Red Manganese Ore from Longbanshytta.*

This ore, of which a description will be found in Jameson's Mineralogy, vol. iii. p. 335, has been lately analyzed by Berzelius. He found it composed of

| | |
|---|---|
| Silica | 48·00 |
| Oxide of manganese | 54·42 |
| Lime | 3·12 |
| Magnesia | 0·22 |
| Trace of iron. | |
| | 105·76 |

He considers it as composed of

| | |
|---|---|
| Bisilicate of manganese | 93·288 |
| Bisilicate of lime | 6·712 |
| | 100·000 |

(See Afhandlingar i Fysik, Kemi och Mineralogi, vol. iv. p. 382.)

### VII. *Garnet of Fahlun.*

All my mineralogical readers must be acquainted with the garnet

of Fahlun, so remarkable for the immense size of the crystals, but nearly opake, and possessing but little beauty. This garnet has been lately analyzed by Hisinger, who found it composed of

| | |
|---|---|
| Silica | 39·66 |
| Alumina | 19·66 |
| Black oxide of iron | 39·68 |
| Oxide of manganese | 1·80 |
| | 100·80 |

He considers it as composed of silicate of alumina and silicate of iron; so that its symbol is A S + *f* S. (See Afhandlingar, vol. iv. p. 385.)

## VIII. *Tantalum.*

This metal has been lately reduced to the metallic state, and its properties ascertained by Berzelius. His method was to expose the oxide to a violent heat, surrounded with a lump of well-burnt charcoal in a hessian crucible. The oxide of tantalum was pressed into a cavity, about the size of a goose-quill, made in the charcoal.

The reduced metal had not been melted; but the particles of it adhered firmly together, and formed a mass, through which water would not penetrate. The grains were hard enough to scratch glass. Dr. Wollaston, at my request, took the specific gravity of a specimen of tantalum sent by Berzelius to Dr. Macmichael, and found it 5·61. But as the mass had not been melted, there can be no doubt that tantalum is still heavier than this. Its colour is dark grey; and, when scratched with a knife, or rubbed against a fine grindstone, it assumes the metallic lustre, and puts on the appearance of iron. It may be reduced to powder by trituration, and the powder has not the smallest metallic lustre, but assumes a dark brown colour. This powder is not in the least altered by muriatic acid, nitric acid, nor aqua regia, though it be digested with them for several days. In this respect it agrees with chromium, titanium, iridium, and rhodium.

When heated to redness, it takes fire, and burns feebly without any flame, and goes out directly if it be removed from the fire. By this means it is converted into a greyish-white matter, which may be again reduced, by heating it in charcoal, to the metallic state. 100 parts of tantalum treated in this manner combine with 3·5, 4, or 4·5, of oxygen. But by this process it is scarcely possible to oxidize tantalum completely.

When pulverized tantalum is mixed with nitre, and thrown into a red-hot crucible, a feeble detonation takes place. The mass is snow-white, and is a compound of potash and oxide of tantalum.

The mean of four experiments on the reduction of oxide of tantalum to the metallic state makes it a compound of 100 metal + 5·485 oxygen. The supposition that the oxygen in the water which converts this oxide into a hydrate is twice as great as that in the oxide, would make it a compound of 100 metal + 5·5 oxygen.

If this last supposition be true, and if tantalum combine only with one atom of oxygen, then the weight of an atom of it will be 18.

Muriatic acid throws down oxide of tantalum from its combination with potash, of a white colour. If this oxide be well washed and dried, it is in the state of a white hydrate, composed of 100 oxide + 12·5 water.

It would appear from the experiments of Berzelius that the oxide of tantalum possesses acid properties.

He alloyed several metals with tantalum, as tungsten and iron. (See Afhandlingar, vol. iv. p. 253.)

### IX. *Tantalite.*

Berzelius has made a new analysis of the tantalite from Finland, which had been previously examined by Ekeberg. The specific gravity of one specimen was 7·236; of another, as determined by Ekeberg, 7·936. He found the constituents of the first specimen as follows:—

| | |
|---|---|
| Oxide of tantalum | 83·2 |
| Oxide of iron | 7·2 |
| Oxide of manganese | 7·4 |
| Oxide of tin | 0·6 |
| | 98·4 |

He considers it as a compound of tantalate of iron and tantalate of manganese. (See Afhandlingar, vol. iv. p. 262.)

### X. *Yttrotantalite.*

Of this mineral from the quarry of Ytterby, in Sweden, which was first noticed by Ekeberg, Berzelius has described three varieties, which he distinguishes from each other by the following names:—

1. *Black Yttrotantalite.*—Colour black. Fracture in one direction foliated. Lustre glimmering, metallic. Fragments indeterminate. Easily frangible. Powder grey. Opake. Hard enough to scratch glass. Specific gravity 5·395. Decrepitates feebly before the blow-pipe, becomes dark brown, but does not melt.

It is composed of

| | |
|---|---|
| Oxide of tantalum | 57·00 |
| Tungstic acid | 8·25 |
| Yttria | 20·25 |
| Lime | 6·25 |
| Oxide of iron | 3·50 |
| Oxide of uranium | 0·50 |
| | 95·75 |

2. *Yellow Yttrotantalite.*—Colour yellowish brown, with spots of green; frequently with streaks and lines of green. Longitudinal fracture foliated; cross fracture conchoidal; fracture of the distinct concretions fine granular. Lustre of the principal fracture resinous,

of the cross fracture vitreous. Opake. Gives a white powder. Scratches glass very readily. Specific gravity, according to Ekeberg, 5·882. Does not fuse before the blow-pipe; but decrepitates feebly, and its colour becomes light straw-yellow.

The analysis of this variety by means of sulphate of potash gave

| | |
|---|---|
| Oxide of tantalum | 60·124 |
| Yttria | 29·780 |
| Lime | 0·500 |
| Oxide of uranium | 6·622 |
| Oxide of iron | 1·155 |
| Tungstic acid with tin | 1·044 |
| | 99·225 |

The analysis of the same variety by means of carbonate of soda gave

| | |
|---|---|
| Oxide of tantalum | 59·50 |
| Yttria | 24·90 |
| Lime | 3·29 |
| Oxide of uranium | 3·23 |
| Oxide of iron | 2·72 |
| Tungstic acid | 1·25 |
| | 94·89 |

3. *Dark Yttrotantalite.*—Colour black, with scarcely a shade of brown. Fracture conchoidal. Lustre between vitreous and resinous. In thin fragments translucent, and almost colourless. Gives a white powder. Hardness the same as of the preceding variety. Specific gravity not determined. Does not fuse before the blow-pipe, but decrepitates feebly, and becomes light yellow.

The analysis of this variety gave the following constituents:—

| | |
|---|---|
| Oxide of tantalum | 51·815 |
| Yttria | 38·515 |
| Lime | 3·260 |
| Oxide of uranium | 1·111 |
| Tungstic acid with tin | 2·592 |
| Oxide of iron | 0·555 |
| | 97·848 |

(See Afhandlingar, vol. iv. p. 267.)

## XI. *Sulphuric Acid.*

I find the specific gravity of sulphuric acid free from water, at the temperature of 60°, to be 1·8777. Now sulphuric acid of the specific gravity 1·85 contains 19 per cent. of water. Supposing no condensation to take place, its specific gravity would be 1·7109. This fact enables us to calculate the degree of condensation that takes place in acid of all different degrees of strength. It will be found that the greatest condensation takes place when one

atom of acid combines with two atoms of water, which constitutes acid of the specific gravity 1·780. The condensation is nearly the same when one atom and when three atoms of water combine with one atom of acid. The following little table will show the condensation in these cases:—

| Acid. | Water. | Sp. Gr. | Sp. Gr. supposing no condensation. | Condensation. |
|---|---|---|---|---|
| 1 atom + | 0 atom .. | 1·8777 | | |
| 1 + | 1 ..... | 1·850 | ..... 1·71 | ..... 0·140 |
| 1 + | 2 ..... | 1·780 | ..... 1·596 | ..... 0·184 |
| 1 + | 3 ..... | 1·650 | ..... 1·514 | ..... 0·136 |
| 1 + | 10 ..... | 1·300 | ..... 1·263 | ..... 0·037 |
| 1 + | 17 ..... | 1·200 | ..... 1·175 | ..... 0·025 |

## XII. *Method of removing old Putty from Glass.*

On this subject I have received the following communication from Dr. Reid Clanny, of Sunderland:—

"Some time ago I observed a request from one of your Correspondents to know the best and shortest method for removing hard putty from window-frames, &c. It has lately come to my knowledge that iron, when heated, will melt the putty, and readily permit the glass, or any other substance, to be detached. This plan has been used by a person whom I know for several years. He says that when a mass of putty becomes hard by being kept too long before it is used, the best plan is to put it into a tin vessel, and keep it in a moderately heated common oven, when it will again be fit for use."

## XIII. *Gadolinite.*

Berzelius has lately subjected this mineral to a very careful analysis. He found gadolinite from Finbo composed of

| | |
|---|---|
| Silica ..... | 25·80 |
| Yttria ..... | 45·00 |
| Oxide of cerium ..... | 16·69 |
| Oxide of iron ..... | 10·26 |
| Volatile matter..... | 0·60 |
| | 98·35 |

He found the constituents of gadolinite from Broddbo to be

| | |
|---|---|
| Silica ..... | 24·16 |
| Yttria ..... | 45·93 |
| Oxide of cerium ..... | 16·90 |
| Oxide of iron ..... | 11·34 |
| Volatile matter ..... | 0·60 |
| | 98·93 |

Berzelius considers it as composed of $f^2 S + ce^2 S + 8 Y S$. (See Afhandlingar, vol. iv. p. 217.)

### XIV. *Topaz and Pyrophysalite.*

These minerals are characterized by the fluoric acid which they contain. They have been analysed by different persons, but their results do not correspond quite exactly. Berzelius has lately subjected them to a very rigid examination. The following table exhibits the results which he obtained:—

| | Alumina. | Silica. | Fluoric Acid. | Total. |
|---|---|---|---|---|
| Brazilian topaz | 58·38 | 34·01 | 7·79 | 100·18 |
| Saxon topaz | 57·45 | 34·24 | 7·75 | 99·44 |
| Pyrophysalite | 57·74 | 34·36 | 7·77 | 99·87 |
| Schorlous beryl | 51·00 | 38·43 | 8·84 | 98·27 |

He considers these minerals, the first three of which obviously belong to the same species, as composed of $A^2$ Fl + 3 A S, and the last of A Fl + 3 A S. (See Afhandlingar, vol. iv. p. 236.)

### XV. *Tungsten.*

Only two species of mineral containing this metal are at present known; namely, wolfram and tungstate of lime. Berzelius has lately subjected both these minerals to analysis. He found the constituents of wolfram

| | |
|---|---|
| Tungstic acid | 78·775 |
| Oxide of iron | 18·320 |
| Oxide of manganese | 6·220 |
| Silica | 1·250 |
| | 104·565 |

He considers it as Mg T + 3 Fe T, or a compound of three atoms of tungstate of iron and one atom of tungstate of manganese.

Tungstate of lime he found composed of

| | |
|---|---|
| Tungstic acid | 80·417 |
| Lime | 19·400 |
| | 99·817 |

(See Afhandlingar, vol. iv. p. 293.)

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## Article XII.

### *New Patents.*

George Bodley, of Exeter, ironfounder; for an improved metallic engine, to work either by steam or water, which he denominates Bodley's improved metallic Engine. April 27, 1816.

John Collyer, of Windsor Terrace, engineer; for a machine for shearing woollen cloths. May 1, 1816.

John Rangeley, of Oakwell Hall, Yorkshire; for certain further improvements of his hydropneumatic engine, being a new or improved method of constructing and working engines or machines for lifting or raising of weights, turning machinery of all descriptions, drawing carriages on railways, and capable of being applied to all purposes where mechanical power is required. May 4, 1816.

Robert Copland, of Liverpool, merchant; for a means of effecting a saving in the consumption of fuel. May 4, 1816.

William Threadgold, of Farm-street, Berkeley square, surveyor and builder; for a machine or apparatus to prevent obstructions to the passage of smoke in and through chimneys. May 4, 1816.

Richard Banks, of Hadley, Salop, engineer; for certain improvements on wheeled carriages. May 4, 1816.

Benjamin Rotch, late of Castle Hall, Milford Haven, now at Bath; for a flexible elastic horse-shoe, for the purpose of allowing the foot of the horse its natural motion when shod. May 11, 1816.

Daniel Wilson, of Usher-street, Dublin, chemist; for certain new and improved apparatus to be employed in the distillation of animal, vegetable, and mineral substances, and in various other processes. May 14, 1816.

Thomas Ruxton, of Dublin, Esq.; for a lock for fastening doors, gates, drawers, desks, trunks, boxes, portmanteaus, and other things requiring fastenings, which he conceives will be of great public utility. May 14, 1816.

Jean Samuel Pauly, of Knightsbridge; for certain improvements in the construction and use of fire-arms. May 14, 1816.

William Simmons, of Wigan, writing master, &c.; for certain improvements applicable to keyed instruments, as the organ, pianoforte, harpsichord, or any other instrument or set of instruments to which keys are, or may, or can be affixed. May 14, 1816.

Richard Francis Hawkins, of Woolwich; for a method, plan, or principle, by which tunnel or tunnels, archway or archways, may be constructed or effected under the river Thames, or other rivers, for the passages of cattle, foot passengers, and other purposes. May 14, 1816.

Philip Taylor, of Bromley, merchant; for a method of applying heat to liquors, and in the processes of brewing, distilling, and sugar refining. May 25, 1816.

Francis Richardson, of Queen-street, Westminster, Esq.; for improvements in the locks and barrels of fire-arms; and also an improvement or addition to bayonets. May 25, 1816.

Christoph Dihl, of New Bond-street, Esq.; for an improvement or improvements in the making mastic cement or composition, and in the mode of working and applying the same to useful purposes; which cement or composition he calls Dihl's Mastic. May 25, 1816.

## Article XIII.

# METEOROLOGICAL TABLE.

| 1816. | Wind. | Barometer. Max. | Min. | Med. | Thermometer. Max. | Min. | Med. | Evap. | Rain. | |
|---|---|---|---|---|---|---|---|---|---|---|
| July 17 | Var. | 29·66 | 29·48 | 29·570 | 67 | 50 | 58·5 | — | ·11 | ☾ |
| 18 | S W | 29·56 | 29·48 | 29 520 | 66 | 51 | 58·5 | — | — | |
| 19 | S E | 29·75 | 29·55 | 29·650 | 70 | 58 | 64·0 | — | ·21 | |
| 20 | S W | 29·76 | 29·57 | 29·665 | 81 | 65 | 73·0 | ·66 | — | |
| 21 | S W | 29·78 | 29·59 | 29·685 | 70 | 54 | 62·0 | — | — | |
| 22 | S W | 29·79 | 29 65 | 29·720 | 70 | 58 | 64·0 | — | ·32 | |
| 23 | S W | 29·65 | 29·65 | 29·650 | 73 | 55 | 64·0 | — | — | |
| 24 | Var. | 29·66 | 29 65 | 29·655 | 64 | 52 | 58·0 | ·59 | ·45 | ● |
| 25 | N W | 29·86 | 29·66 | 29·760 | 65 | 54 | 59·5 | — | ·05 | |
| 26 | N W | 29·95 | 29·86 | 29·905 | 68 | 53 | 60·5 | — | — | |
| 27 | N W | 29·96 | 29·80 | 29·880 | 64 | 53 | 58·5 | ·26 | — | |
| 28 | N W | 29·80 | 29·74 | 29·770 | 64 | 46 | 55·0 | — | | |
| 29 | Var. | 29·74 | 29·63 | 29·685 | 63 | 45 | 54·0 | — | | |
| 30 | N W | 29·63 | 29·55 | 29·590 | 64 | 41 | 52·5 | ·25 | — | |
| 31 | N E | 29·65 | 29·54 | 29·595 | 65 | 48 | 56·5 | ·10 | ·03 | ☽ |
| Aug. 1 | N W | 29·80 | 29·65 | 29·725 | 63 | 49 | 56·0 | — | — | |
| 2 | W | 29·85 | 29·80 | 29·825 | 67 | 51 | 59·0 | — | ·27 | |
| 3 | S W | 29·88 | 29·85 | 29·865 | 68 | 49 | 58·5 | ·41 | | |
| 4 | S W | 29·88 | 29·85 | 29·865 | 69 | 47 | 58·0 | — | | |
| 5 | N W | 29·95 | 29·80 | 29·875 | 70 | 51 | 60·5 | — | ·51 | |
| 6 | S W | 29·97 | 29·88 | 29·925 | 68 | 57 | 62·5 | ·23 | ·03 | |
| 7 | S W | 29·88 | 29·79 | 29·835 | 70 | 57 | 63·5 | — | — | |
| 8 | S W | 29·79 | 29·77 | 29·780 | 74 | 55 | 64·5 | — | — | ○ |
| 9 | S W | 29·98 | 29·79 | 29·885 | 67 | 53 | 60·0 | ·42 | — | |
| 10 | N W | 30·06 | 29·98 | 30·020 | 65 | 57 | 61·0 | — | — | |
| 11 | S W | 30·06 | 30·06 | 30·060 | 70 | 57 | 63·5 | — | — | |
| 12 | N W | 30·06 | 30·00 | 30·030 | 67 | 56 | 61·5 | ·35 | ·26 | |
| 13 | S W | 30·00 | 29·80 | 29·900 | 66 | 52 | 59·0 | — | | |
| 14 | S E | 29·80 | 29·58 | 29·690 | 68 | 58 | 63·0 | — | ·08 | |
| 15 | S E | 29·59 | 29·53 | 29·560 | 71 | 56 | 63·5 | ·25 | ·09 | |
| | | 30·06 | 29·48 | 29·771 | 81 | 41 | 60·4 | 3·57 | 2·41 | |

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.

*July* 18.—Squally day. 19. Rainy morning: very boisterous wind all day, with showers. 20. Fine morning. 21. Showery day: a strong breeze from the S. W. 24. Wind variable: very rainy day: some thunder in the afternoon: a *Stratus* on the marshes at night. 25. Foggy morning: a *Stratus* on the marshes at night. 30. A heavy shower of rain between one and two o'clock, p.m.: some hail. 31. Very foggy morning: a thunder storm in the evening.

*Aug.* 2.—Showery day. 5. Foggy morning: trees dripping: some thunder in the afternoon: very rainy night.

## RESULTS.

Winds variable: chiefly S.W. and N.W.

| | | |
|---|---|---|
| Barometer: | Greatest height | 30·06 inches |
| | Least | 29·48 |
| | Mean of the period | 29·771 |
| Thermometer: | Greatest height | 81° |
| | Least | 41 |
| | Mean of the period | 60·4 |

Evaporation, 3·57 inches. Rain, 2·41 inches.

Laboratory, Stratford, Essex, *Aug.* 20, 1816. JOHN GIBSON.

*** During the absence of Mr. Howard, the Editor has been favoured with a continuation of the Journal by his friend Mr. Gibson.

BRITISH MUSEUM
NATURAL HISTORY.

1

2

3

REMARKABLE HEAD OF A HEN.

J. Shury sc

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1816.

## Article I.

*Description of a Hen having the Profile of the Human Face, with some Observations.* By Professor Fischer.*

(With a Plate.)

NEVER has a hen attracted so much attention; never has any animal, even the most rare, so greatly excited the curiosity of the public, as the hen with the human profile, which was found in the district of Belef, in the government of Tula, and sent to the Imperial University of Moscow by his Excellency the Civil Governor, Mr. Bogdanoff.

I had no sooner received this hen with the human profile than the curious presented themselves to see it; and the number of visitors increased so greatly, from day to day, from hour to hour, that frequently my chamber could not contain them; so that I was obliged to fix certain days for exposing this animal to public view.

For the satisfaction of such individuals as have not had an opportunity of seeing this animal, I here present them with a faithful sketch, accompanied with some observations.

The hen is of the middle size, *i. e.* eight inches high, and 14 long. Her feathers are of a pearlish-grey colour, and brown in some places, particularly at the points. The form of her body, as well as her manner of living, is the same as that of other hens; but her head presents an extraordinary phenomenon; for at the place where the beak ought to be she exhibits a human profile, resembling that of an old woman. The beak is entirely wanting;

* Translated almost verbatim from the Russ, with some additions from the German edition, by Dr. Lyall, Physician to Count Orlof, at Moscow.

and the jaw-bones, or jaws, are shortened in such a manner that they terminate where, in other hens, the nostrils are found. They are covered with flesh, and resemble lips. The comb, in a front view, in this hen, forms a kind of nose; which appears the more astonishing, as the nostrils are found between the termination of this nose and the jaw; but we are most liable to be deceived when we see, as sometimes happens, some drops of liquid in them, or when the dust is accumulated there. To the inferior jaw is attached a fleshy excrescence, not to be found in other hens, and which forms a kind of chin. This chin is bare, or naked, with the exception of some hairs of beard, and is prolonged with naked skin, even to the ears, as in other hens. The eyes are round and black, and surrounded with an iris of a cinnabar-red colour. The parts of the head under the eyes are of a flesh colour, mixed with blue, and almost naked, or covered, like the chin, with a kind of stiff hairs, which form towards the ears a sort of whiskers, and conceal the aperture of the ear. (See Plate LVI. Fig. 1.) These peculiarities of the head, united, present a great resemblance between the profile of this hen and that of an old woman, particularly if one does not attend to the tuft of feathers on the head of this animal; and the longer and more attentively we look at this profile, especially when the hen feeds, the more striking does the resemblance become. In consequence of this conformation, the animal cannot take the kind of food which suits it. As the beak is wanting, and has for substitute a kind of mouth, it is very difficult for her to eat, and still more so to collect grains. The too great advancement of the nostrils prevents her altogether from drinking; it is, therefore, necessary to feed her with bread soaked in water or in milk. She prefers eating white bread with cream; and when hemp-seeds are presented to her in the hand, she appears to swallow them with great avidity; yet she likes, as well as all other hens, hashed meat, corn, &c. I have heard that she has also been seen to eat cheese with much eagerness. She is very tame, as is the case with all birds which have the beak maimed, whether done by the hand of man or by chance. She prefers eating from the hand, as the soft parts about the mouth (the comb and under chin) are soon injured when she is obliged to peck her nourishment on hard bodies.

Since I have had her in my chamber, and nourished her from my hand, she knows me very well, and approaches the place where I sit, whether while dining or drinking tea, and calls for something to eat by a particular cry. Her voice, although feeble, resembles that of other hens; and often, when alone, she cackles like a hen about to lay. Notwithstanding the loss of the beak, after having eaten she makes the ordinary motions of the head to wipe and clean the two sides of the jaw upon hard bodies, as on the table or the ground. This hen appears better pleased to be in human society than among other fowls. When another hen is carried into the chamber, and placed near her, she begins to be angry, lets her wings fall, swells and raises herself, and makes a noise like that

of a cock which is preparing for combat. In the kitchen she is at continual warfare with the other hens, which she chases; but she herself takes to flight as soon as she perceives the cock. The cock appears now, however, to inspire more confidence; and we have the best founded hope that a favourable connexion will take place between them. In the open air she appears to be timid; and she conceals herself among the grass on the approach of crows or birds of prey, or on the least noise. If she happens to be at the chamber window, and to observe crows passing, she sinks at every one of their movements, and gapes with fear. This hen was changing her feathers when I received her (four months ago), and the change is not yet finished, which proves that it is more slowly effected than in other hens, because her nourishment, being insufficient, renders her unhealthy; yet the feathers become more thick and lucid, and the plume on the head and neck is become much more bushy. The feet are strong, and the scales which cover them are almost the same as those of a hen of two years of age. She is without spurs; and I am unable to determine whether she was hatched without them, or has lost them in battle, or by cold. On the right foot one nail is deficient, and on the left two.

This is a true description of the hen, and of the facts which I have observed.

I intend now to answer some questions which many of my readers may wish to propose.

*First Question.*—Does this hen in reality exhibit the profile of a human face?

She is constructed in the same way as other hens; but the more we observe her laterally, the more we find, in the profile of the head the resemblance to that of an old woman. But this signifies nothing; it only proves that one object may resemble another; as, for example, that a cloud may have the figure of a lion, or the head of a fly, bruized among sheets of paper, may resemble the head of an American crocodile; and so with a thousand other objects. It is certain that man in his features appears to resemble an animal more or less; as we sometimes see the nose on the human face like an eagle's, but no one is offended. Indeed every one who has observed his fellow creatures with a little attention will have remarked this. This has been observed by Posta, Goree, Lavater, and many others. They have rendered themselves more comprehensible to the senses by figures. Yet there never has been found any animal which resembles man in its exterior form. Whatever has been exhibited of this kind was the effect of imposture, as was the case at Erfurth with the shaved pig with the human hand, and with the shaved bear at Gottingen; as those who have described them have added much more than a reasonable view could possibly discover. I do not wish to enumerate the many writers who have endeavoured, during a certain time, to occupy the imagination of men by figures invented, and said to be the miracles of Nature. In detailing, however, the true nature of the profile of this hen,

we do not wish to persuade ourselves that, besides the *visual line*, like that of man, which is produced by the want of the beak, and which is certainly very remarkable, that there is any thing supernatural. By the shortening of the jaws, the comb is found more prolonged, and its extremity is become more thickened, and redder than ordinary, perhaps by the difficulty which she has to peck her nourishment. The redness of the comb and of the chin in these animals appears to vary as the redness of the cheeks of man, which are more or less pale at different times. The thick comb, which at its extremity assumes the form of a bunch of grapes, resembles a nose in front, as well as in profile. The nostrils are found just below the nose; yet they do not ascend, as in the human body, but descend, and terminate in the upper jaw. They are smaller than ordinary, and are unequal, that on the right side even appearing to be closed. By the shortening of the jaws, the mouth is rendered smaller, and thus forms below a kind of a bag, which takes the place of the chin. The chin of this hen is nothing else than the enlargement of a *naked skin*, which is found under the throat, or at the jaw of all hens, or an enlargement of the throat itself.

*Second Question.*—Was this hen hatched with this appearance, or her beak been shortened, when a chicken, by chance or by art? I think this animal was hatched in the state in which she is; for Nature has constructed her beak, although simple, yet with much art. It is covered in such a manner with horn, which is firmly connected to the jaws, that the beak could not be pulled away without breaking in the middle, injuring a part of the tongue, and thus occasioning the inevitable death of the animal. To render this truth yet more evident, let us suppose a shortening of the beak by art. What would be the consequence of such a mutilation? The sides of the jaw would be uncovered, and that on the right side would always be separated from that on the left. The tongue would be lost, even to the *lamdoidal suture*, and have, by its continual and trembling motion, been extremely difficult to cure; or, in case of repose, would have attached itself to the inferior jaw, and formed one body with it. In fine, the animal would have perished from want, before the cure could have been completed. If we could even suppose the possibility of a less fortunate cure, we should now be able to see the cicatrices of the wounds; yet the parts appear to be connected naturally. The tongue is short and fleshy, and has the form of a triangular and arched spade. (See Fig. 3.) In the palate we observe some furrows, which, however, cannot be compared to teeth.

Perhaps it may be opposed to this reasoning that a chick formed in the egg without a beak would not be able to break its calcareous envelope, the shell, and consequently would be unable to get out of it. Although it be true that the chick often opens with its beak the prison in which it is enclosed, during its developement; nevertheless it often happens that the shell is burst or split, in all its length, by the growth and nourishment of the chick. Besides, it

must be considered that much less force is requisite to break the shell from the interior than from the exterior. We often find an egg with the shell broken, whilst the chick is entirely enveloped in that interior skin, or in that humid envelope, which surrounds it.

*Third Question.*—Does this singular conformation of the hen indicate something supernatural? It indicates nothing at all. I would have passed this question in silence, had it not been addressed to me by many individuals. But my answer could not satisfy those who sought, in this extraordinary form, an extraordinary design; because they sought an augury where there was none. Besides, it is not unusual to find chicks deformed, especially in places where they have been hatched by an artificial heat. There we may find some hatched without a beak, without wings, without legs, but they do not attract attention, as they cannot exist without the assistance of man; and as the poultry dealers only want complete animals, on which they may reckon a neat profit, the most curious are seldom preserved by them for naturalists or the utility of science. Ten years ago there was exhibited publicly in Poland a hen which resembled ours. In short, Nature, though always the most perfect in her works, yet may be led from her usual path by circumstances unknown to man, or at least may be interrupted in her operations. In the process of formation the equilibrium of the seed may be broken by the slightest touch, and produce a protuberance; and the inhabitant of the thickest shell covers with calcareous layers the places which a parasite or a *piercing animal* threatens to bore, even till it has rendered the *smooth place* of its habitation, so small that at length it perishes itself.

*The Fourth and last Question.*—In what point of view do naturalists regard this curiosity? Naturalists consider this animal as an irregularity of Nature, or, *lusus naturæ*, as a monster. Monster is the general term of naturalists and anatomists for every organic body which exists in its formation something removed from the general laws according to which those of its kind are formed. This deviation of nature may consist in the absence, or superabundance, or overgrowth, of some of the parts, or of the whole of organic bodies; and thus they may be double, or in plurality, or in deficiency or deformity of some of the parts, or of the whole. In museums it is not rare to find quadrupeds with two heads, eight feet, &c. Thus by the kindness of his Excellency Mr. Paul Golenitscheff Kutusoff, Curator of the University, &c., and that of his Excellency Mr. Nicholas Vsevoloschsky, Vice-President of the Medico-Chirurgical Academy, &c. our museum has been enriched with two remarkable objects of this kind—two chickens, each with four feet. Those monsters of the second division, those with deficiency or malconformed parts; such, for example, as living bodies with only one eye. Thus we have in the Museum of the University a stuffed dog with only one eye, placed in the middle of

the brow. I have seen here, in Moscow, a living calf with three legs, which could leap very nimbly. Our hen without the beak will always be a great curiosity for our museum, and we are indebted to Mr. Bogdanoff for his politeness and attention.

If the last division, that of deformed or deficient bodies, to which our hen belongs, appears to be more rare, we must attribute it to the want of attention, because we give less heed when parts are wanting than when they are superfluous. An amateur of similar deformities will find a rich treasure of observations in the works of Regnault, Sëmmering, Blumenbach, and others. It remains for physiologists to show how such deformities have their origin. May I be permitted just to add here a single idea, which, if it be not itself a sufficient reason, appears, however, to be in the way of facilitating the road to truth. There reigns in all organized nature an eternal tendency to transformation, which may be looked upon as a gradation of increase, according to positive laws. This transformation is most evident in insects and in frogs, which, before arriving at their full size, assume extraordinary forms. But this transformation is not wanting in any organic body, not even in plants, as Goethe shows so well, and with so much force, in their green periods (*i. e.* on the earth), and is yet better demonstrated in their first stages (*i. e.* in the first developement of the germs of plants, yet enclosed in the earth) than by any other means. This mystery is profoundly concealed in the most elevated classes of animals. It appears to partake of the nature of prodigy when it has attained the most inferior degrees of organization. How great ought to be the transformation which takes places in a drop of water, which, although it appears clear and limpid, encloses myriads of animals in full motion! What eye is able to measure the transformation which takes place in drops of vinegar, where one believes all is dead, whilst in a minute we see a vibration, and bubbles, which arise in serpentine motions! By the force of this transformation the blood which flows in the veins is changed into animal flesh. Further, who can tell us how this immensity of animals, which live in calcareous shells, as the moluscæ and the polypes, can transform organized matter into lime. Or if we represent to ourselves every organized being, every animal, every plant, according to the degree of their formation, with attention; *i. e.* in the first germ, the first birth of the organic mass, for the form which that being ought to adopt, according to the laws of things, we could easily devise a hindrance to the developement of the *formative virtue*, by which there would result a being incompletely organized or deformed; and by the same indeterminate causes there might result a being or animal doubly deformed. The artists Pharof and Osipof, belonging to the University, have prepared a plate of this hen for his Excellency Mr. Golenuschef-Kutusoff; and I have had another prepared for the public, representing the head of its natural size, by Mr. Valesicon, the artist.

EXPLANATION OF THE PLATE.

Fig. 1 is a true profile of the hen's face, like to that of an old woman.

Fig. 2 is a front view of the head.

Fig. 3 is a profile view of the head, with the mouth open to show the tongue.

## Article II.

*On Rheumic Acid.* By John Henderson, Esq. Surgeon.

(To Dr. Thomson.)

SIR, *Lawton, Dec.* 1, 1815.

I MUST beg to be allowed a corner of your Journal for the insertion of a paper the subject of which will, I trust, be interesting to some of your readers, when it is found to contain the history of a substance hitherto unknown in the chemical world; and though I am well aware that it may contain some inaccuracies, I am certain of its meeting with every necessary indulgence from those who are able to appreciate the difficulty of such an investigation.

Having for a long time remarked that, amongst the immense variety of vegetable productions, but a very few salts were peculiar to them, and being anxious to prove for myself that no others existed, I entered on the analysis of some of them, with as much care as circumstances would allow; and amongst other plants, I subjected to experiment that commonly called rhubard, class *enneandria*, genus *monogynia*. I had two reasons for choosing this plant; the first was, that I conceived it might contain a very considerable portion of the citric acid, and that this article might be more advantageously manufactured from it; and, secondly, that it might contain an acid peculiar to itself; and this idea, I was happy to find, was not altogether groundless.

*Exper.* 1.—Having expressed the juice of the stems of the rhubard, first by beating them in a mortar, and afterwards passing the whole through a cloth, I put in gradually a quantity of powdered chalk, tasting the liquid now and then, till I found that the juice had lost every trace of acidity, and the liquor, from being clear and colourless, became cloudy. It was then allowed to settle for some time, when the supernatant liquor was carefully poured off, and pure water put in its place. The whole was then perpetually stirred, that the powder at the bottom might be completely washed.

Again the water was cloudy; and the same was repeated till it was nearly pure. This, last of all, was drained off also; and in its place a quantity of sulphuric acid was added, diluted with twice or thrice its bulk of water.

A very strong effervescence, as may be supposed, took place, while the whole swelled to three times its former bulk, and at the same time a vapour issued, giving a sensation of smell like the concentrated juice of rhubard, evidently showing that a part of the pure acid of the rhubarb was then in a state of vapour. When the acid was poured in a stream upon any part of the powder, it assumed a purple or black colour, as is the case in pouring sulphuric acid on phosphate of lime. While the effervescence continued, the mixture was frequently stirred, and afterwards was allowed to stand for twelve hours. The liquid part was then drawn off into a Florence flask, where it was evaporated to one-fourth, and set to crystallize. The first crystals formed were the most beautiful I ever saw; they were as white as snow, and shining like the crystals of benzoic acid. I had, however, to resort to a more accurate experiment to ascertain their form and properties.

*Exper.* 2.—8 lbs. of the stalks of the rhubarb were taken, deprived of their leaves, from which was obtained about a pint and a half of juice.

| | lb. | oz. |
|---|---|---|
| The pint weighed ..... | 4 | $4\frac{1}{2}$ |
| Remaining juice ..... | 2 | 1 |
| | 6 | $5\frac{1}{2}$ |
| Ligneous remainder ..... | 1 | 7 |
| | 7 | $12\frac{1}{2}$ |

$3\frac{1}{2}$ oz., therefore, were lost during the operation.

To this liquor $13\frac{1}{2}$ oz. of powdered chalk were added, until no taste of acidity could be perceived. Still, however, on the liquor being put to the test of tincture of litmus, the colour was instantly changed to red. Having found this for a considerable time after no acid taste could be perceived, I began to suspect that this test might be a fallacious one, on account of the water being by this time saturated with carbonic acid gas, which, I need not say, issued in bubbles from the chalk. I then ceased to put in more chalk, but after stirring it well, set it by to settle, and allowed it to stand 12 hours, when the liquor was drawn off, and a grey powder remained at the bottom, which was the rheumate of lime.

This powder was washed, as formerly, five or six times. Next day, on examining the fluid first drawn off, I was, I confess, a good deal surprised to find that it had assumed a blackish purple colour; and, what is singular, the same took place in every other experiment; therefore this was a liquid which possessed little or no colouring matter whilst its acid existed, whose colouring matter became

evident on that being removed. This appearance, also, it must be mentioned, could not be owing to iron, for the liquid was contained in an earthenware vessel.

I was now very much puzzled regarding the exact quantity of sulphuric acid necessary to form a neutral sulphate of lime.

By analysis, sulphate of lime is composed of 32 chalk and 46 acid. But the specific gravity of chalk and acid are as 11 chalk : 7·5 acid, as 13·8. Chalk is to a fourth proportional 11 : 7·5 :: 13·8 : 9·3; and by multiplying the third by 32, and fourth by 46, the proportion is 427 : 415·14, or in nearly equal proportions. However, I found that a greater proportion of rheumate of lime was necessary to saturate the sulphuric acid; for 8 oz. saturated the whole 13½ oz. I must here remark, that when the acid is poured on the chalk in a concentrated state, the grey powder is apt to get into lumps, and always turns of a blackish purple. Water was then added to the mass; and when the effervescence began, the smell of the rheumic acid was very agreeable, though too strong to be borne for any length of time. The liquor, after having stood for 24 hours, during which time it was repeatedly stirred, was drawn off, and put into shallow plates by the side of a regularly heated stove. I afterwards, however, found it convenient to make use of a hot-house; and after being evaporated to about ¼, they were set aside to crystallize, the liquor assuming a slightly red colour during evaporation. The crystals, as soon as formed, were taken out of the liquid, and dried in paper. The first formed were the whitest, and those constituted what chemists denominate *needle-formed* crystals, resembling nearest of any the crystals of benzoic acid.

The method I have chosen to prepare this acid, I am sensible, is on many accounts a difficult and laborious process; at the same time there is a very considerable quantity lost during the operation. It also contains a quantity of selenite; so that it is always necessary, in order to obtain it pure, to crystallize it a second time: but although it contains that foreign salt; when it is crystallized a second time, the form of its crystals are still the same. As I mentioned before, there is a considerable loss during the operation, which is owing to a property it has in common with the citric acid of being acted upon by sulphuric acid; and, therefore, when the evaporation has gone a certain length, the sulphuric acid becomes concentrated, and reduces the other acid to carbon, which is one of its ingredients; and I therefore allow that an easy method of preparing it remains yet to be discovered.

I once tried another method of preparing it, which, though it was not more successful, led me, however, to a conclusion which I conceive of no small importance. Instead of using chalk in preparing the acid, I tried the effect of quick-lime, when, in place of carbonic acid gas being given out, as formerly, *ammoniacal* vapours issued. This, as might be supposed, attracted my attention; but it was some time before I could acquire a satisfactory explanation of

it, I at first thought that there might have remained a quantity of vegetable matter amongst the lime, and that, during the process of shaking, a part of the water was decomposed, while the vegetable matter, from the heat, gave out *nitrogen*, which attached itself to an atom of *hydrogen*. This was, however, a very doubtful explanation; and I afterwards altered my opinion, from some other circumstances; and I am now convinced that the rheumic acid exists in the plant under the form of *rheumate of ammonia*.

In an experiment I made some time afterwards, in order to prove whether any connexion existed between *this* acid and the *citric*, I added a quantity of mercury in its metallic state to a solution of rheumic acid, which was immediately acted on. I then tried whether the juice would act on mercury when assisted by heat; and I was happy to find this was the case. This induced me to try the same thing in other experiments; and at length I applied the juice in a concentrated state, which was done by evaporating it to dryness by the action of slow fire. The mass thus formed was of a brown colour, with evident marks of crystallization. It was rather deliquescent than otherwise, and the taste was that of acid and salt, rather inclining more to salt. To this I added a small quantity of alcohol; and having mixed the whole together, the albumen was dissolved, and small crystals were deposited in the bottom of the vessel. These were taken and dried. They were found to resemble the salt commonly known by the name of *salt of lemons*. On being placed on a red-hot iron, they first swelled in the form of a pyramid, giving out at the same time certain *gases*, and afterwards there remained only a black coal. The same thing happens when pure acid is put upon a red-hot iron.

From certain experiments I made on the pure acid, I conceive it to be composed of 31 oxygen and 69 carbon. In this, however, I may be mistaken; for from the apparatus I possessed, I could not perform any very delicate experiment. The last time I prepared any of the acid, instead of putting the chalk in the vessel containing the juice, I divided it into five equal portions, each of which was put into a separate vessel.

A part of the juice was then poured on No. 1; and, after standing some time, was drawn off into the one marked No. 2; and this was drawn off in the same way, first into No. 3, then into Nos. 4 and 5. When it came to the last, not the slightest mark of acidity could be perceived by the most delicate test. Another quantity of juice was then applied in the same way, till the whole was saturated. This plan I conceive to be far better than the first way of doing it; and I may at the same time also mention, that I at length extracted the juice, not only from the stems, but also from the leaves, by boiling them to a syrup.

I conceive there might still be another way of preparing this acid; which is, after dissolving a quantity of acetate of lead in water, to add a quantity of the juice of the rhubarb.

A white precipitate will then fall to the bottom, which must be

allowed to settle, and the supernatant liquor be poured off. Then to this is added a quantity of *nitric* or *nitrous* acids; and when it has been allowed to stand some time, it is drawn off and evaporated, as in the foregoing experiments.

Dissolve the crystals, and then evaporate it a second time. The acid, when obtained pure, is soluble in about two parts of water, and is slightly deliquescent. It differs materially in its crystals from all the acids yet known, coming nearest to the *benzoic*; it is not, however, so flocculent, has not the aromatic smell, is more soluble, and of greater specific gravity.

There are four acids which it resembles in a few of its effects; it will, therefore, be necessary, before going further, to enumerate those properties which discriminate them from the acid now under our consideration.

These are the citric, oxalic, tartaric, and benzoic acids.

### *Citric Acid.*

This acid forms beautiful rhomboidal crystals. Citrate of potash is very soluble, crystallizing with difficulty, and very deliquescent. Citrate of soda has a saline taste; its crystals are that of a six-sided pyramid. The supercitrate of lime is a crystallizable salt, and is soluble in water.

Quicksilver is not acted on in its metallic state, but is readily acted on when in the state of an oxide. It forms small crystals, scarcely soluble.

Zinc is dissolved by it, and forms small brilliant crystals, nearly insoluble.

### *Oxalic Acid.*

This acid is obtained in slender four-sided rhomboidal prisms.—The salts of the oxalic acid are scarcely soluble, such as oxalate of soda, which can scarcely be obtained in regular crystals. The oxide of mercury is dissolved, which forms a white powder that blackens on exposure to the sun.

Copper is dissolved by it, and a powder is obtained from their union of a pale blue colour, and scarcely soluble.

Lead, when digested with it, forms crystallized grains that are scarcely soluble, except when an excess of acid is used. With tin it forms prismatic crystals. Zinc is converted by it into an insoluble powder, which, however, becomes soluble on an excess of acid being employed. With the white oxide of arsenic, it forms prismatic crystals.

### *Tartaric Acid.*

This acid crystallizes in prisms and pyramids. Tartrate of soda crystallizes in fine needles.

It combines with the oxide of mercury, and forms an insoluble compound. With tin it forms a white powder, and precipitates a solution of bismuth white.

It dissolves the oxide of antimony.

### *Benzoic Acid.*

This crystallizes in soft flocculent crystals, which possess an aromatic smell, that is increased by heat. It is scarcely soluble in cold water. Benzoate of lime is soluble. Oxide of tin is not dissolved by it; but antimony is.

It would be needless to detail the differences between the rheumic acid and the other vegetable acids, as there does not exist the slightest resemblance between any of them.

### *Rheumate of Potash.*

To a solution of the acid was added a small quantity of pure potash; and after the effervescence had ceased, I set it by to crystallize.

Small crystals were formed, but I did not ascertain their form. They had rather an acrid taste, and tinged bismuth green.

I confess I was considerably surprised, on adding the caustic alkali to the pure juice, to observe that, when it came to the point of saturation, the whole assumed a deep red or purple colour, at the same time that the albumen was precipitated of the same colour. After filtering the solution, I set it to crystallize. The crystals formed were red, but by washing they became transparent.

This salt is not deliquescent: it may, therefore, be distinguished from the tartrate, citrate, oxalate, and benzoate of potash.

### *Rheumate of Soda.*

This salt I have formed either by adding carbonate of soda to the pure acid, or in an impure way by adding it simply to the juice. In the latter way it has the same effect upon it as the pure potash has with regard to changing its colour, and precipitating a part of the albumen. When evaporated, it is obtained in the form of a four-sided prism. Of this, however, I was not quite certain, as those I chose for my examination were very irregularly formed. This salt is not deliquescent, and possesses alkaline qualities. It may, therefore, be distinguished from the citrate, oxalate, and tartrate of soda.

### *Rheumate of Ammonia.*

This salt I have not yet examined.

### *Rheumate of Iron.*

The concentrated or simple juice acts very readily on iron in its metallic state, especially if assisted by heat. It also acts on the carbonate and oxide of iron. The crystals thus formed are small, but I am not certain as to their exact form.

The carbonate of soda precipitates it of a grey colour; and, on the other hand, the rheumate of iron precipitates the acetate of lead. With the prussiate of potash a blue precipitate falls to the bottom; and with tannin it forms a blackish precipitate. When

the acid is made to act on iron, it first converts it to a black oxide, and then dissolves it. It possesses rather a caustic taste.

### *Rheumate of Zinc.*

The rheumic acid seems to have a great affinity for zinc. It acts very violently on it when in the state of the white oxide. The solution is of a straw colour; and when the carbonate of soda is added, it is precipitated of an orange colour. This salt does not crystallize. It possesses a very caustic taste; and, when added to the acetate of lead, a precipitation takes place. This, I suspect, is owing to a superabundance of the rheumic acid. It may be distinguished from citrate and benzoate of zinc.

### *Rheumate of Tin.*

When the plain or concreted juice is boiled on tin foil, it forms first a darkish coloured oxide, and then dissolves it. When the juice is used for this experiment it turns of a blackish purple colour. If the pure acid is used, care must be taken that it contains no sulphuric acid.

When crystallized, it forms five-sided obtruncated pyramids. It may, therefore, be distinguished from the tartrate and benzoate of tin.

### *Rheumate of Antimony.*

The rheumic acid has very little action on the oxide of this metal. However, it dissolves it in a slight degree, which may be shown by chemical tests. Its crystals have not been examined.

### *Rheumate of Mercury.*

This acid has a very considerable affinity for mercury, and acts upon it in its metallic state; even in the diluted way, it exists in juice. This was the first test that assured me it was different from the citric acid. On all its oxides it has similar effects, changing their colour before dissolving them.

The oxide of mercury may be again precipitated in the form of a yellow powder. In its crystals it exactly resembles those of water, shooting out in a most beautiful manner. They possess a shining appearance, and resemble in some measure the crystals of oxalic acid.

When exposed to the sun, they do not change colour. They may, therefore, be distinguished from oxalate of mercury, tartrate of mercury, and citrate of mercury.

### *Rheumate of Copper.*

The rheumic acid dissolves the oxide of copper; and, when evaporated, it forms a powder of a dark green colour, which takes a considerable quantity of water to dissolve it. It may, therefore, be distinguished from oxalate and benzoate of copper.

*Rheumate of Bismuth.*

The rheumic acid dissolves the white oxide of bismuth very readily, and forms with it small crystals. It may, therefore, be known from tartrate of bismuth.

*Rheumate of Arsenic.*

This acid dissolves the white oxide of arsenic, and forms crystals of a pretty large size. They seem to be irregular cubes; and, therefore, can be distinguished from benzoate of arsenic and oxalate of arsenic.

*Rheumate of Lead.*

With this metal it forms an insoluble compound; and from my observations I should conceive this to be one of the best tests of lead existing in any fluid.

I have not yet fully investigated the action of this acid upon the various earths. With lime, however, it forms an insoluble compound; nor does it become soluble when an excess of acid is added, as the supercitrate of lime does. It also dissolves magnesia and argile; but the crystals formed by these I have not yet examined.

The stronger acids, when distilled with it, have the effect of charring it.

I have now brought this paper to a conclusion; and though the investigation is still in its infancy, it may perhaps have the good effect of stimulating some one to prosecute this very curious, and, I may add, very useful subject.

J. Henderson.

## Article III.

*Answer to Mr. Davenport's Defence of Prevost's Theory of Radiant Heat.* By John Murray, M.D. Lecturer on Chemistry in Edinburgh.

(To Dr. Thomson.)

Sir,

I had written the following short reply to the observations of Mr. Davenport in your number for April last, on the subject of the radiation of cold; but from circumstances it was not transmitted at the time I intended. I have now to request the favour of its insertion.

For the general answer to the original argument I must refer to my former paper. (*Annals of Philosophy*, vol. vii. p. 223.) Mr. Davenport's last observations are not so much, I conceive, in reply to what I had offered, as a new statement of the argument under

another form. If a metallic canister, he observes, with one surface clean, another blackened, be so placed that thermometers are opposite to each surface at equal distances (the canister being empty, and of the same temperature with the bodies around) neither of the thermometers indicates any variation; though at a given temperature the blackened surface radiates much more caloric than the other. And this circumstance, it is added, I do not account for. Now in Pictet's theory, which is the one I have maintained, it is accounted for by the very principle of the theory, that from bodies which are at the same temperature with the contiguous medium, and the bodies around, there is no radiation of caloric. In the case stated, therefore, by Mr. Davenport, there can be no effect produced on the thermometers.

Supposing, however, Prevost's principle to be granted, that caloric is radiated from bodies according to the temperature, and that on this the effect in the experiment of the apparent radiation of cold depends; when the canister is filled, Mr. Davenport adds, with a freezing mixture "has not the blackened surface lost a part of its intensity of radiation?" No doubt, according to the theory it has. "Has the polished surface," he continues, "lost its power of reflection." It certainly has not. "It follows, then," he adds, "that unequal diminutions have been imposed on those powers of returning heat to the thermometer, which before were equal; the radiating surface has lost more than the reflecting surface, and its thermometer receives less return than that on the reflecting side." It is here, I conceive, that the fallacy lies. The clean surface has also had its intensity of radiation proportionally reduced. And *at any temperature the blackened surface radiates more caloric than the clean surface does at the same temperature.* The former, therefore, returns more caloric to the thermometer than the latter; and hence ought to produce less cold; or at least, allowing all the effect that can be ascribed to difference of reflection, no cause is assigned why it produces a greater degree of cold.

I submit this, with much deference, to Mr. Davenport's attention, and remain,

With much respect,

Sir, your most obedient servant,

J. Murray.

*Edinburgh, July 20, 1816.*

## Article IV.

*A Comparison of the Old and New Theories respecting the Nature of Oxymuriatic Acid, to enable us to judge which of the two deserves the Preference.* By Jacob Berzelius, M.D. Professor of Medicine and Pharmacy, and Fellow of the Royal Academy of Sciences at Stockholm.

(*Concluded from* p. 209.)

### II. FLUORIC ACID.

It was as little in the power of the new doctrine to produce a body similar to chlorine, as it was of the old doctrine to produce a body similar to oxymuriatic acid. The objection, which may be stated against both, is, that both suppositions may be transferred with facility from the one doctrine to the other.

Davy, in hopes of establishing his new doctrine the more completely, undertook to show that all the former experiments which he had performed respecting the reduction of fluoric acid were inaccurate, and that no traces of oxygen can be discovered in it: and he endeavoured to prove, by a set of very ingenious experiments, that fluoric acid contains a body analogous to chlorine, to which he has given the name of *fluorine*. Although, according to the old doctrine, as well as the new, it is very probable that such a body (a superoxide of fluoric acid) exists, yet it was not in Davy's power to obtain it. He was equally unsuccessful in showing that the experiments of Gay-Lussac and Thenard on the reduction of the base of fluoric acid are inaccurate.

I must here make the general remark that after we have discussed at some length the new doctrine, as far as it respects muriatic acid and its compounds, very little remains to be said respecting the other two acids; for if the new doctrine, as far as it regards muriatic acid, be inaccurate, the opinions respecting the nature of fluoric acid and iodic acid fall of themselves.

The peculiar compounds of fluoric acid are *fluoboric acid* and *silicious fluoric acid*, both of which are double acids. The new doctrine considers them as acids composed of *boron* and *silicium* combined with *fluorine*. The boron and the silicium are the bases of the acids, and the fluorine acts the part of oxygen. They can only combine with ammonia without being decomposed. The new doctrine assumes here, also, what is not very probable, that in this ammoniacal salt the silicium exists in the metallic state without containing any combined oxygen. The same observations, which I made when speaking of the chlorides of phosphorus, apply in the present case.

The fact that when neutral fluate of potash is precipitated by muriate of glucina a considerable quantity of potash is set at liberty

might embarrass any one who was not an adept in the new doctrine. But as the supporters of this doctrine consider compounds as salts, or chlorides, or fluorides, according to the necessity of the doctrine, and even venture to affirm that the hydro-fluates, like the hydro-sulphurets, are undecomposed by heat, it would be difficult from this phenomenon to draw a consequence which would be acknowledged to be inconsistent with the accuracy of the new doctrine.

Most of the combinations of fluoric acid, if we view them according to the new doctrine, are as inconsistent with the laws of chemical proportions as the submuriates. I have, for example, analyzed the *topaz* of Brazil, of Schneckenstein, and of Fahlun, with the greatest care, and from all have obtained the same result. I found it a compound of one integrant particle of subfluate of alumina (in which the earth contains twice as much oxygen as the acid) with three particles of siliciate of alumina (in which the alumina and silica contain equal quantities of oxygen). But if, instead of subfluate, the compound contains a fluoride of aluminum, then the oxygen in the oxidized portion of aluminum must be to that in the silica as four to three. Here we have deviation from the rule as in the aqueous submuriates. In the schorlous beryl of Altenberg I found one particle of neutral fluate of alumina combined with three particles of siliciate of alumina; so that this compound, according to whatever doctrine we view it, corresponds with the laws of definite proportions.

To dwell larger upon fluoric acid is quite unnecessary, as far as concerns the present treatise.

### III. IODINE.

Iodine was discovered just at the time when the new doctrine began to spread; and as its properties were investigated and described by those only who had embraced the new doctrine, the consequence has been that every thing known respecting it has been stated according to the views and language of the new doctrine. Chemists have never given themselves the trouble to inquire whether the old doctrine by this discovery has lost or gained in probability.

It has been taken tacitly for granted that the phenomena which iodine presents can only be explained by the new doctrine: and the great degree of attention which iodine, as a new body, has drawn, has given to the new doctrine a degree of publicity which, without its assistance, it probably never would have reached. This circumstance has certainly induced many chemists to abandon the old doctrine, and to be satisfied by evidence which in other circumstances would not have been considered as convincing. The celebrated Vauquelin, whose merits as a chemist are above my praise, affords a remarkable example of this. He found that when the compound of iodine and phosphorus is put into water, or when iodine and phosphorus are made to act upon each other under water, phosphoric acid and hydriodic acid are formed. "This," says he, "can be explained by the decomposition of water." Hence it is obvious

that he considered it as necessary to adopt the new doctrine. In other circumstances it would not have escaped the sagacity of this chemist, that if iodine be considered as an oxide analogous to oxymuriatic acid, this appearance must take place by the decomposition of the hyper-oxide, which loses its oxygen while the phosphorus is converted into an acid by combining with this oxygen. The water unites without decomposition with the acids, which are converted into the state of hydrous acids.

Even when the first intelligence respecting iodine was published, it was considered as a simple combustible substance, similar in appearance to galena or sulphuret of antimony. It was stated to have the greatest analogy with sulphur and chlorine, as, like them, it forms a coloured gas, and combines with hydrogen and oxygen, constituting two different acids, &c.: and from all these analogies the consequence was drawn that the truth of the new doctrine could no longer be disputed.

But allowing the importance of these analogies, it cannot be denied, on the other side, that iodine has a greater resemblance to black oxide of manganese than to sulphur. Gaseous chlorine and iodine are not only more like nitrous acid vapour than to sulphur, but their peculiar smell has the greatest analogy to the same acid. It is clear, therefore, that if the new doctrine, on the one hand, has gained by the analogy discovered between iodine and chlorine, the old doctrine has equally gained by the discovery of an acid capable, like the muriatic, of combining with different doses of oxygen.

As all the observations which I have made upon muriatic acid apply equally to iodine, a further comparison of the two hypotheses would be superfluous. But as the phenomena of iodine have hitherto been explained only according to the language of the new doctrine, I conceive that a short sketch of these phenomena, explained according to the principles of the old doctrine, may with propriety be introduced in this place. I have to regret that the distance of my abode from the centre of literary communications has put it out of my power to peruse all that has been written on iodine, and in particular the half of Gay-Lussac's treatise. Several of the phenomena in consequence remain unknown to me.

### 1. *Iodic Acid.*

In several varieties of kelp, besides the carbonate of soda, and other known salts, there is contained a new soluble salt, which is a compound of soda and a new acid, the *iodic* acid. This salt is found in the mother ley after all the crystals readily formed have been separated. The iodic acid is easily separated by means of stronger acids. But as it readily combines with oxygen, and is converted into a hyper-oxide, similar to oxymuriatic acid, it is separated by the easily decomposed acids (likewise by sulphuric acid) in the form of a hyper-oxide, in consequence of which the separating acid is partly deoxidized. As this is an easy method to

purify the acid from other bodies, the following formula for procuring this oxide in a pure state has been devised. The mother ley is evaporated to dryness, and the residue mixed with sulphuric acid in a retort with a long beak. By the application of a gentle heat the muriatic acid which it contains is driven off. Black oxide of manganese is then added, and the mixture is distilled. When a moderate heat is applied, the retort becomes filled with a violet-coloured gas, which condenses in the upper part of the retort in the form of metallic crystals. In this case the iodic acid, like muriatic acid, is changed into a hyper-oxide by the excess of oxygen in the black oxide of manganese. We shall call, therefore, the substance found in crystals in the upper part of the retort hyper-oxide of iodine. To convert it into iodic acid, it is mixed with water, and a current of sulphureted hydrogen gas passed through the liquid. The hydrogen reduces the hyper-oxide to an acid, while the sulphur precipitates. When the liquid appears colourless, all the hyper-oxide is converted into acid. When the acid is filtered, and distilled in a vessel destitute of oxygen, the greatest part of the water passes over, and the acid becomes much more concentrated. When this hydrous iodic acid is exposed to the air, it is easily converted into a hyper-oxide; and, as the acid dissolves the hyper-oxide, a reddish brown solution is formed, from which the oxide cannot be separated by boiling, though it be more volatile than the acid.

Iodic acid forms neutral compounds with water and the different salifiable bases, from which it has not hitherto been possible to separate iodic acid in an anhydrous state. 100 parts of pure acid saturate a portion of base, which contains 6·851 oxygen.* This acid, therefore, has a very small capacity of saturation. When iodic acid combines with as much water as is necessary to saturate it, we obtain an acid gas very similar to muriatic acid gas. We obtain it by sprinkling the anhydrous compound of phosphoric and iodic acids with water. The acids combine with water, and experience an elevation of temperature; by this means they are separated and converted into hydrous acids, in which state the iodic acid is gaseous. 100 parts of iodic acid require 7·767 parts of water. The specific gravity of this gas is 4·443. It is rapidly absorbed by water, and the saturated solution is a smoking, colourless, very acid liquid. This concentrated acid boils at the temperature of 257°, and has the specific gravity of 1·7. Sulphuric acid, nitric acid, oxymuriatic acid, several salts, and the oxides of iron, are deoxidized by it, while it is converted into a hyper-oxide.

Iodic acid dissolves different metals, with the evolution of hydrogen gas. It readily combines with oxides, and forms both neutral and subsalts. The iodates have a peculiar tendency to form neutral and sub-double salts. Several of the latter are soluble in

* According to Gay-Lussac's determination that 100 parts of iodine form a neutral iodate with 26·225 parts of zinc.

caustic alkalies, without undergoing decomposition. In other respects the iodates possess the characters which belong to the salt of each basis. Thus the iodate of iron is greenish, and has an astringent sweetish taste. The iodate of zinc is colourless, and has an astringent or metallic taste. Several of them are more volatile than the salts composed of the same bases combined with other acids. This is the case with iodate of potash. Others, as the iodate of lime, are decomposed by heat, because the acid combines with oxygen, and separates. But this does not take place in close vessels. But this is not the place to give a particular description of each of these salts.

### 2. *Hyper-oxide of Iodine.*

The hyper-oxide of iodine is obtained when iodic acid is oxidized by black oxide of manganese, nitric acid, oxymuriatic acid, and other easily deoxidized bodies. It is precipitated from the solutions in the state of a brown powder, and may be collected and dried upon the filter. It melts at the temperature of 224°; and, on cooling, assumes the form of a dark grey foliated substance, having a fatty lustre, and easily reducible to powder. It is a nonconductor of electricity. At the temperature of 347° it is converted into a beautiful violet-coloured gas, possessing the properties of oxymuriatic acid, but much weaker; and again condensing on cold bodies in the state of blackish-grey, metallic-looking, crystals. It is easily distilled over with water at a moderate temperature, as is the case with most volatile bodies. In the open air it is gradually, but very slowly, volatilized. Cold water dissolves only a very small portion of it, and assumes a reddish colour. When the solution is exposed to the solar rays, it is gradually discoloured, the oxide being converted into iodic acid and hyper-oxiodic acid, which dissolve in the water. If water contains in solution either an iodate, or any other salt, as sal-ammoniac, nitrate of ammonia, it becomes capable of dissolving a notable quantity of hyper-oxide of iodine. But this must be considered as a mere solution, and not as a chemical combination of the hyper-oxide of iodine with the salt. Finally, this hyper-oxide is distinguished by this circumstance, that its acid has a much greater affinity for oxygen, and a much weaker affinity for saline bases, than muriatic acid has.

Sulphur deoxidizes this hyper-oxide at a moderate temperature, and is converted into iodate of sulphur. When the compound is heated, the acid reduces the oxide of sulphur, and flies off in the state of hyper-oxide, leaving the sulphur behind.

Phosphorus decomposes hyper-oxide of iodine, and forms different compounds, according to the proportion of phosphorus employed. One part phosphorus and eight parts hyper-oxide of iodine form an iodate of phosphorus, which has a yellow colour, and is decomposed by water, by which we obtain hydrous iodic acid, and (the oxide of phosphorus being decomposed) phosphorous acid and phosphorus. When a greater proportion of phosphorus is employed, the

excess immediately separates from the compound, and we obtain phosphorus in the red state to which it is converted by exposure to the rays of the sun. If we take one part phosphorus and 16 parts hyper-oxide of iodine, we form a compound of anhydrous, phosphorous, and iodic acids. If we take one part phosphorus and 24 hyper-oxide of iodine (that is, $1\frac{1}{2}$ as much),* we obtain phosphoric and iodic acids in the same anhydrous state. When water is added, it combines with the acids, the compound is decomposed, and the acids separated in a hydrous state. If too much hyper-oxide of iodine has been employed in the last combination, it dissolves in the hydrous acid, and colours it. The addition of a little phosphorus destroys the colour.

The greater affinity of iodic acid to oxygen, and the smaller to bases, indicates that hyper-oxide of iodine is capable of combining with several bodies, by which oxymuriatic acid is instantly decomposed. Hence this hyper-oxide unites with various bases, in which combinations it exists as a hyper-oxide, as with ammonia, lime, magnesia, and probably with others. The compound with ammonia is black, and is produced without decomposition from some elastic solutions. As the hyper-oxide has a weaker affinity for ammonia than water has, the compound is decomposed by water, liquid ammonia is formed, by which the hyper-oxide is converted into iodic acid, the hydrogen of a portion of the ammonia uniting with the oxygen of the hyper-oxide, and forming water. The azote thus set at liberty decomposes another portion of the hyper-oxide, and forms an insoluble, pulverulent, black compound of *nitrous* acid and *iodic acid*, both in an anhydrous state. This double acid has the property of being decomposed with an explosion in a still higher degree than the corresponding compound of nitrous acid and muriatic acid. In a liquid state this body is gradually decomposed spontaneously, azote is disengaged, and hydrous iodic acid and hyper-oxide of iodine formed.

Hyper-oxide of iodine combines, likewise, with various vegetable bodies, as sugar, starch, gum, without undergoing decomposition; just as we know that the same vegetable bodies can combine with other binary oxides, as the oxide of lead. At a high temperature, by dry distillation for example, these compounds are decomposed, the hydrogen decomposes the hyper-oxide, and the product of the distillation contains iodic acid.

### 3. *Hyper-oxiodic Acid.*

When the hyper-oxide of iodine is placed in contact with oxymuriatic acid gas, a combination takes place, and a yellowish, very

* These numbers are taken from Gay-Lussac. They can scarcely be accurate. But if the relative proportion of 16 and 24 be correct, as can scarcely be doubted, it follows that our estimate of the constituents of the acids of phosphorus is incorrect, as I have long suspected.

acid body, is formed, which volatilizes in the open air. If there be an excess of oxymuriatic acid gas, a part of it is dissolved. This body is a compound of hyper-oxiodic acid and muriatic acid, which is evolved because the hyper-oxide of iodine decomposes the oxymuriatic acid. If an additional portion of hyper-oxide of iodine be mixed with the double acid, it is dissolved, and the solution acquires a dark orange colour, the intensity of which increases with the proportion of hyper-oxide of iodine. When the double acid is heated, the same solution is obtained, because the muriatic acid again absorbs oxygen from the hyper-oxiodic acid, reduces it to hyper-oxide of iodine, and flies off in the state of oxymuriatic acid. If the double acid be dissolved in water, and saturated with caustic potash, we obtain muriate of potash and hyper-oxiodate of potash. If there be a portion of hyper-oxide of iodine in solution, it is precipitated by the first portions of the alkali, but is soon redissolved. Sulphate of indigo is deprived of its colour by the double acid; and the solution then contains only common iodic acid and muriatic acid.

When hyper-oxide of iodine is thrown into caustic potash, a colourless solution is obtained. In this case the hyper-oxide is decomposed in the same manner as the oxymuriatic acid. The excess of oxygen is concentrated in a smaller portion of the hyper-oxide forming hyper-oxiodic acid. while the greater portion is converted into iodic acid. The hyper-oxiodate of potash precipitates from the liquid in the state of a difficultly soluble powder. Soda, lime, barytes, and strontian, produce the same phenomena, with all of which hyper-oxiodic acid forms difficultly soluble salts. When these salts are heated, they are converted into common iodates, while a quantity of oxygen gas is given out. In order to obtain the greatest quantity of hyper-oxiodic acid from a given portion of hyper-oxide of iodine, the best method is to convert the hyper-oxide into an acid by means of oxymuriatic acid, and then to saturate the double acid with the requisite base. The hyper-oxiodic acid may be obtained in a crystallized form by decomposing hyper-oxiodate of barytes by means of diluted sulphuric acid. The liquid is then to be concentrated by evaporation, and allowed to crystallize. The crystals are colourless.

When a concentrated solution of iodate of magnesia is mixed with a solution of hyper-oxiodate of magnesia likewise concentrated, a flea-coloured matter falls down, which is a compound of hyper-oxide of iodine and magnesia. Lime and strontian produced something similar, though in a less degree. The property seems to belong to the weaker bases. Hence the hyper-oxiodates cannot be obtained by treating them directly with hyper-oxide of iodine.

* * * * * *

The greatest part of the preceding sketch has been taken from Gay-Lussac's excellent treatise. As I have hitherto seen only one half of it, many defects must unavoidably occur in this attempt of

mine. But every person may easily fill them up for himself according to the language of the old theory.

*Conclusion.*

To decide the question respecting the three bodies, of which I have been treating, completely in favour of the old doctrine, it would be undoubtedly necessary to produce the combustible bases of muriatic acid, iodic acid, and fluoric acid, in a separate state. But at present this is not possible. But how can it be fairly concluded from this that such a decomposition may not hereafter be in the power of chemists; and how can such a circumstance be objected as a decisive proof against the accuracy of the old doctrine? Suppose that in the year 1806 a chemist had asserted that the alkalies were simple bodies, and not (as Lavoisier had conjectured from their analogy with other salifiable bases) oxides, and had supported this assertion by observing, that hitherto it had been impossible to reduce the alkalies, it will be admitted that such a chemist would have argued erroneously, although his assertion would not have been refuted so completely by alleging the arguments of Lavoisier as it would have been by reducing the alkalies. This example may be applied to the present philosophy of the supporters of the new doctrine, as they, disclaiming all analogy, require the reduction of muriatic acid as the only thing capable of refuting the new doctrine. In the year 1806 we were not aware of the reducing power of Davy's electrical piles and troughs; and who at present can estimate the energy of an electrical battery a thousand times greater in all dimensions than those which he employed.

I think, then, that though it be not in our power at present to prove and establish our position by experiment, it ought not to be permitted in chemical philosophy to build any thing on this impossibility, nor to lay aside as false and absurd what appears exceedingly probable in other points of view, and because it is not in our power to allege positive experiments, to allege that our opinions are destitute of all positive evidence whatever. We surely should take such views of the subject that posterity (as the progress of experimenting will always bring new facts into view) will rather be disposed to confirm than refute our present conjectures. This can only be done by carefully studying analogy, and confining ourselves to conjectures which are conformable to those parts of chemistry which we consider as established. Whoever lays hold of a particular phenomenon, and considers every thing else, how probable soever, provided it be not established by actual experiment, as inaccurate, merely that he may establish a new hypothesis, however little it may accord with the rest of chemistry, is in danger that some other chemist shall be more fortunate than he in his view of the phenomenon, and that posterity shall consider him, not without justice, as short sighted in his views.

Since Davy has discovered that the alkalies and alkaline earths are true oxides, we conclude that alumina, zirconia, glucina, and

yttria, are likewise oxides, although hitherto nobody has succeeded, so far as I know, in his attempt to separate oxygen from these bodies. Yet no chemist has any doubt about the accuracy of the conjecture, as he sees the analogy between these bodies and the oxides of zinc, manganese, cerium, &c. But have we less reason, from the still greater analogy of muriatic acid, iodic acid, and fluoric acid, with sulphuric acid, nitric acid, and phosphoric acid, to draw the consequence that the former as well as the latter acids are composed of a combustible basis united to oxygen, although hitherto we have not been able to reduce them? Or can we with propriety affirm that we can make no further progress in this subject, and that our posterity never will be able to reduce these bodies by methods at present unknown to us? I do not believe that any philosophical chemist is of this opinion.

What I have said respecting the disputed doctrines concerning the nature of muriatic acid, fluoric acid, and iodic acid, seems to me sufficient to determine the choice of the unbiassed reader. I have always spoken in favour of the old doctrine, because I conceive I have shown that the new doctrine neither agrees with the electro-chemical theory, with the laws of affinity, nor with the doctrine of definite proportions. Nor does it in general accord with the other parts of chemical science, in so far as under this name we comprehend a system of propositions which agree completely with each other. But whoever has no reason to consider the general doctrine of chemistry as false must reject every particular doctrine which is inconsistent with it. If, therefore, chemical theory, as far as it is founded on electro-chemical discoveries, and on the doctrine of chemical proportions, be correct, it follows as a consequence that the new doctrine must be inaccurate.

I shall equally admit the inaccuracy of the old doctrine whenever any person shall produce an experiment respecting muriatic acid, fluoric acid, or iodic acid, which cannot be explained by that doctrine so as to accord with the other parts of chemical theory. But I will never declare myself an advocate for the new theory till that doctrine be shown to agree completely with the other branches of science, which has been built upon the ruins of chemical theory overthrown by it. For I demand imperiously of such a chemical explanation that it shall agree with the other parts of the science, and incorporate itself with it. Unless this be the case, I must reject it, as the admission of it would of necessity occasion a revolution in those branches of theory which are inconsistent with it.

I conclude with requesting chemists to pay attention to what I have said. If the mistake, contrary to my opinion, should be on the side of the old doctrine, perhaps it would be a task not unworthy of the attention of a supporter of the new doctrine to take the trouble to place the facts noticed by me in a better point of view, and to render the arguments in favour of the new doctrine so clear that they shall not fail to draw conviction after them.

## Article V.

### *Answer to Mr. Longmire's Objections to Sir H. Davy's Lamp.* By J. G. Children, Esq. F.R.S.

(To Dr. Thomson.)

DEAR SIR,

Knowing, from the conversation I had with you on the subject the last time I had the pleasure of meeting you,* that you do not entertain the same high opinion of the excellence of Sir H. Davy's lamp that I do, I am the less surprised at meeting in the last number of your *Annals* the *remarks* of Mr. John B. Longmire. In his opening paragraph, Mr. Longmire deprecates personal attacks. Of such I shall hardly be accused, as I am not aware that I ever had the honour of seeing your Correspondent; and as to his remarks, I certainly receive them with at least *as much good will* as they are given.

Mr. L. next proceeds to describe the lamp; in doing which he might as well have remembered to mention the inclined tube, for the supply of fresh oil, and the contrivance for occasionally trimming the wick, without, in either instance, making it necessary to open the lamp; both circumstances of material importance to the perfection of the instrument. He then describes the operation of the lamp, and admits that inflammable gases cannot be set fire to by means of it *when newly made*; "but when its materials have *begun to wear*, an ample source of accident is opened." Before I go further, I shall take the liberty of quoting a passage from a paper in the last number of the Philosophical Magazine, by Mr. I. Murray; whose opinion on scientific subjects, especially in the department of chemistry, few will feel inclined to treat lightly. He says, "Parallel bars I found prevented the communication of flame with equal facility as wire gauze. The interval must not exceed $\frac{1}{8}$th of an inch. Here, therefore, is a limit pointed out to us. If the meshes are $\frac{1}{16}$th of an inch apart, no danger would occur, should an accident break down the alternate one. When the bars are crossed by others at right angles, it constitutes a double security."† Now the meshes in Sir Humphry Davy's lamp are only $\frac{1}{20}$th of an inch, as Mr. Longmire states, asunder; consequently every alter-

* Since the conversation to which Mr. Children alludes, I spent a night at Newcastle, and took the opportunity of making some inquiry into the present state of the coal-mines in that neighbourhood. From the information which I received, I am satisfied that Sir H. Davy's lamp is in general use, that it serves the purposes of the colliers, and that hitherto it has been employed with perfect safety. I am happy, therefore, to inform my friend Mr. Children that my opinion of the safety lamp is now as favourable as his own.—T.

† Mr. Murray, in a subsequent part of his communication, states that flame will not kindle gunpowder. This is a mistake. If grains of gunpowder be suffered to fall through the flame of a spirit lamp (without touching the wick), many of them will take fire and explode.

nate wire might be removed, without reaching the maximum of safe distance. I humbly submit, therefore, that the lamps must have a little more than *begun to wear* before danger can ensue from that cause. If the miner *will use* his lamp after it is worn out, that is no fault of Sir Humphry Davy's. So much for argument No. 1 of this *controversialist*.

To proceed: "It would appear," says Mr. L., "that even the construction of the lamp containing in itself the seeds of future uncertainty, is a conclusion which may be drawn from Sir Humphry Davy's own words." I suppose Mr. L. means to say, in this very obscure sentence, which can scarcely be called English, that from Sir H. Davy's own account the construction of the lamp is imperfect. Had Sir H. Davy ever asserted its absolute perfection, he would have merited Mr. Longmire's attack; but, on the contrary, he candidly states those particulars in which he considers it imperfect, and suggests the best means that occur to him to remedy the defects. I shall, however, venture to go further, and assert, what Sir H. Davy's delicacy would not permit him to do, that this lamp is for all practical purposes *as perfect*, or *more so*, as the most approved instruments employed in art, science, or the common operations of life, usually are, especially at their first introduction. It is not to be expected that absolute perfection is attainable in this, more than in other things; and so far, I grant, *perfect security* is a want which the colliers will probably never get over. But it is not in the principle or construction of the lamp, but in the materials, of which it is not of necessity made, that the very small imperfections that can be attributed to it consist; and the unqualified testimony of Mr. Buddle, and a large number of the most respectable practical miners, to the perfect success which has attended every experiment made with the lamp *in the mines*, and its *general use* for the last three months, is a pretty sufficient answer to Mr. Longmire's assertion, "that *very few*, if *any*, practical miners will come before the public, and pledge their credit on the unrestricted assertion that Sir Humphry Davy's lamp will give the perfect security so much boasted of." Still, Mr. L. will say, I suppose, that wire (*copper*, or *brass*, I believe, and not *iron*, as he states,) will break and burn, and the lamp will wear out; and, therefore, it is an imperfect invention, and little worth. According to this reasoning, we should avail ourselves of no invention the ingenuity of man has supplied us with; for none are without the same faults. The compass, and the ship that steers by it, should equally be abandoned; for both are liable to destruction! But, after all, what do these great imperfections, which Mr. Longmire so triumphantly lays hold of, from 'Sir Humphry Davy's own confession,' amount to? Why to this: that the lamps must be trimmed every day (as most lamps must), and that it will be well to employ particular persons for this work, who shall, besides, so secure the lamp in its place, when trimmed, that the carelessness, which long security is apt to induce, shall not expose the authors of it to destruction. I see nothing very

militant against the *common sense perfection* (to use such an expression) of the lamp in all this; nor does it strike me that an Argus will be necessary to inspect them; for, though Mr. L. has thought it worth his while to count the number of apertures (of which he informs us there are between 14,000 and 16,000 in each lamp), yet I do not think the inspector need be so curious; but rather conceive that a very cursory glance will be sufficient to detect any broken wires, or enlarged spaces. But suppose that were necessary, a very simple method may be adopted, which would answer the purpose, and save the inspector's eyes. A parcel of small spheres (patent shot, for instance) a very little larger than the mesh of the wire-gauze, may be put into the cylinders, so as to spread over its whole interior length, and the cylinder then turned gently round in a horizontal position, when the escape of some of the shot through the enlarged spaces would immediately detect them. But the screw, it seems, is a sad stumbling block to Mr. Longmire, who professes his inability to advise the inspector, *to a certainty*, how to *manage it*; and even Sir Humphry himself, he says, "will be puzzled to do it." I should suppose Sir Humphry would advise the inspector to *turn* it, as the simplest mode of managing this difficult operation. But perhaps it is not the mere screwing on the bottom of the lamp that Mr. Longmire means (though certainly all his words express), and this sentence is intended to be taken in connexion with those that follow, in which he complains that if the "lamps are to be padlocked by one person, when a collier loses his light he has to travel in the dark (probably!) till he find the man with the key; that is to say, he is to travel, if he can find his way in the dark!" (which, by the bye, in a former sentence Mr. L. has told us he can do). "If they are not padlocked, and every miner has the management of his own lamp, it is not to be supposed that every one will always go to the place fixed on to light the lamp; when they are in separate workings, and alone, they will light them in less distant places, which they think suitable, but which will not be *always safe*. The lamp, *therefore* (mark you his absolute "*therefore!*") as to construction, neither yields a desirable degree of convenience, nor absolute security." The second objection is already answered. Sir Humphry was aware of the impossibility of preventing carelessness, and suggested the padlock to obviate its mischiefs. As to the man's being left in the dark, that cannot happen from the lamp's going out for want of fresh oil, or occasional trimming of the wick; both of which are ingeniously provided for, as I have already noticed, without opening the lamp; nor from its being extinguished by a great accumulation of inflammable gas; except, in both instances, through his own negligence; for, in the latter, the miner will have timely warning to turn back by the enlarged flame, which it is his own fault if he will not attend to. A sudden blast, or other accident, may certainly sometimes leave him, if alone, in darkness, and then he must find his way back as well as he can, which, I dare say, his knowledge of the

passages of the mine seldom makes a very dangerous or difficult operation. But the much greater probability is, that when those accidents happen there will be more than one lamp in company, and it must be singularly unfortunate if they are all extinguished together. I shall not follow Mr. Longmire through all the *ifs* his fears and ingenuity have conjured up of possible cases in which the structure of the lamp is liable to injury. The principal one (enlargement of the spaces by wear and burning) is already answered, by the assistance of Mr. Murray: and as to the rest, I shall only say that, to the causes of derangement he enumerates, I wonder he has not added one more *if*, viz. *if* the miner takes out his knife, and cuts the wire-gauze to pieces.

There is rather more appearance of argument in Mr. Longmire's next paragraph, in which he suggests the preference of the steel mill to the safe lamp, under certain circumstances; especially in the search for sufferers, whom an explosion (produced, for instance, by the fire of the ventilating furnace) has buried. I am ready to grant that the feeble light of the steel mill is better in such circumstances than the *no light at all* of a lamp which would not burn. But if Mr. Longmire means to contend for the general preference of steel mills over the safe lamp, I can only say that, according to the best information I have received on the subject, he must prefer darkness to light. To conclude, in the style of Mr. Longmire, but with very opposite impressions, I am of opinion that the reputation which the wire-gauze lamp enjoys at present is *greatly less* than it will hereafter obtain, when time shall have silenced the objections (I will not call them by a harsher name) that at present may *perhaps* occasion doubts in some minds. The beauty and novelty of the invention, and, let me add, the acuteness of induction by which it was brought, step by step, to its present excellence, by its admirable author, are truly fair media through which to contemplate it; but that the obstacles it may meet with in the mine have been overlooked, I deny. But I have done; and willingly meet Mr. Longmire on this point; the same, indeed, to which in our conversation we agree to refer it—the experience of a few years; when, I have no hesitation in expressing my confidence, that *its real utility* will assign it as high a place in the estimation of miners as its incomparable ingenuity has given it in that of science.

I remain, dear Sir, sincerely yours,

*Saleham, July* 7, 1816. JOHN GEO. CHILDREN.

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(To Dr. Thomson.)

DEAR SIR,

Since I wrote to you respecting Mr. Longmire's remarks on Sir Humphry Davy's lamp, I have read Mr. Buddle's letter to Sir Humphry, in the Journal of Science and the Arts of the Royal Institution, in which I perceive that I was mistaken in supposing the

lamps are made solely of copper and brass wire-gauze. All that I have seen hitherto have been made of those materials, and I was not aware that iron wire had been employed. The fact, however, of so oxidable a metal being completely adapted to the purpose, is additional testimony of the safety of the instrument; and should any doubts remain in the most sceptical minds of the efficacy of the lamp, I think a perusal of Mr. Buddle's letter, and the annexed observations by Sir Humphry, must entirely do them away; unless obstinate prejudice, or a worse feeling, render them insensible to the impressions of truth.

I am ever, dear Sir, very truly yours,

*Saleham, July* 10, 1816. J. G. Children.

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## Article VI.

### *On Safety Lamps for Coal Mines.* By J. H. H. Holmes.

It is unnecessary to make any further remarks than what have already been made upon the importance of these inventions, and the great necessity of exposing them to the examination and scrutiny of the public. Unconscious of any prejudiced motives, or any other desire than that of promoting the interests of mining, and the security of the miners, I observe with indifference the pointed aspersions of other writers, and readily attributing the warmth of some to their tenacity for a premature reward, I dismiss their observations, as natural in their origin, but ineffectual in their application.

After the dispassionate and sensible remarks of J. B. Longmire, Esq.* it is almost unnecessary to start more real objections to the wire-gauze lamps than what he has therein mentioned; but I have previously disputed their security, and feel myself pledged to society to substantiate the objections I have made upon the best possible testimony. Indeed, were I not to do this, my motives might be attributed to *malevolence*, or my observations to *ignorance*; but, discarding any *merit* of these epithets myself, I regret that they should have been thrown out against any person who has commented upon safety lamps.†

In this country every individual has the privilege of assenting to, or dissenting from, any subject or matter according to his own opinion; and so long as his arguments are founded upon facts within his own knowledge, the opposition of interested parties is nugatory and unavailing.

I should have but little, if any hesitation, in proceeding through a mine rendered dangerous by fire-damp, with the gauze lamp, providing there was the probability of my proceeding without many

* *Annals*, July, 1816, p. 31. † Vide Till. Phil. Mag. July, p. 55.

accidental circumstances, which might break through the wire, or without encountering some powerful blowers of gas, or strong currents of air; as the former would most likely force the inflammation of the gas through the wire, particularly after the cylinder got warm, and the latter would cover the lamp with a coating of coal dust, and render it liable to communicate explosion; for in addition to my experiments,* I have been informed by Mr. Wood, a most respectable viewer, that two wire-gauze lamps were procured, by way of trial, for Lady Vane Tempest's mines, but were found so ineffectual, that they are totally disused; the gas, when inflamed within the cylinder, was forced through the wire apertures by a current of gas issuing from a gas pipe, at which the experiment was made, and the light was so feeble that it could not be kept in down one of the shafts.

Mr. Buddle, in speaking of the wire lamps, says, "I have not, however, thought it prudent in our present state of experience to persist under such circumstances (where the explosive mixture is so high as to fill the cylinder with flames, and render it red-hot), because I have observed that in such situations the *particles of coal dust* floating in the air fire at the gas burning within the cylinder, and fly off in small bituminous sparks." †

I shall not remark upon the manner Mr. Buddle has endeavoured to palliate this danger; for however his friendly intentions may induce him to pass over real objections, his determination not to use them under such circumstances is a strong proof of their insecurity; and yet the inventor, in his observations,‡ says, that no danger exists from the presence of coal dust, &c.: but I fear his experiments have been too much confined to the laboratory, without considering the air, currents, &c. &c. &c. of a coal-mine to their full extent. I am sorry that Sir H. Davy should have made me active in supporting my objections to his apparatus, as I would much rather bear testimony of his general abilities and distinguished eminence as a chemist and a philosopher.

Notwithstanding, however, all the security which has been stated to exist in the wire lamps, additional meshes or coverings of wire are now suggested, which certainly would not have been necessary had they possessed all the recommendations spoken of in the numerous accounts which have been published of them; and the cylinders of copper perforated with small holes was first adopted by Mr. Stephenson. With all these objections, the wire lamps have much merit; and had not the constructor endeavoured to enhance that merit by detracting from the value of other inventions, he would have stood much higher in my estimation.

Whatever may have been done by the abilities of those who have devoted their attention within the last year to mine lamps, nothing

* Vide *Annals*, for August, p. 129.
† Journal of Science and the Arts, No. ii. 1816, p. 303.
‡ Ibid. p. 306. Tilloch's Phil. Mag. for July, p. 84.

can exceed the merits of Dr. Clanny, who first, and so long ago, commenced the subject, without any thought or any wish about reward, except what the mind derives from having rendered a service to humanity. His exertions now, however, entitle him to that; and I am happy to find that there is a strong sentiment rising in his favour amongst the coal owners. His motives originated purely in benevolence; and the observation of Mr. Murray, in the last number of Tilloch's Magazine, that "science never shines with a sublimer lustre than when exerted in the cause of humanity," applies more properly to Dr. Clanny than to him to whom it is offered.

His last invention * is a singular apparatus for burning the gas through steam. In this lamp the air necessary for combustion passes, by means of tubes, through a cistern of steam, and the lamp continues to burn with a very brilliant light while surrounded by inflammable air, for the flame consumes all the gas attracted through the tubes, without any explosion or re-action; and there is no comparison between the brilliancy of its light and the dimness of others.

J. H. H. Holmes.

(copy.)—*Certificate.*

*Herrington, Aug.* 5, 1816.

I this day made some experiments upon Dr. Clanny's new steam lamp, at the Herrington Mill Pit, accompanied by Dr. Clanny, and made in the presence of Mr. Patterson, the engineer, and a great number of pitmen, &c.

The principle of security in this lamp was clearly demonstrated by applying the tube through which the air enters into the steam cistern to a leaden tube conveying a strong current of gas from the gas pipe of the mine. In previous experiments upon Dr. Clanny's insulated lamp, an explosion was always produced when the quantity of gas within the chamber had mixed up the atmospheric air to the firing point; but in this case the light continues to burn with an increased flame when the stream of hydrogen is not too strong; but when the air tube of the lamp is put close to the leaden pipe conveying gas from the mine, a bright blue flame extends itself for a moment within the lamp, and then quietly dies out, without any exterior communication, and without any explosion whatsoever; so that, in fact, this lamp burns the gas, and gives a more brilliant light than any other.

J. H. H. Holmes,
Anthony Brown,
William Patterson.

* See my Treatise on Coal Mines, p. 209.

P.S. When I went out, Patterson was below ground, working at the bottom of an engine pit, in a most dangerous mine, where no lights, except Dr. Clanny's, have been for years. He had one of the steam lamps with him, and expressed the greatest confidence in its security, and says it gives a very fine light. The wire lamp was repeatedly tried, but could not be made to keep in down the shaft.

J. H. H. H.

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## Article VII.

### *Solution of a curious Mathematical Problem.*

By James Ivory, Esq. F.R.S.

(To Dr. Thomson.)

SIR,

As I perceive you sometimes admit mathematical articles in your *Annals*, I have sent you a solution of a curious problem about curves. This problem is the subject of a memoir of some length, written by the celebrated Euler, and published in the Memoirs of Berlin for 1756. Euler is not the first mathematician who considered the problem; it had been proposed before, and many solutions had been given of it. But he is the first who entertained clear notions about the nature of it, showing that its general solution is impossible, and that it can be solved only in particular cases. This memoir is remarkable for that clearness which distinguishes all his writings, and for the admirable skill with which he manages the processes of analysis so as to accomplish the object he has in view. He follows a kind of induction, which is often a favourite method with him. The analysis given below is direct.

*Sept.* 7, 1816. J. I———.

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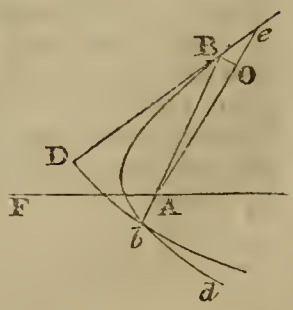
### *Problem.*




To find a curve such that if any straight line, B A $b$, be drawn through a given point, A, to meet the curve in two points, B and $b$, and tangents, drawn to the curve from these points, be produced to meet in D, the angle B D $b$ shall always be equal to a given angle.

Let F A be a fixed axis passing through A, and let $\varphi$ denote the angle F A B; then both the point B in the curve, and the position of the tangent B D, will depend entirely upon the variable angle $\varphi$. If, therefore, $\tau =$ tan. A B D, then $\tau = f(\varphi)$. Con-

ceive the line A B to revolve about A; then when $\varphi$ has increased to be equal to $\varphi + \pi$ ($\pi$ denoting half the circumference), A B will take the position A $b$, the tangent B D will have the direction $b\,d$, and the angle A B D will be changed into A $b\,d$. That this is the case will be more easily apprehended if we suppose A $b$ to continue to revolve till it comes to A B; for it is now clear that $b\,d$ will coincide with B D, and the angle A $b\,d$ with A B D. It appears, therefore, that the angle A $b\,d$ depends upon $\varphi + \pi$, in the same manner that A B D depends upon $\varphi$: consequently, if $\tau' =$ tan. A $b\,d$, then $\tau' = f\,(\varphi + \pi)$. Suppose $x =$ tan. $\frac{1}{2}\,\varphi$, then $-\frac{1}{x} =$ tan. $\frac{1}{2}\,(\varphi + \pi)$. In place of supposing that $\tau$ is directly a function of $\varphi$, we may suppose it to be a function of $x$; and then $\tau'$ will become the same function of tan. $\frac{1}{2}\,(\varphi + \pi)$, or of $-\frac{1}{x}$. Thus we shall have $\tau = f\,(x)$, and $\tau' = f\left(-\frac{1}{x}\right)$.

If $\lambda$ is any odd number, we may, for the greater generality, put $x =$ tan. $\frac{1}{2}\,\lambda\,\varphi$; for then $-\frac{1}{x} =$ tan. $\frac{1}{2}\,(\lambda\,\varphi + \lambda\,\pi) =$ tan. $\frac{1}{2}\,(\lambda\,\varphi + \pi)$, by dropping a certain number of whole circumferences. It may be observed that $f\,(x)$ must not denote any function whatever; but it must be restricted to such functions of $x$ as have one value only, corresponding to every value of $x$. The expression $f\,(x)$ must not involve radicals, or quantities depending upon the subdivision of angles; it must be a rational and integral function of $x$. This condition is necessary, in order that the line B $b$ shall meet the curve in two points, and no more; one on each side of the point A.

Let $a =$ tangent of the constant angle B D $b$; then, because B D $b =$ A $b\,d -$ A B D, we have $a = \frac{\tau' - \tau}{1 + \tau\,\tau'}$; therefore $\tau - \tau' + a + a\,\tau\,\tau' = o$; and by substituting the values of $\tau$ and $\tau'$,

$$f\,(x) - f\left(-\frac{1}{x}\right) + a + a\,f\,(x) \times f\left(-\frac{1}{x}\right) = o. \qquad (1)$$

This equation will determine the function $f\,(x)$; it remains to consider how the line A B is to be found. Let $e$ be a point in the curve indefinitely near B; join A $e$, and draw B O perpendicular to A $e$; put A B $= \rho$, A $b = \rho'$; then B O $= \rho\,d\,\phi$, O $e = \pm\,d\,\rho$, and $\tau =$ tan. A B D $= f\,(x) = \frac{\rho\,.\,d\,\phi}{d\,\rho}$. Thus we get

$$\pm\,\frac{d\,\rho}{\rho} = \frac{d\,\phi}{f\,(x)}\,. \qquad (2).$$

$$\pm\,\frac{d\,\rho'}{\rho'} = \frac{d\,\phi}{f\,(-\frac{1}{x})}\,.$$

The problem is now reduced to find what function will answer for $f\,(x)$ in the equation (1). This equation ought to subsist for every point in the curve; for the point $b$, as well as for the point B,

otherwise the points B and $b$ would be in two different curves, and not in two branches of the same curve connected by the law of continuity. Now the quantity which is $x$ at the point B becomes $-\frac{1}{x}$ at the point $b$; the equation (1) ought, therefore, to subsist both for $x$, and likewise when $x$ is changed into $-\frac{1}{x}$; that is, we ought to have both these equations, of which one belongs to the point B, and the other to the point $b$, viz.:—

$$f(x) - f\left(-\frac{1}{x}\right) + a + a\,f(x) \times f\left(-\frac{1}{x}\right) = o$$

$$f\left(-\frac{1}{x}\right) - f(x) + a + a\,f(x) \times f\left(-\frac{1}{x}\right) = o.$$

But these equations are different from one another, and they cannot both subsist generally for all possible values of $x$. In effect, by first adding, and then subtracting them, we get, $1 + f(x) \times f\left(-\frac{1}{x}\right) = o$, and $f(x) = f\left(-\frac{1}{x}\right)$: therefore, by substitution, $1 + \{f(x)\}^2 = o$, which is an impossible equation. No single curve can, therefore, be found, subject in all its parts to the law of continuity, that will satisfy the equation (1), and the problem cannot be generally solved.

The preceding reasoning hinges on this circumstance, that the functions $f(x)$ and $f\left(-\frac{1}{x}\right)$ are not alike concerned in the equation (1): but there are two particular cases that do not fall under the same argument, and which are, therefore, capable of solution. The first of these cases is when $a = o$, or when the tangents at the extremities of the line B $b$ are parallel. In this case the equation (1) becomes $f(x) - f\left(-\frac{1}{x}\right) = o$, or $f(x) = f\left(-\frac{1}{x}\right)$; whence by means of the equations (2) we get $\frac{d\rho}{\rho} = \frac{d\rho'}{\rho'}$. Now $\rho$ and $\rho'$ are like functions of $\phi$ and $(\phi + \pi)$, and they must have different signs, since the lines A B and A $b$ have always opposite directions: the equation $\frac{d\rho}{\rho} = \frac{d\rho'}{\rho'}$, therefore, requires that $\rho = -\rho$; and hence $\rho$ must be such a function of $\phi$ as retains the same value, and merely changes its sign, when $\phi + \pi$ is substituted for $\phi$. The curves in question are, therefore, such as have a centre, a class very well known.

The other case is when $a$ is infinitely great, or when the angle B D $b$, made by the tangents, is a right angle: and here the equation to be solved is, $1 + f(x) \times f\left(-\frac{1}{x}\right) = o$. It is evident that the product $f(x) \times f\left(-\frac{1}{x}\right)$ must be always negative, and hence $f(x)$ must be an odd function. Let $m$ and $n$ be both odd numbers;

we may suppose $f(x) = x^{\pm \frac{m}{n}} \times f^1(x^2)$; then $f\left(-\frac{1}{x}\right) = - x^{\mp \frac{m}{n}} \times f^1\left(\frac{1}{x^2}\right)$; and, by substitution, the equation to be solved will become $1 = f(x^2) \times f\left(\frac{1}{x^2}\right)$, of which the general solution is, $f(x^2) = \frac{F(x^2)}{F\left(\frac{1}{x^2}\right)}$, which will evidently answer the conditions.

Thus $f(x) = x^{\pm \frac{m}{n}} \times \frac{F(x^2)}{F\left(\frac{1}{x^2}\right)}$; and, because $d\phi = \frac{2\,dx}{1 + x^2}$, we derive, from equations (2), this formula for finding $\rho$, viz.:—

$$\pm \frac{d\rho}{\rho} = \frac{2\,dx}{1 + x^2} \times x^{\pm \frac{m}{n}} \times \frac{F(x^2)}{F\left(\frac{1}{x^2}\right)};$$

which is a general solution of the problem, since F may represent any function whatever.

If we make $m = 1$, $n = 1$, and $F(x^2) = \text{const.}$, the general equation will become, $\pm \frac{d\rho}{\rho} = \frac{2\,x\,dx}{1 + x^2}$; whence, on account of the double sign, we get,

$$\rho = \frac{p}{1 + x^2} = \frac{p}{\text{Cos.}^2 \frac{\phi}{2}} = \frac{2\,p}{1 + \text{Cor.}\ \phi};$$

$$\rho = p \times (1 + x^2) = p \cos.^2 \frac{\phi}{2} = \frac{p}{2} \cdot (1 + \cos.\ \phi);$$

of which the first belongs to the common parabola, and the second to an epicycloid generated by the revolution of a circle upon the circumference of an equal circle.

Let $F(x^2) = (a + b\,x^2)\,(a' + b'\,x^2)$ &c., then, by taking $\frac{m}{n}$ of a proper value according to the number of factors in $F(x^2)$, we shall get

$$\pm \frac{d\rho}{\rho} = \frac{2\,x\,dx}{1 + x^2} \cdot \frac{a + b\,x^2}{a\,x^2 + b} \cdot \frac{a' + b'\,x^2}{a'\,x^2 + b'} \cdot \text{\&c.}$$

from which formula an infinite number of algebraic values of $\rho$ may be deduced. There are, therefore, an infinite number of algebraic curves that possess the property in question, as Euler has found: but it is not the present intention to enter into a detail of applications, but merely to explain the general solution of the problem.

The most curious circumstance attending this problem is the limitation to which it is subject, first remarked by Euler. If we denote by $\alpha\,x$ such a function of $x$ that $\alpha\,(\alpha\,x) = x$, or $\alpha^2\,x = x$, we may infer from the preceding analysis that the functional equa-

tion F $\{\varphi \alpha x, \varphi x\} = o$, can be solved only in the case when F is a symmetrical function, or one in which the two variables are alike concerned. For, by substituting $\alpha x$ for $x$, we get F $\{\varphi \dot{x}, \varphi \alpha x\} = o$, an equation which will be identical with the proposed one only in the case mentioned. If the two equations are not identical, they cannot both subsist generally for all possible values of $x$. It is not at all impossible that equations of a nature so very general are subject to other limitations in other circumstances.

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## Article VIII.

*Experiments on Topaz, and Carbonate of Bismuth, with some Observations relative to Smithson Tennant, Esq.* By the Rev. Wm. Gregor.

(To Dr. Thomson.)

SIR,

It has, I believe, been made a subject of doubt and discussion by some chemists what sort of a combination it is into which the several constituent ingredients of the topaz enter to form that singularly beautiful and interesting mineral. Now it is evident that, before this question can be satisfactorily settled, *all* the constituent ingredients of the topaz should be specified without the exception of any *one* of them, however inconsiderable it may be, in respect of quantity, when compared with the rest; that is to say, if this one ingredient should be found uniformly present in the crystallized fossil. But it may be asked, has not this been done by Vauquelin, so justly celebrated for his skill and science in chemical inquiries? Have we not the analyses of Klaproth? of whom *that* may be said which can be *justly* said of very few, that his *genius* is equalled by his *industry*. Klaproth, who, far above all his contemporaries, has contributed the most to our knowledge of mineral bodies, and who, drawing upon the ample resources of his scientific information, has given a wonderful simplicity and certainty to chemical analysis. But Klaproth, with all his science and sagacity, has, sometimes, in his later analyses, rectified the result of his former labours. He has, therefore, shown that he is not infallible; and he has shown, moreover, that he is not ashamed to acknowledge his fallibility. I am convinced, therefore, that this eminent man will not be offended by the following suggestions, which lay claim neither to sagacity nor ingenuity; but might have occurred to any man who chanced to make a very easy experiment on the mineral in question.

A few years ago, I thought that I had detected potash in the topaz; and I have since repeated my experiments, and they have tended to confirm me in this opinion. Vauquelin's analysis of the topaz I have not seen. The experiments of Klaproth in the fourth volume of his Beiträge zur Chemischen Kentniss der Mineralkörper

negative the existence of potash in the *Saxon* topaz only. As I had no specimen of the Saxon topaz, the subjects of my experiments were limited to the Brazilian, the Scottish, and the white topaz found in the rock of St. Michael's Mount, in the county of Cornwall. By treating all these with sulphuric acid, I obtained regular crystals of alum. The Cornish topaz produced this salt the most sparingly. Indeed, it resisted the action of the acid more obstinately than the rest; and I had but a small quantity of this fossil on which I could operate. I employed about 70 grains both of the Brazilian and Scottish topaz. In order to prevent the intrusion of foreign matter, and to cut off the sources of erroneous conclusions, the several minerals were reduced to powder, not in a flint mortar, but in a mortar of polished steel. The distilled water which I employed was not distilled from a glass retort, but from a vessel of tinned copper; and the sulphuric acid was pure test acid. The powdered topaz was moistened with the sulphuric acid in a platina crucible, and the acid was gradually evaporated. The soluble part was extracted by distilled water. The several solutions were reduced to a smaller compass by evaporation in a vessel of platina, and were then transferred to glass capsules, which were placed in a warm window. Alum, regularly crystallized, gradually appeared. The Brazilian and Scottish topaz, which had resisted the acid, and remained after the elixation, were respectively treated, in the usual way, with nitrate of barytes and sulphuric acid, and another crop of crystallized alum was produced.

As alum contains but a very small proportion of potash, when compared with its other ingredients; by means of such a combination we are enabled easily to detect the existence of a very minute quantity of potash, which, under the circumstances of the common process, would either elude observation, or be volatilized by the heat employed.

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In a letter which appeared in your Journal many months ago, it was asserted that I had discovered native carbonate of bismuth in the county of Cornwall. I was prevented by peculiar circumstances from noticing this letter at the time of its appearance, which I wished to have done. The mineral alluded to was given to me several years ago as a carbonate of bismuth by Mr. John Mitchell, of St. Austle, to whose kind contributions and communications respecting minerals I have been frequently indebted. *The credit of the discovery, therefore, belongs to him.*

At the time when I sent a small specimen to Mr. Sowerby, I was not aware that native carbonate of bismuth was a nondescript fossil. As this ore is not now easily to be procured, I shall mention a few particulars respecting it. It was raised, I believe, in the parish of St. Agnes. The vein in the different specimens of this ore which I have seen is unequal in thickness, scarcely exceeding half an inch. It is not uniform in respect of colour, as it assumes various dull

tints of greyish green, and brownish and yellowish grey. Neither is it perfectly homogeneous, as small patches of quartz and stony matter are discoverable in it. The ore is rather hard; so that angular fragments are capable of scratching glass. I found the specific gravity of the *purest pieces* which I could select to be 4·31 at temp. 64.

I am led to conclude, from the experiments which I have made upon this ore, that it consists of carbonate of bismuth and a portion of oxide of bismuth more intimately combined with stony matter. 50 grains of the pulverized ore were treated with nitric acid of moderate strength; a solution took place, with effervescence, but it went on but slowly. More acid was added, as long as it appeared to act upon the powder, and, at last, the matrass was placed in warm sand. 26·1 remained undissolved. The solution contained nothing but oxide of bismuth. Water separated 17·3, and, subsequently, ammonia precipitated 3·2 grains. The 26·1 gr. were treated with potash, and 8 gr. of oxide of bismuth were obtained, and alumina, silica, and oxide of iron. In another experiment on 50 grains, 20·7 gr. only remained undissolved. 50 grains were exposed to a low red heat for about ten minutes. A diminution of 3·6 in weight took place, which I conclude arose from the expulsion of water. Muriatic acid dissolved the remaining matter slowly, and with effervescence, which grew fainter till it ceased, although the action of the acid on the mineral had not, apparently, ceased: 7·7 grains remained undissolved. The solution was found to contain, not oxide of bismuth only, but some alumina and oxide of iron also. The undissolved 7·7 gr. contained only 0·3 of oxide of bismuth, with silica and alumina. The oxide of bismuth separated by the effusion of water = 28·8. The oxide of iron amounted to 2·1. The alumina = 7·5, silica = 6·7, and water 3·6.

In order to ascertain the relative weight of carbonate of bismuth when compared with the oxide separated by the effusion of water, I dissolved 50 gr. of bismuth in muriatic acid, to which a small portion of nitric acid was occasionally added. The acid was in excess. The contents of the solution were precipitated by small pieces of carbonate of soda, and at last a saturated solution of carbonate of soda in water was dropped into the muriate of bismuth.

The precipitate edulcorated, &c. and dried in a moderate heat, = 61·3. But, alas! I found that this precipitate consisted of a mixture of carbonate of bismuth and oxide of bismuth!

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I received much pleasure from the perusal of your Biographical Account of Smithson Tennant, Esq.; but it was a pleasure mingled with melancholy reflections, and recollections of past times. To the accuracy of many of the facts therein detailed, and more especially to the fidelity of the sketch of the *discriminating traits of character*, which are touched with a masterly hand, I can bear my

testimony. You mention Mr. Tennant's want of energy in prosecuting his numerous discoveries, and his habits of procrastination in laying them before the public; and you express your fears that many useful hints may have been lost to the community. When I saw him last (upwards of five years ago), he told me, in confidence, that he had discovered that the peculiarities of the steel which is imported from the East Indies, called wootz, is to be attributed to the presence of a very small quantity of arsenic. He informed me that he intended soon to publish his experiments on this subject. Whether he afterwards did this, I never heard; or whether his subsequent experiments tended to substantiate, or to invalidate his opinion, I know not. One thing we know, that Mr. Tennant did not usually form his opinions rashly, or on light grounds. And it may not be altogether useless for it to be known that he once formed this opinion. Mr. T. might have been prevented by various causes from prosecuting his inquiry. Other persons may take advantage of this hint, and be induced to investigate this subject; and the arts may be benefited by such investigation.

Yours, &c.

*Creed, Aug.* 5, 1816. WM. GREGOR.

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## ARTICLE IX.

### I. *On Annuities.*—II. *Imaginary Cube Roots.*—III. *Roots of Binomials.* By Mr. W. G. Horner.

(To Dr. Thomson.)

DEAR SIR,

WITHOUT entering at all into the merits of the method by which Mr. Baily directs the summation of certain annuities to be effected, as noticed by your correspondent Mr. Benwell (*Annals*, Aug. 1816) one is struck with the intricacy of the formulæ proposed to be substituted in its place. After correcting what are probably typographical errors in each of them, no connecting system is apparent by which the method can either be impressed on the memory, or extended to other similar cases. On their form, however, the writer must have founded his claim of novelty; the series and the method of summing them being very well known. But it does not appear to me that any facility in application is attained by the use of the proposed formulæ, which can render them preferable to theorems easy to be remembered, and perfectly general in their nature.

Such a theorem I take the liberty to subjoin. The accuracy of the principle will be recognised by every mathematician. Its application extends to all cases in which the annuities proceed according to *any integral function of the time*; and its universality is equalled by its facility.

Let $r$ represent the ratio of compound interest, or amount of one pound in one year; $t$, the number of years; $n$, the index of the power, or other function of the time, according to which the annuities increase. *Suppose the series expressing the rate of increase to be preceded and followed by an indefinite number of zeroes; and take the differences as far as the $n + 1$ order. Multiply these differences, as taken in order from right to left, by* $1, r, r^2 \ldots\ldots r^{t+n}$. *Divide the aggregate of all these products by* $(r - 1)^{n+1}$; *and the quotient will be the amount of the smallest integral annuity.*

This theorem, expressed by means of a general formula, may be given thus:—

$$f^n(t) + f^n(t-1) \,.\, r + f^n(t-2) \,.\, r^2 \ldots f^n(1) \,.\, r^{n-1} =$$

$$\frac{A r^{t+n} + B r^{t+n-1} + C r^{t+n-2} \ldots - \underset{1}{A} r^n + \underset{1}{B} r^{n-1} - \underset{1}{C} r^{n-2} + \ldots}{(r-1)^{n+1}}$$

The numerator of this expression will contain $2n + 1$ terms; the first $n$ coefficients being all positive, and connected with the powers of $r$, whose indices are $(t + n) \ldots\ldots (t + 1)$, and the remaining $n + 1$ coefficients being alternately positive and negative, and attached to the $n$th and inferior powers of $r$. All the coefficients connected with powers between $r^{t+1}$ and $r^n$ are zeroes.

As a specimen of the process, the calculation of the case in which the term is five years, rate of interest five per cent., and the annuities are as the cubes of the times, is here presented to your readers:—

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Times ..... | 1 | 2 | 3 | 4 | 5 | | | | |
| Annuities ..... | 1, | 8, | 27, | 64, | 125, | | | | |
| 1st diff. ..... | 1, | 7, | 19, | 37, | 61, | −125, | | | |
| 2nd ..... | 1, | 6, | 12, | 18, | 24, | −186, | 125 | | |
| 3rd ..... | 1, | 5, | 6, | 6, | 6, | −210, | 311, | −125 | |
| 4th ..... | 1, | 4, | 1, | 0, | 0, | −216, | 521, | −436, | 125. |

| | | | | | | |
|---|---|---|---|---|---|---|
| Then | $1{\cdot}05^8$ | × | 1 | = | | 1·4774554437890625 |
| | $1{\cdot}05^7$ | × | 4 | = | | 5·628401690625 |
| | $1{\cdot}05^6$ | × | 1 | = | | 1·340095640625 |
| | $1{\cdot}05^3$ | × | − 216 | = | − | 250·047 |
| | $1{\cdot}05^2$ | × | 521 | = | | 574·4025 |
| | 1·05 | × | − 436 | = | − | 457·8 |
| | 1 | × | 125 | = | | 125 |

Positive aggregate ..... 707·8484527750390625
Negative ditto ..... − 707·847

$\cdot 05^4 = (\frac{1}{20})^4 = \frac{1}{160000}$) 0014527750390625

Amount of annuities ..... 232·44400625

To extend the praxis, cases may be proposed, where the annual increase accords with any part of an arithmetical or figurate series, with such series as 2, 5, 9, 14, &c. or in short, any series whose general term is $\alpha t^n + \beta t^{n-1} + \gamma t^{n-2} \ldots\ldots \mu$, $\alpha$, $\beta$, $\gamma$, &c. being any number whatsoever.

II. My object in troubling you with this paper was to communicate some casual, but not careless, nor useless, nor, I hope, offensive observations, on one or two professedly new discoveries announced in your repository. One of these, which has already elicited remarks from several correspondents, was communicated last year by Mr. Lockhart, who states, "that all numbers have four imaginary cube roots." (*Annals*, April, 1815.) He has since avowed the opinion that the number of such cube roots is "infinite." (Aug. 1816.) Now as Dr. Tiarks, in your number for May, 1816, had so completely, though concisely, detected the error into which Mr. L. had fallen in exemplifying his first statement, I concluded that this gentleman, enveloped in *vanishing* fractions, had effected a masterly retreat from an untenable position. (See *Annals*, June, 1815.)

His late avowal convinces me of my mistake; but leaves me quite at a loss to conjecture by what mode of investigation he arrived at a conclusion so repugnant to the clearest principles of algebra. To demonstrate that *every equation has as many roots as it has dimensions, and no more*, it is sufficient if the simple postulate be granted, *that every equation has at least one root.** 

In the equation $x^3 = a^3$ or $x^3 - a^3 = o$, which occasioned the present discussion, one of the roots is known to be $a$, which is contained in the equation $x - a = o$. The other roots are comprised in the equation $x^2 + a x + a^2 = o$; and if one of them be $b$, the other will be $-(a + b)$. No fourth root can exist, nor can any supposition of the kind be made, but by imagining that one or more of the equations $x = a$, $x = b$, $x = -(a + b)$, admits of several solutions, notwithstanding the quantities $a$ and $b$ remain unaltered in their numerical value. Such, then, must be the meaning of the opinion hazarded by your correspondent, that "the number of cube roots of numerical quantities is infinite;" a position whose absurdity is self-evident, when the roots are exhibited, as above, by means of simple equations, and detached from the consideration of imaginary quantities, which Mr. L. represents as so "perplexing."

In his antipathy to these unfortunate symbols, it is to be hoped this gentleman will stand alone; or at least that no young student will suffer himself to be influenced by it. A wide distinction exists between difficulty and perplexity. The former, when surmounted,

* The English reader cannot be directed, in this point, and every other connected with algebra in its most improved form, to a more satisfactory work than Mr. Bonnycastle's, in two volumes, 8vo.

creates half the pleasure of science. The other has its origin in the habit of shunning difficulties, and it paralyses all the powers of the mind.

Those writers who, to evade the consideration of negative quantities, ramify a simple theorem into numerous varieties, afford a specimen of skill in perplexing; and their art will lead to greater intricacies than the labyrinth of Dædalus, if they extend it to imaginary quantities also.

These quantities *must* occur in calculations. The management of them is not attended with any peculiar difficulty. The very *rationale* of them is not so abstruse as some persons represent. They are among the most useful and elegant implements of research, and are the only key to the diophantine and trigonometric analysis.

With these symbols dodging him at every turn, some merit will doubtless be attributable to the mathematician who can run through the evolutions of a country dance without jostling; and he may still find a vacant niche in the Temple of Fame, near Tryphiodorus, the leipogrammatist.

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III. The merit of Mr. Lockhart's method of exhibiting the cube roots of a binomial (*Annals*, June, 1815), does not consist, as Mr. Atkinson supposes, in the originality of the principle; but simply in exhibiting the roots in a tabular arrangement with the proper signs. As the accuracy of these signs has been disputed, it may gratify some readers to see the result of *Waring's* general rule for the formula $x + y = \sqrt[n]{A + B}$, when restricted to the three cube roots in question.

His words are, "Ita reducatur data quantitas, ut evadat $A^2 - B^2 = \pi^n$; inveniatur *radices* æquationis $z^n - n\pi z^{n-2} + n \cdot \frac{n-3}{2} \pi^2 z^{n-4} - \&c. = 2A$; erit $z = 2x$ et $y = \sqrt{\frac{z^2 - 4\pi}{4}}$ unde radix quæsita $x + y = \frac{z + \sqrt{z^2 - 4\pi}}{2}$." (Med. Alg. Prob. 50.)

Conformably to this precept, in the equation $x^3 - bx = c$, let the roots be R, $-r$, $-\varrho$; and, since $A = \frac{c}{2}$, $\pi = \frac{b}{3}$, and therefore $B = \pm\sqrt{\frac{c^2}{4} - \frac{b^3}{27}}$. We have as a general expression for the cube root of $\frac{c}{2} \pm \sqrt{\frac{c^2}{4} - \frac{b^3}{27}}$, the formula $\frac{x}{2} \pm \sqrt{\frac{x^2}{4} - \frac{b}{3}}$.

The actual cube of this expression is $\frac{x^3 - bx}{2} \pm \left(x^3 - \frac{b}{3}\right) \sqrt{\frac{x^2}{4} - \frac{b}{3}}$, and is correctly equal to $\frac{c}{2} \pm \sqrt{\frac{c^2}{4} - \frac{b^3}{27}}$. Now

as the same interpretation must be given to the dubious sign in each of these equivalent formulæ, it is clear that the sign belonging to $\sqrt{\frac{x^2}{4} - \frac{b}{3}}$ will be either like or unlike that of $\sqrt{\frac{c^2}{4} - \frac{b^3}{27}}$, according as $\left(x^2 - \frac{b}{3}\right)$ is positive or negative. Also $x^2 - \frac{b}{3} = \frac{c}{x} + \frac{2b}{3}$, which is $= r\varrho + \frac{2b}{3}$, or $- \mathrm{R}\varrho + \frac{2b}{3}$, or $- \mathrm{R}r + \frac{2b}{3}$, according as $x$ is interpreted by R, or $- r$, or $- \varrho$.

The affections of the signs, in the irreducible case, are easily determined thus:—When $r = \varrho$, each is $= \frac{1}{2}\mathrm{R}$; at the same time $4b$ is $= 3\mathrm{R}^2$, and $- \mathrm{R}\varrho + \frac{2b}{3} = - \mathrm{R}r + \frac{2b}{3} = o$. Again, when $r = \mathrm{R}$, $\varrho = o$, $b = \mathrm{R}^2$ and $- \mathrm{R}\varrho + \frac{2b}{3} = \frac{2b}{3}$, while $- \mathrm{R}r + \frac{2b}{3} = - \frac{b}{3}$. So that between the limits $r = \varrho$ and $r = \mathrm{R}$, $x^2 - \frac{b}{3}$ is limited by O and $\frac{2b}{3}$, or O and $- \frac{b}{3}$, according as $x$ is interrupted by $- r$ or $- \varrho$; and is consequently positive on the former assumption, and negative on the latter. The formula $x^2 - \frac{b}{3} = r\varrho + \frac{2b}{3}$ is undoubtedly positive.

In the irreducible case, then, the values of

$$\sqrt[3]{\frac{c}{2} \pm \sqrt{\frac{c^2}{4} - \frac{b^3}{27}}}$$

$$\text{are } \frac{\mathrm{R}}{2} \pm \sqrt{\frac{\mathrm{R}^2}{4} - \frac{b}{3}} = \mathrm{A}, \mathrm{A}'$$

$$- \frac{r}{2} \pm \sqrt{\frac{r^2}{4} - \frac{b}{3}} = \mathrm{B}, \mathrm{B}'$$

$$- \frac{\varrho}{2} + \sqrt{\frac{\varrho^2}{4} - \frac{b}{3}} = \mathrm{C}, \mathrm{C}' \ldots\ldots\ldots (1)$$

as given by your correspondents.

But in the reducible case, the real and imaginary parts of the factor

$$r^2 - \frac{b}{3} = \tfrac{1}{6}(4b - 3\mathrm{R}^2) + \tfrac{1}{2}\mathrm{R}\sqrt{4b - 3\mathrm{R}^2}$$

will be affected by contrary signs; neither of which can claim the preference, because no estimate can be made of the aggregate value. This is the true reason why the formulæ B, B, given by Mr. L., are of no utility when applied to this case.

As $\sqrt{\frac{x^2}{4} - \frac{b}{3}}$ by actual extraction becomes a binomial, the

general form of A, B, C, in their simplest state, is that of a trinomial. And to exhibit this without incurring disputable signs, it must be obtained by simple equations.

Now it is known that

$$r = \tfrac{1}{2}(A + A') - \tfrac{1}{2}(A - A')\sqrt{-3}$$

and $$\varrho = \tfrac{1}{2}(A + A') + \tfrac{1}{2}(A - A')\sqrt{-3}$$

Whence, by eliminating A′ and A alternately, we find

$$\tfrac{1}{2}(r + \varrho) \pm \tfrac{1}{6}(r - \varrho)\sqrt{-3} = A, A'$$

Multiplying these values by the imaginary cube roots of 1, we further obtain,

$$-\tfrac{1}{2}r \pm \tfrac{1}{6}(r + 2\varrho)\sqrt{-3} = B, B'$$

and $$-\tfrac{1}{2}\varrho \mp \tfrac{1}{6}(2r + \varrho)\sqrt{-3} = C, C'$$

Or, substituting R for its equal $r + \varrho$ in the three equations just found,

$$\tfrac{1}{2}R \pm \tfrac{1}{6}(r - \varrho)\sqrt{-3} = A, A'$$

$$-\tfrac{1}{2}r \pm \tfrac{1}{6}(R + \varrho)\sqrt{-3} = B, B'$$

$$-\tfrac{1}{2}r \mp \tfrac{1}{6}(R + \varrho)\sqrt{-3} = C, C' \ldots\ldots (2)$$

where no ambiguity exists.

*Bath, Sept.* 9, 1816. W. G. Horner.

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## Article X.

*A rigorous Investigation of the Length of the Seconds' Pendulum in the Latitude of Plymouth, being* 50° 22′ 28″. By W. Watts.

I had long been of opinion that the length of the simple pendulum vibrating seconds of time in the latitude of London was never determined with that degree of exactness which might be desired, and which is so very necessary in the investigation of many physical problems; such, for example, as the determination of the increase of gravity from the equator to the poles, which augmentation appears difficult, if not impossible, to assign, in an island, by means of the measurement of the degrees of the terrestrial meridian, on account of the defect of attraction on the part of the ocean; but it was the length of the seconds' pendulum adopted by the Honourable Committee of the House of Commons, namely, 39·13047 inches, which excited my particular attention to this important subject, as this length was stated as the result of the experiments of Sir George Shuckburgh, with a minute correction of an arithmetical error under the authority of Professor Playfair and Dr. Wollaston; and as I found that this length differed very sensibly from the length deducible from M. Laplace's formula, given in his Mechanique Celeste, tom. ii. p. 151; that is to say, from $0^{me}$ ·739502

$+ 0^{me} \cdot 004208 \sin.^2 \psi$, where $\psi$ denotes the sine of the latitude, which formula gives 39·13881 inches for the length of the pendulum vibrating seconds in vacuo, in the latitude of St. Paul's, London; and it is well known that observation should give it still greater; so that there is every reason for supposing that it is not less than 39·175 inches in vacuo; for the only remaining difficulty against the homogenity of the earth is the difference which is found between the observed length of the pendulum in different latitudes, and that which theory gives in the same latitudes; for if the earth were of the form of a homogeneous spheroid, the augmentation of gravity from the equator to the poles should be proportional to the square of the sines of the latitudes. This difference induced me to verify M. Laplace's formula in the latitude of Plymouth, as it is very desirable, even with respect to the fixation of our linear measures, to assign the true length of the seconds' pendulum, since it may be regarded, in fact, as an invariable standard or unity of measure, to which all other linear measures should be referred as to a standard of comparison; and, therefore, though all our standards of weights and measures should happen to be altered, or even lost, by the lapse of time, the length of the seconds' pendulum might, at any time, be easily determined; and consequently, by properly dividing the length of this pendulum, we should arrive at the exact measure of an inch, a foot, &c.

It is also necessary to assign the variations in the length of the pendulum vibrating seconds at the principal stations of the trigonometrical survey extended through Great Britain, in order to determine, by this means, the true figure of the earth, because the trigonometrical survey gives results which are at variance with the received theory, and which would seem to establish the opposite hypothesis, namely, that the earth is of the form of a prolate, and not an oblate spheroid. And notwithstanding in the expressions of the magnitude of a degree of the meridian, and of the length of the pendulum, the variation of two consecutive degrees is more sensible in the measure of the degrees, than in the lengths of the pendulum, as M. Laplace has shown in his Mechanique Celeste, tom. ii. art. 33; yet the defect of attraction on the part of the ocean is such as to render these variations, in the degrees, much less certain than those in the length of the pendulum. Moreover, the remarkable relation between the expression of a degree of the meridian, and the length of the pendulum will always serve to verify any hypothesis that may be considered proper to represent the measures of the degrees of the meridian; and it is astonishing that this consideration did not induce Government to give the necessary directions for determining the length of the simple pendulum at the different stations of the trigonometrical survey, especially as the subject is of so much importance in many points of view, particularly with respect to the moon's parallax in latitude, longitude, and azimuth.

These general observations being premised, I now proceed to state the method that I employed for determining the length of the simple pendulum vibrating sexagonal seconds in the latitude of 50° 22′ 28″, on Feb. 8, 1816, barometer 29·63, and thermometer 44° of Fahrenheit.

For this purpose, I carefully compared the going of my watch, which is one of Litherland's patent watches with a quarter second hand, during several successive days, with an accurate chronometer regulated according to mean time, and found that my watch gained on mean time 35 seconds in 24 hours.

In the second place, I avoided having the thread of the proof pendulum either of hemp, flax, or silk, because all these substances are liable to extend themselves; and I equally avoided having it made of any of the metals, because it would not possess sufficient flexibility. For these reasons I employed a thread of pite or coir as fine as a hair, in imitation of M. de Mairan and the other academicians who went to Peru to determine the figure of the earth, as it has been found more suitable for this purpose than any other known substance whatever, both on account of its flexibility and inextensibility. And since the form of the weight to be attached to the thread is not indifferent, I adopted the spherical figure as best suited to the purpose in hand.

In this manner I formed a pendulum, consisting of a cast brass ball, whose specific gravity was 8, very accurately turned, and being exactly one inch in diameter. With respect to the length of the pendulum, it is well known that it is the distance of the centre of oscillation of the thread and weight conjointly, from the point or line of suspension; but as the weight of the thread is insensible, with respect to that of the ball, the length of the pendulum will be equal to the distance of the centre of oscillation of the ball from the point of suspension. Therefore the length of the thread of pite being 72 inches, and the radius of the ball 0·5 inch, the length of the pendulum will be $72{\cdot}5 + \frac{2 \times 0{\cdot}5^2}{5 \times 72{\cdot}5} = 72{\cdot}500138$ inches, at the temperature of 62° of Fahrenheit's thermometer. But when I took the measure of the said pendulum from a standard brass yard, the temperature was only 44°, being 18° below the standard temperature of 62°, which is universally employed in the comparison of English standards; and therefore we ought to deduct from the above length of the pendulum the contraction due to 18° of Fahrenheit's thermometer, which is equivalent to 8° of Reaumur's. Now it appears from the experiments of M. Berthond, made on a stove in which Reaumur's thermometer rose from zero to 27°, on brass rods of three feet two inches, five lines in length, three lines in thickness, and five lines in breadth; that they dilated $\frac{121}{360}$ of a line, which, being reduced to English measure, shows that a brass rod of the above dimensions expands by a 27·000000 part of its length for every degree of Reaumur; consequently we shall have for

the true length of the proof pendulum at the temperature of 44° of Fahrenheit's thermometer,

$$72{\cdot}500138 - 8 \times 0{\cdot}000027 \times 72{\cdot}500138 = 72{\cdot}484478 \text{ inches.}$$

Having thus determined the length of the proof pendulum, I next suspended it, by putting the thread of pite between the faces of two pieces of wood, which were very accurately jointed, and then inserting them in a mortice, so contrived in the architrave of a window as to press them very close together; so that from the accurate joint of the two pieces of wood, the point of suspension was situated exactly in the inferior surfaces of these two pieces of wood, and in the same horizontal plane. The proof pendulum being thus suspended, and having ascertained that my watch gained 35″ per day on mean time, that the barometer stood at 29·63 inches, and that the temperature was 44° by Fahrenheit's thermometer, I deflected the proof pendulum about an inch from its vertical position, in order that it might perform but a very small arc, and having made it oscillate in a vertical plane, lest the oscillation should be conical, perpendicularly to the joint of the two pieces of wood above-mentioned, I began to count its vibrations, and also those of the watch, from the instant that they commenced their motions together, counting from a given point; that is to say, I counted the number of seconds on the watch from this instant, during $480\frac{2}{3}''$, and also the number of oscillations performed by the proof pendulum in the same intervals, and found that in $480\frac{2}{3}''$, as indicated by the watch, the proof pendulum had vibrated 353 times; but as the watch gained at the rate of 35″ in 24 hours on mean time, it is evident that we shall have this proportion :—

$$\begin{array}{r} 86400'' \\ 35 \text{ add} \\ \hline 86435'' : 86400'' :: 480{\cdot}66'' : 480{\cdot}3''; \end{array}$$

so that the proof pendulum vibrated 353 times in 480·3″ of mean time, consequently it performed 44·094 vibrations per minute. Moreover, since the lengths of pendulums are in the inverse ratio of the square of the number of vibrations made in the same time, we shall have also this proportion :—

$$\text{As } 60^2 : 44{\cdot}094^2 :: 72{\cdot}484478 \text{ inches} : 39{\cdot}14831 \text{ inches,}$$

the length of the proof pendulum in air.

There is a correction due to the atmosphere which diminishes the real weights of bodies suspended in it; so that the force which actuates the pendulum in air being less than that which solicits it in vacuo, the length of the pendulum found above is a little too short. This correction may be determined as follows :—As the barometer always indicates the actual weight of the atmosphere, we can, by this means, always determine the diminution which it occasions; for since atmospheric air is 850 times lighter than water, when the barometer stands at 30 inches, and water eight times lighter than

cast brass, we shall find the ratio of the specific gravity of air to that of cast brass as 1 : 6800; and therefore the small brass ball attached to the thread of the proof pendulum loses the 6800 part of its weight in atmospheric air. Hence it follows that the length of the pendulum which vibrates seconds in the latitude of Plymouth, 50° 22′ 28″ is too short by $\frac{39{\cdot}14831}{6800} = 0{\cdot}005877$ parts of an inch; and therefore it appears that the length of the simple pendulum of seconds, in vacuo, in the latitude 50° 22′ 28″, is 39·154187 standard English inches, being about 0·021387 parts of an inch more than Laplace's formula gives, which shows that the earth is much less flattened in this parallel of latitude than in the hypothesis of homogeneity, that its ellipticity is somewhat greater than $\frac{1}{335{\cdot}78}$, and that the ratio of its axes cannot be supposed greater than that of 320 to 321.

There is no correction necessary with respect to the resistance which the atmosphere opposes to the motion of the pendulum, because it does not produce, as we might be led to suppose, any sensible change in the duration of the oscillations; for, as M. Bouguer observes, if by means of the resistance of the air, the duration of the descending semivibration be a little increased, the duration of the ascending semioscillation will, in like manner, be a little diminished; so that there will be an exact compensation with regard to an entire oscillation; and consequently its duration differs only by an infinitely small quantity from what it would have been, in case there were no resistance whatever.

It should also be remembered that the centrifugal force diminishes that of gravity, and consequently it renders the length of the pendulum less than it would be if the earth were immoveable, and it will be found necessary to add 0·1358852 inch to the length of the seconds' pendulum, observed under the equator, in order to have that which would be observed if the earth were at rest; and if we multiply 0·1358852 inch by the square of the cosine of the latitude, the result will be the correction for any other latitude. This correction for the latitude of Plymouth is 0·055251 inch; so that the length of the seconds' pendulum, if the earth were immoveable, would be 39·209438 inches.

In like manner may the length of the simple pendulum of seconds be determined in any other latitude; and this, in my opinion, is the only mean presented by Nature for determining the unity of linear measures, being "independent of moral revolutions," and not liable to "any sensible alterations, but by very great changes in the physical constitution of the earth." Besides, this system is much more simple and correct than the meridian system, on account of the excess of attraction on the part of the mountains, and the defect of it on the part of the ocean.

## Article XI.

### *On the Course of the Niger.* By Mr. S. Rootsey.

(To Dr. Thomson.)

SIR, *Bristol, Aug.* 4, 1816.

Should you consider the following as meriting a place in your *Annals of Philosophy*, it is at your service.

I am, Sir, your most obedient servant,

S. Rootsey.

It is generally known that we have lately sent out two expeditions for the purpose of exploring the interior of Africa, and more especially the course of the river Niger, to which the attention of the philosophical world has been particularly directed.

This river is understood to stretch across Africa, but with regard to the direction in which it flows geographers are much at variance: however, a river which rises in the country of Manding, and which flows for 300 miles, as far as Silla, from W. to E., is considered as its western extremity. Haoussa seems to be about 400 miles farther down the stream, and it is presumed that its easterly course is continued so far; directly contrary to what we were given to understand was its direction at Tombuctoo, which is farther on than Silla. On the other side of the continent it is said to flow by Ghana, Cassena, and Toerur, from E. to. W., but some contend that it flows from Haoussa to Ghana, from W. to E. A learned writer supposes that there are two rivers, to both of which the name of Niger has been given, and which, running in opposite directions, unite in an inland sea. Some have adduced strong arguments to prove that the river Congo or Zaire is its western extremity, and others consider that it discharges its waters into the Atlantic at Benin. Two recent publications, the Narrative of Robert Adams and the Travels of Ali Bey, have been thought to increase the confusion. Hence any observations tending to reconcile these apparent contradictions will not require an apology.

By laying down the positions of some of the towns by which this river is said to pass, and by noting the direction in which it is stated to flow at each place, I hope to determine this long agitated question. The cause of this confusion I attribute to the misplacing of Toerur and of Haoussa, and to the statement of Edrisi, that the river terminates between Toerur and Ulib.

Edrisi says that Toerur is 40 days journey from Segelmessa, and 24 from Ghana. I know of no reason to suppose that Toerur is not in the direct line from Ghana to Segelmessa, and I therefore place it accordingly. Now Ghana seems to lie about 12 degrees S. by E. of Tripoli, for it is 45 days' journey from Kucu, and not quite so

much from Caugla or Gaoza, which is to the W. of Nubia, and 20 days' journey, or 400 miles S. of Kucu; and Segelmessa is situated to the E. of Morocco. This position will bring Tocrur a little to the N. of the great lake of Warguela, in which to the S. of that town must lie the island of Ulib. With the country beyond this Edrisi was unacquainted, and therefore thought that the river terminated in this inland sea, which he must have mistaken for an arm of the Atlantic. This is agreeable to the opinion of Mr. Hugh Murray, who thought that this lake was the Soudan sea. Between Tocrur and Ghana, 12 days' journey from each, and to the S. of Andeghest or Agades, 12 days' journey, was the town of Berissa, 10 days' march to the N. of the land of Lamlem, beyond which to the S., says Edrisi, it is not known that there is any inhabited place. He says that the river flows from E. to W., and this seems to be confirmed by the relation of Shereef Imhammed to Mr. Lucas, who declared that he passed it at the town of Cassena, and that the stream ran in that direction. Cassena is W. of Cano, which all agree is the same as Ghana. Tirca is six days' journey E. from Ghana, and the road lies on the bank of the river. Hence, I think, there can be no doubt that the river described by Edrisi runs from E. to W. into the abovementioned lake.

It appears to pass through this lake, and to flow in a S.W. direction past Tombuctoo, after receiving those rivers which are laid down as running eastward from Morocco. All who have been at Tombuctoo assert that at that place the river runs from E. to W.; but Robert Adams, who went up its right bank for more than 150 miles, says it comes from the N. E., and is $\frac{3}{4}$ of a mile broad; so that at Tombuctoo it would seem to wind a little more to the W. Leo Africanus says that Tombuctoo, which he had visited, was situated within 12 miles of a branch of the Niger. This branch I consider as the river Joliba, discovered by Mr. Park, and which rises in the country of Manding. Below the confluence of these rivers the stream certainly passes by Haoussa. I shall not infer the direction in which the river flows from the position of Haoussa, but the position of Haoussa from the course of the river. Now Leo asserts that merchants sail with the stream from Tombuctoo to Ghinea, which kingdom he says is that which is so called by the Europeans; he does not speak of it as a town called Genne. A person who had been at Haoussa likewise informed Mr. Beaufoy that merchants sailed from thence with the stream towards Ghana. Hence as the country we call Ghinea or Guinea is to the S. of Tombuctoo, I think it is evident that the river runs in a southerly course past Haoussa. Ben Ali heard it stated at Tombuctoo that the Niger terminated to the S. of that capital.

A merchant, who had resided at Tombuctoo, informed Ali Bey that that town was 300 miles distant from a river called Nile-abid, which ran from W. to E. into a vast sea that is 48 days' sail from one shore to the other. Leo tells us that the town of Gago is distant S. of Tombuctoo almost 400 miles, inclining somewhat to the

S. E.: and in another place that Guber is situate almost 300 miles E. of Gago; between them, he says, is a vast desert, which is in great want of water, being about 40 miles distant from the Niger. I therefore conclude that the Niger runs from W. to E., about 40 miles S. of the line that connects Gago with Guber. Leo says that Gago is a place of great trade, and that the negroes buy cloth brought hither from Barbary and Europe; I suppose via Tombuctoo and the lake of Werguela. Mr. Jackson, in his account of Morocco, says that 15 days' journey E. of Tombuctoo there is an immense lake called Bahar Soudam. This must be the vast sea into which the river falls 400 miles farther S. I think also, that if the great river entered this sea to the E. of Tombuctoo, Mr. J. would not have used the terms 15 days' journey; but if we suppose that the desert between Gago and Guber (which Leo calls vast) extends to the N. E. past Tombuctoo, this expression would be proper, as the breadths of deserts are always expressed in that manner.

Thus I think I have proved that the river runs from E. to W. past Ghana, Cassna, and Tocrur, and then S., through the lake of Werguela, by Tombuctoo, and Haoussa, to Gago: and that from thence it passes by Guber, and falls into the Soudam Sea, which is said to have no connexion with the ocean, and respecting which many particulars may be found in Mr. Jackson's Morocco, and, amongst others, that its Eastern shores are inhabited by Christians, who are whites. Thus also does this river seem to agree with the Gihon of Moses, encompassing the whole land of Ethiopia. Being desirous of not excluding from your Journal more interesting matter, I have been as concise as the limits to which I confined myself would allow.

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## Article XII.

### Analyses of Books.

*De Coloribus Corporum Naturalium, præcipue Animalium Vegetabiliumque, determinandis Commentatio phisiographica, quæ ad Prælectiones suas in Universitate Litteraria Berolinensi habendas invitat* Dr. Fridiricus Gottlob Hayne, &c. Berlin, 1814.

An accurate arrangement and nomenclature of colours has of late years attracted much of the attention of naturalists. Its utility in mineralogy has been long understood; and it is probable that it might be applied to Zoology and Botany, with nearly the same advantage. Dr. Hayne published a first attempt of the kind some years ago, in his book entitled *Termini Botanici Iconibus illustrata*; and the present publication he considers as an improvement, or rather a completion of his former labours. His plan is to define the

different colours, by referring each to some natural substance of easy access, which is distinguished by the colours in question. To render his method intelligible, nothing more is necessary than to give his table of the different colours, and their varieties.

SPECIES OF COLOURS.

1. Albus, white.
2. Canus, grey.
3. Niger, black.
4. Brunneus, brown.
5. Ruber, red.
6. Luteus, yellow.
7. Viridis, green.
8. Cœruleus, blue.

VARIETIES.

## I. *Varieties of White.*

1. Niveus, snow-white.
2. Argillaceus, greyish-white.
3. Betulinus, (birch-white), brownish-white.
4. Carneus, reddish-white.
5. Eborinus, (ivory-white), yellowish-white.
6. Amiantinus, greenish-white.
7. Lacteus, bluish-white.

## II. *Varieties of Grey.*

1. Incanus, whitish-grey.
2. Griseus, blackish-grey.
3. Murinus, (mouse-colour), black-grey.
4. Fumigatus (smoke-grey), brownish-grey.
5. Capreolatus, brown-grey.
6. Margaritaceus (pearl-grey), reddish-grey.
7. Cycaceus, red-grey.
8. Cinereus, yellowish-grey.
9. Roborinus (oak-grey), yellow-grey.
10. Strychninus, greenish-grey.
11. Fœninus (hay-coloured), green-grey.
12. Leucophœus, bluish-grey.
13. Schistaceus, blue-grey.

## III. *Varieties of Black.*

1. Ater, pure-black.
2. Basaltinus, greyish-black.
3. Piceus, brownish-black.
4. Morinus, reddish-black.
5. Ureaceus, yellowish-black.
6. Coracinus, greenish-black.
7. Anthracinus, bluish-black.

## IV. *Varieties of Brown.*

1. Glandaceus, whitish-brown.
2. Helvolus (hair-brown), greyish-brown.
3. Cascarillinus, grey-brown.
4. Fuscus (coffee-brown), blackish-brown.
5. Fuliginosus, black-brown.
6. Badius, reddish-brown.
7. Castaneus, red-brown.
8. Cinnamomeus, yellowish-brown.
9. Ferrugineus, yellow-brown.
10. Hepaticus, greenish-brown.
11. Guaiacinus, green-brown.
12. Juniperinus, bluish-brown.
13. Pullus (wood-brown) blue-brown.

## V. *Varieties of Red.*

1. Carmineus, pure-red.
2. Persicinus (peach-blossom), whitish red.
3. Rozeus, white-red.
4. Githaginosus, greyish-red.
5. Linotius, grey-red.
6. Sanguineus, blackish-red.
7. Cerasinus, black-red.
8. Lateritius, brownish-red.
9. Hematiticus, brown-red.
10. Coccineus (scarlet), yellowish-red.
11. Miniatus (minium) yellow-red.
12. Purpureus, bluish-red.
13. Lilacinus, blue-red.

## VI. *Varieties of Yellow.*

1. Ranunculaceus, pure-yellow.
2. Citrinus, whitish-yellow.
3. Flavus, white-yellow.
4. Buxeus, greyish-yellow.
5. Cerinus, grey-yellow.
6. Luridus, blackish-yellow.
7. Mulatinus (Mulatto), black-yellow.
8. Vitellinus, brownish-yellow.
9. Ochraceus, brown-yellow.
10. Aurantiacus, reddish-yellow.
11. Croceus, red-yellow.
12. Sulphureus, greenish-yellow.
13. Laureolaceus, green-yellow.

## VII. *Varieties of Green.*

1. Smaragdinus, pure-green.
2. Pomaceus, whitish-green.
3. Pisaceus, white-green.
4. Thallasinus, greyish-green.
5. Glaucus, grey-green.
6. Populeus, blackish-green.
7. Chloriticus, black-green.
8. Capparinus, brownish-green.
9. Olivaceus, brown-green.
10. Psittacinus, yellowish-green.
11. Ligurinus, yellow-green.
12. Malachiticus, bluish-green.
13. Æruginosus, blue-green.

## VIII. *Varieties of Blue.*

1. Cyaneus, pure-blue.
2. Azureus, whitish-blue.
3. Endiviaceus, white-blue.
4. Cœsius, greyish-blue.
5. Nubilus, grey-blue.
6. Indigoticus, blackish-blue.
7. Myrtillinus, black-blue.
8. Pruninus, brownish-blue.
9. Lividus, brown-blue.
10. Parcellinus (litmus-blue), reddish-blue.
11. Violaceus, red-blue.
12. Tarcinus, greenish-blue.
13. Coraciaceus, green-blue.

The Latin names by which these colours are distinguished, are derived each from some natural object, which possesses the colour denoted by it. Thus *roborinus* is the colour of the small twigs of the oak tree, *betalinus* is the colour of the cuticle of the beech tree, and so on. In general these names may be traced to the natural substances, which are intended to constitute the types of the colours; but this is not always the case. The derivation of some of the words is rather obscure; and some of them, as *cascarillinus* (from cascarilla bark) allude to substances not likely to be generally familiar to naturalists.

The book is terminated by two tables; the first of which gives a view of the different simple colours of which the compound colours are composed; and the second shows in what way the different simple colours pass into each other.

# Article XIII.

## *Proceedings of Philosophical Societies.*

### ROYAL INSTITUTE OF FRANCE.

*Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year* 1815.

Mathematical Part.—*By M. le Chevalier Delambre, Perpetual Secretary.*

ANALYSIS.

(*Continued from* p. 228.)

*Discovery of two Sorts of double Refraction, attractive and repulsive.* By M. Biot.

When a ray of light penetrates into a crystal, whose primitive form is not the regular octahedron, nor the cube, we observe in general that it divides into two pencils unequally refracted; one, called the ordinary pencil, follows the law of refraction discovered by Descartes, and which is common to all bodies, whether crystallized or uncrystallized. The other follows a different and more complicated law. It is called the extraordinary pencil.

Huyghens determined this last law by observation in the rhomboidal carbonate of lime, usually called Iceland spar, and expressed it by a construction equally ingenious and exact. Laplace, by combining this fact with the general principles of mechanics, has deduced the general expression of the velocity of the particles of light which constitute the extraordinary pencil. This expression indicates that they are separated by a force proceeding from the axis of the crystal, and which in Iceland crystal is repulsive.

It was generally supposed to be the same in all crystals possessed of double refraction; but M. Biot has discovered by new experiments that in a great number the extraordinary ray is attracted to the axis instead of being repelled. So that under this point of view crystals may be divided into two classes; the first of which Biot calls *attractive double refraction*; the second, *repulsive double refraction.*

Iceland spar belongs to the first class; rock crystal to the last. M. Biot supposes that the force, whether attractive or repulsive, proceeds from the axis of the crystal, and follows always the same laws, so that the formulas of Laplace may be always applied to it.

Preceding observations had already led M. Biot to perceive a singular opposition in the nature of the impressions which different crystals exercise on light in polarising it. (See our history for 1814.) He had expressed this opposition by the terms *quartzous polarization* and *beryllous polarization*, from the names of the two

bodies which first exhibited it. He now finds that all crystals possessing the quartzous polarization are attractive, while those possessing the beryllious are repulsive. Iceland spar is in the latter predicament.

These results show that there exists in the action of crystals on light the same opposition of force, which has been already recognised in several other natural actions, as the two magnetisms, the two electricities. The other observations already published by M. Biot on the oscillations and the relations of luminous particles lead equally to the same conclusion.

*Determination of the Law according to which Light is polarized at the Surface of Metals.* By M. Biot.

When Malus discovered the polarization which light experiences when reflected from the surface of diaphanous bodies, he recognized at the same time that this phenomenon is not produced at least in the same manner at the surface of metals. M. Biot, in his work on light, showed afterwards that two sorts of reflections are produced in general at the surface of coloured bodies: the one, which appears to be produced without the body, acts indiscriminately on all the luminous molecules, and produces a white ray, if the incident light be white: the other, more interior, acts only on the luminous bodies which compose the peculiar colour of the body. The first, under a certain incidence, polarizes in great part the light in the direction of the plane of reflection, after the manner of diaphanous bodies. The second does not produce this effect, or at least produces it with a much smaller intensity. Thus if we place a mirror so that it transmits or absorbs the first kind of light, it will reflect the other, and we shall see the body of its own colour without any mixture of white light. From employing this method, M. Biot thought at that time that the portion of light of which these colours are composed proceeded from the body polarized in a manner quite confused. M. Arago showed that a very considerable portion proceeded from all sides polarized parallel to the surface of the body, and perpendicular to the plane of emergence. Dr. Brewster, by reflecting several times from plates of silver or gold a ray of light already polarized, observed that this light was modified so that when analysed with a prism of Iceland spar it divided into two differently coloured pencils. M. Biot hastened to verify this remarkable observation; and, to distinguish the nature of the tints the better, he made a white ray of light from the clouds previously polarized by a black glass to fall upon the plates. Then, by varying the incidences of the rays on the plates, he easily perceived that the tints into which the reflected pencil divided itself were precisely those of the coloured rings, reflected and transmitted, which were observed by Newton; and that in this respect, as well as in the direction of the polarization, the phenomena followed exactly the law of the moveable polarization, which holds in thin crystallized plates. He communicated this analogy to the Class on March 27 last, when he gave an account of the new discovery of Dr. Brewster.

At this time the communication with Great Britain was interrupted, and MM. Biot and Brewster, who had mutually communicated the results of their researches, continued their solitary labours without being able to correspond. M. Biot convinced himself, by numerous observations, that silver and other metals modify the light which they reflect exactly as doubly refracting crystals modify that which they refract; the number of successive reflections corresponding to the greater or smaller thickness of the crystal.

The phenomena observed by M. Biot were different, at least in appearance, from those announced by Dr. Brewster. The acute Edinburgh philosopher had described at first the tints of the reflected images as succeeding each other by simple changes from the greatest to the smallest refrangibility; whereas M. Biot discovered in them the whole series of reflected and transmitted rings. Dr. Brewster pointed out these tints as polarized, the one in the plane of reflection, the other in the perpendicular plane; M. Biot found them polarized at equal distances from this plane, the one in the direction of the primitive polarization, and the other on the opposite side, agreeably to the theory of oscillations. From this it followed that a single reflection from silver ought not to give natural light any determinate polarization. Dr. Brewster, by continuing his observations, had completed his first notions. He had come to conclusions partly similar to those contained in the last letter which he had received from M. Biot, and conformable to the laws of moveable polarization, at least for the similar reflections, as that letter had pointed out. But M. Biot, not having any knowledge of what was done in Edinburgh, and uneasy at the apparent contradiction which he perceived between his own experiments and those of Dr. Brewster, mentioned the subject to M. Arago, who assured him that he had observed that light reflected by silver, as well as by other metallic bodies, always experiences a very sensible partial polarization, according to the plane of incidence; and he gave him a piece of polished silver which really possessed that property. The new experiments made with this piece were conformable to those indicated by Dr. Brewster, and contrary to those formerly perceived by M. Biot. He endeavoured, in consequence, to discover the difference which existed in the elements of the two observations. He conceived that the different manner of polishing might have some effect on the polarizing power of metallic plates. This supposition was confirmed by experience.

A metal may be polished either by hammering or friction. The first method when applied to silver gives it great whiteness; but the images are always a little hazy, and ill defined round the edges. It reflects light in abundance; but we do not recognize in it the lively polish and brilliancy of a mirror. By the other method we obtain more accurate and brilliant images, and the reflection has quite a specular appearance.

It is very remarkable that these two kinds of polish produce a different effect upon incident light. We do not here allude to the

greater or smaller quantity which the surfaces reflect, but to the manner in which they act on the luminous molecules, and the direction in which they polarize them.

When the surface has received the specular polish, it produces by regular reflection two distinct effects. It first gives to a part of the incident light the moveable polarization round the plane of incidence; that is to say, it makes the luminous molecules oscillate on one side or other of this plane, in the same way as a thin crystallized plate, or a plate having a weak polarizing power, makes them oscillate on both sides of the principal section; and in both cases the tints pass through the whole series of Newton's reflected and transmitted rings. But, besides this, the metallic surface gives to a white portion of the light the fixed polarization in the plane of incidence; in the same way as a thick crystalline plate, or one having a strong polarizing power, gives to the light which traverses the polarization fixed in two rectangular directions. And as Biot has shown that in all crystallized bodies the luminous molecules pass progressively from the moveable to the fixed polarization, when they have penetrated to a certain depth, in like manner in each reflection between two metallic plates, we observe that a part of the light which had undergone the moveable polarization in the preceding reflections assumes the fixed polarization, which it can never after lose if the reflecting plates are parallel; so that in this case, after a number of reflections more or less considerable, according to the nature of the metal and the polish which it has received, we ought to find, and we do in fact find, almost all the light polarized fixedly in the plane of reflection. In the reflection from steel, and probably from the other metals that take a very lively specular polish, the portion of white light thus taken from the moveable polarization, is incomparably the greatest. So that the appearance of colours, which the moveable polarization can alone produce, becomes insensible, or can only be perceived in certain peculiar positions pointed out by the theory. Accordingly Biot has been able to perceive it distinctly, even when the best polished steel is employed.

When we employ silver plates that have received the specular polish, the portion of light which assumes the fixed polarization at each reflection is still very considerable, though it is much less than when we employ the two mentioned metals. By a necessary compensation, the portion which assumes the moveable polarization is greater, and the phenomenon of the tints becomes more beautiful, and more easily observed. But the direction of the polarization of the white pencil being exactly intermediate between the two coloured pencils, it follows that it mixes with them in the refraction produced by the rhomboid; and it is only by refracting them in particular directions indicated by the theory that we can fully exhibit the law of their tints. This difficulty disappears almost entirely in the plates of silver polished by the hammer. Then the portion of light which assumes the fixed polarization at each re-

flection, becomes very small, compared with that which retains the moveable polarization; at least when the plates are not presented to the incident rays under a very great obliquity; for we know that in that case all the plane surfaces, even those unpolished on purpose, assume the specular polish. Accordingly, if we avoid great inclinations, and restrict ourselves to a small number of reflections, the laws of moveable polarization alone are perceived, and the tints of the pencils, which nothing alters, are developed with the greatest regularity according to the series of Newton's rings.

In this extract we have followed the author of the memoir step by step. We have taken from his experiments and explanations what may be useful to those philosophers who wish to confirm phenomena so new and curious; and we refer them to the memoir itself for the proofs of the different assertions which we have stated.

*Phenomena of successive Depolarization observed in homogeneous Fluids.* By M. Biot.

The investigations undertaken by M. Biot required that he should put crystallized plates in different fluids to make the rays penetrate to them very obliquely at their surfaces; and these experiments led him to the discovery of a phenomenon so much the more remarkable as it seems owing to the individual action of the particles of bodies on light, without any relation whatever to their state of aggregation.

This phenomenon is analogous to that which we observe in plates of rock crystal when we transmit light through them parallel to the axis of crystallization. In this case the force which produces the double refraction and the regular polarization has become null, because it proceeds from the axis of the crystal. But we then see other forces unfold themselves which the first, while energetic, concealed, and which being alone active modify the luminous molecules in a manner quite peculiar. The characteristic of this kind of force is, that instead of causing the axes of polarization of the luminous particles to oscillate, like other polarizing forces, it seems to give them a continued rotatory motion round the axis of the crystal, more rapid in the violet rays than in the blue; in the blue than the green; and so on inversely as the order of refrangibility. The influence of these forces is not confined to a change of position in the luminous particles; it communicates to them true physical properties, similar to permanent magnetism, the nature and intensity of which modify the motions which they afterwards assume when they are made to traverse other crystals.

These modifications and properties are very different from those possessed by molecules polarized by a single reflection; and M. Biot has just discovered them in a substance perfectly fluid, pure oil of turpentine.

The apparatus with which he made the first observation was a tube about three centimetres long, the two ends of which were closed by glass plates, in order to contain the different fluids in which he plunged the crystalline plates that he wished to examine.

When he employed in this manner oil of turpentine, he perceived that the polarized ray transmitted through the apparatus exhibited traces very weak indeed, but quite perceptible of depolarization; the extraordinary ray was of a dark blue, scarcely perceptible. Then on turning from right to left the achromatic rhomboidal prism which serves to analyse the transmitted light, it was seen that this extraordinary ray went on continually diminishing in intensity without changing colour till it became sensibly null at an azimuth of about 12°: and as the molecules which had at first undergone the ordinary refraction had been continually giving way to it in that interval, the ray appeared at that azimuth wholly polarized in the ordinary manner. On turning the rhomboid further, there was again formed a new extraordinary ray, very weak; but, instead of being blue, it was yellowish red. These characters, slight as they were, were precise, and showed a perfect identity between this class of phenomena and that presented by plates of rock crystal perpendicular to the axis.

M. Biot knew that in these last the developement of the colours increases in proportion as they become thicker, and that the amplitude of the minimum of the extraordinary pencil is proportional to their thickness. Hence he concludes that the increase of thickness in the mass of turpentine would have the same effect. M. Fortin had the goodness to construct for him very promptly another apparatus 16 centimetres in length, and having filled it with very pure oil of turpentine, he saw the most beautiful colours appearing when a ray of polarized light was made to pass through it. The nature of the tints in each azimuth, their direction, and the laws of their succession, were absolutely the same as those which he described in our memoirs for 1812, and which were produced by a plate of rock crystal of 2 095, from which we see that this action in the oil is about 80 times more weak than in the crystal.

M. Biot considers this as the first example of successive polarization produced in the interior of a fluid perfectly homogeneous, in which we cannot suppose any regular arrangement of the particles. We have seen from the example of rock crystal that the forces which produce it are distinct from those which crystallization developes.

This is not the case with the phenomena of polarization which depend upon the attractive and repulsive forces proceeding from the axis. These cannot exist in a liquid. Therefore on inclosing oil of turpentine in a hollow prism of glass with a considerable refracting angle, but the thickness of which scarcely exceeds a centimetre, not only no double refraction was observed, but, on account of the small thickness, there were no sensible vestiges of depolarization.

The author proposes to examine whether other fluids will exhibit analogous properties. He knows already that water, fish oil, ammonia, give no traces of similar properties at thicknesses much more considerable than that at which oil of turpentine exhibits them completely. But other liquids possess analogous properties.

The essential oil of laurel makes light turn from the right to the left, like oil of turpentine. The essential oil of lemons, on the contrary, and the solution of camphor in alcohol, make it turn from the left to the right. Thus we find in fluids the opposition already observed between the actions of this kind in plates of rock crystal, quite similar in their external characters. If we take two liquids which thus turn light in contrary directions, which we estimate by trying the absolute intensity of their individual action; and if we mix them in proportions inversely as these intensities, we produce neutral mixtures. We obtain this result, for example, when we mix together one part in volume of pure oil of turpentine with three parts of the solution of camphor in alcohol of 40°. But we must raise the temperature of the apparatus, because this mixture is transparent only while hot. Camphor alone dissolved in oil of turpentine diminishes its rotatory force; but it does not dissolve in sufficient quantity to neutralize it. (This notice was read to the Institute on Oct. 23 and 30, 1815.)

*On a new Species of coloured Rings observed in Plates of Iceland Spar cut perpendicularly to the Axis of Crystallization.* By M. Biot, Nov. 20, 1815.

The phenomena of polarization which crystals capable of double refraction produce on the rays of light that traverse them depend on the principal polarizing force which proceeds from the axis, and whose influence on the time of the oscillations is proportional to the square of the sine of the angle formed by the axis of the crystal with the direction of the refracted ray. Hence if we cut in any crystal a plate with parallel faces perpendicular to the axis, and if we place the eye in the prolongation of this line, the different rays which come to the eye through the plate will form different angles with the axis; and they will experience different polarizing forces, so much the more powerful as they become more oblique, and each of these forces will act in the plane passing through the axis and the refracted ray. It follows from this that if the luminous cone which comes to the eye is composed of white rays, all polarized in one direction, suppose the vertical, each of these rays will resolve itself into two tints, the colour of which, and the direction of the polarization, may be determined in general by the theory of moveable polarization, when we know the obliquity of the ray on the axis, and the thickness of the crystallized plate. Here the symmetry shows us that these effects ought to be the same all round the axis, at equal distances, so that each tint must form a ring concentric with it. This is what we really observe when we analyse light transmitted by means of an achromatic prism of Iceland spar, or by reflection from a mirror, in order to separate in each ray the portion which has preserved its primitive polarization from that which has lost it. The system of these last forms rings exactly similar to those which Newton observed in thin plates of water and air situated between two spherical objectives; and the squares of their diameters are exactly proportional to the numbers assigned by Newton for the

same tints in his table of coloured rings. But the system of rings, formed in the spar, has this particularity besides, that it is divided into four quadrants, by the four branches of a great black cross, which as they go further from the axis assume the appearance of the tails of comets, and whose direction is parallel and perpendicular to the primitive plane of polarization of the incident ray. It is easy to see that the rays included in these two planes do not lose their primitive polarization in passing through the crystallized plate; and this is the reason why the prism of spar, or the reflecting glass, which serves to analyse the transmitted light, excludes them from the system of rings which we are considering, these being solely composed of tints which have lost their primitive polarization. The diminution in thickness of the branches of the cross is likewise a consequence of the same theory; and from it also may be deduced the order of the colours of the rings, the proportion of their diameter, and even their absolute size, provided we know the thickness of the plate, and the distance of the eye at which we wish to measure the dimensions of the rings, and the results thus obtained have an exactness equal to that of the observations themselves. The formula which expresses them shows that for each crystal the sizes of the rings are reciprocally as the square roots of the thickness of the plates, as M. Biot has ascertained. For crystals of different natures this size ought to vary reciprocally as the square roots of the intensity of action. This at least is what the theory indicates, which served to explain the cause of these rings in plates of Iceland spar. This theory shows us that rings of the same size ought to be produced, though the thickness be unequal, in all the crystals which have no other polarizing force but that which proceeds from the axis, and which is connected with double refraction. This reservation is indispensable; for the crystallized masses on which we operate are always groupes of perfect crystals, exceedingly small, agglutinated together; and the way in which that junction operates often developes polarizing forces independent of their intimate nature. Thus the purest pieces of sulphate of lime are always a congeries of plates, and the finest crystals of beryl are merely assemblages of fine needles agglutinated together. Accordingly sulphate of lime exercises particular polarizing forces in the direction of its plates, and the beryl in the direction of its joints. These crystals, and all those which are affected in the same way, cannot exhibit the phenomena described above as belonging to a pure crystal; the variations of the polarizing force round the axis being sensibly modified by the forces depending on the structure, which become comparable to it. These peculiarities, which vary without any law from one crystal to another, can only be deduced from experiment, and never can be arranged under any theory.

### MEMOIRS APPROVED BY THE CLASS.

*Reflecting Circle*, presented by M. Gambey.

This circle differs in some respects from that of Borda; and these differences are either not quite new, or not free from incon-

veniences. But the pieces of which it is composed, and the graduation in particular, appeared carefully made, and by a good method. The commission was of opinion that M. Gambey had not laboured unsuccessfully to perfect his instrument, and that his zeal was deserving of encouragement.

The Commissioners were MM. Bouvard, Burckardt, and de Rossel, Reporter.

*Explanation of the Operations executed in the Departments of the Haut et Bas-Rhin, to serve as the Basis of a Map of Helvetia, and to the Measure of the Parallel from Strasburg to Brest.* By M. Henri, Colonel of the Royal Corps of Geographical Engineers.

The work of M. Henri contains the details and the results of a great geodesical measurement, made by order of government, and under the immediate inspection of the general depot of war. This useful and fine establishment has long possessed numerous materials of the same kind; but the long and minute calculations which they require, the desire to fill up some gaps, the care requisite in drawing and engraving the maps, some considerations respecting the public interest, and particularly the active part which the officers of the corps of Royal Geographical Engineers were obliged to take in the campaigns of our armies, have hitherto retarded the complete publication of their operations.

All the country lying between the Meuse and the Rhine is covered with triangles connected with the new meridian of France, near Dunkirk, and with the geodesical operations executed in Holland, by General Krayenhoff. (See the notice for 1813.) This undertaking is completed by the observations of the latitude, and of the azimuth, made at Aix-la-Chapelle. It would give an arc of the meridian from Groninguen to Treves, and an arc of the parallel of Dunkirk to Cologne, if circumstances had permitted the addition of some new astronomical determinations to those already made.

The Depot of War possesses likewise the triangulation of Suabia and Bavaria. They are connected with each other, and with those of Bohemia, Saltzburg, and the archduchy of Austria, executed by Austrian engineers. A great base has been measured in the neighbourhood of Munich, and many astronomical observations have been made in that capital and in the castle of Hohenstein.

The trigonometrical network, which comprehends the greatest part of Westphalia and of Lower Saxony, is connected with the geodesical operations of Holland, and with those of the Copenhagen Academy in the kingdom of Denmark. We have likewise an arc of the meridian from Cassel to Copenhagen, and an arc of the parallel of Amsterdam. It is only wanted that astronomical observations be made in sufficient number to remove all doubts respecting the longitudes and latitudes of the extreme points.

The triangulation of the island of Elba is connected with that which was executed in Corsica and on the coast of Tuscany in 1789. It is accompanied with astronomical observations made at Porto Ferrajo.

Almost the whole surface of Lombardy and Piedmont is covered with

a triangulation, executed with repeating circles, and which extends from east to west in Piedmont, not far from Little Saint-Bernard. This grand network rests upon the six verefied bases of Turin, Milan, Padua, Tagliamento, Rimini, and Rome. It is accompanied by astronomical observations made at Milan, Rimini, Rome, Venice, and St. Salvador, and may furnish very valuable data for the measure of an arc of the parallel between Turin and Fiumé.

The triangulation of the Appenines from Mondovi to Savona is connected with the preceding. That of Savoy reposes on one of the sides of the meridian of France, by means of a provisional network, which goes from Mont-Blanc to Mont-d'or in Auvergne, and from that point to Bort and Hermont. Astronomical observations have been made with the greatest care at Clermont, Geneva, and Lyons. It will be very curious to compare the longitude of the first of these towns with that of d'Evaux, which was determined by one of us.

The trigonometrical network destined to connect Mont-Blanc with the new meridian of France, to assign correctly the geographical position of this highest summit of the Alps, is in a great measure completed. Finally, there is a project of uniting Brest to Strasburg, by a chain of well-arranged triangles, leaning, on the one hand, on the base measured near Colmar with platinum rods, and on the other on a similar base which it is proposed to measure near Brest.

If to these details we add that the construction of the maps has occasioned very interesting researches in the depôt itself; that the numerous mensurations of which we have spoken were made by skilful engineers, furnished with excellent instruments; that the observations have been calculated according to the most accurate formula, and that all the minutiæ have been attended to with as much care as if the special object of the operations had been to determine the figure of the globe, we cannot avoid perceiving that this undertaking, executed by the royal corps of Geometrical Engineers, will form from its extent and accuracy, when some gaps which we have pointed out are filled up, one of the finest monuments that has ever been raised to the sciences, and particularly to geography.

The work, which Colonel Henri has already completed, contains all the details that can be desired respecting the operations, mensurations, and calculations; and gives the longitudes, latitudes, and heights, above the level of the sea of each of the stations.

The base of Ensisheim, on which this part of the operations reposes, was measured by means of three of the platinum rods, which were employed at the bases of Melun and Perpignan, with the same attention and the same success. The length at the temperature of 13° of Reaumur (61°$\frac{1}{4}$ Fahrenheit) is 9771·2056 toises of Peru. It is connected with a base of 7749·54 toises measured near Darmstadt, by M. M. Eckardt and Schleyermacher. The agreement between the calculation and the measurement is 0·23 metre, the same sensibly as that of the bases of Melun and Perpig-

nan, which are connected together by 64 triangles. The base of Bavaria deduced equally from that of Ensisheim, by a series of 24 triangles, exceeds the base measured only 0·14 metre. This agrees nearly with the English bases, which are not separated by so great a number of triangles.

The mean of the two measurements, which M. Tralles has made of the marshes of Arberg, is smaller by 1·34 metre than would be deduced from the base of Ensisheim.

Finally, M. Henri has joined his base to that of Melun by a series of 75 triangles, some of which have been measured by himself or by M. Tranchot, and the rest by General Krayenhoff, and the last belong to the meridian of France. Notwithstanding this great *detour*, and the numbers of triangles, measured by four observers supplied with different kinds of instruments, the deviation is only 1·34 metre; as for the base of M. Tralles, the perpendicular proposed to be drawn from Brest to Strasburg will furnish a more direct comparison. But we see already that the base of Ensisheim has been determined with all the exactness which could be expected, either from the skill of the observer, or the means that he had in his power.

The operation of M. Henri, compared with that of Peru, gives $\frac{1}{274}$ for the difference between the two diameters of the earth. It would be $\frac{1}{424}$ if we compared it with that of Lapland. The mean $\frac{1}{298}$ differs very little from the number usually adopted.

If we suppose the diminution of the polar axis $\frac{1}{334}$, and the height of the pole at Strasburg perfectly determined, that of Bern will be too great by 3″, and that of Lichtenburg too little by 3·19″. But these errors would be reduced to one half by supposing an error of 1·5″ in the latitude of Strasburg.

The observations of the azimuth have likewise all the precision possible and desirable, and the commission concludes that the operation of Colonel Henri is very worthy of the approbation of the Class. The commissioners were MM. Dalambre and Arago, reporter.

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## Article XIV.

### SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Lectures.*

The following arrangements have been made for Lectures at the Surrey Institution during the ensuing season:—

1. On Chemistry, by John Murray, Esq. To commence on Tuesday, Nov. 12, at seven o'clock in the evening precisely, and to be continued on each succeeding Tuesday.

2. On Aerostation, by John Sadler, Esq. To be delivered on Friday evenings, Nov. 15 and 22, at the same hour.

3. On the Principles and Practical Application of Perspective, by John George Wood, Esq. To commence on Friday, Nov. 29, and to be continued on each succeeding Friday at the same hour.

4. On Astronomy, by John Millington, Esq. Civil Engineer. To commence in January, 1817.

5. On Music, by W. Crotch, Mus. D. Professor of Music in the University of Oxford. To commence in February, 1817.

## II. *New Method of dissecting the Brain.*

The Editor has been requested to insert the following notice in the present number of the *Annals*:—

During last month Dr. Spurzheim made a very splendid display of the structure of the brain, in the new mode of dissecting it, before a scientific class of surgeons, and other Professors, at Edinburgh, and has received the approbation of various persons of eminence in the science of anatomy. The novelty of the subject excited much interest, and he had a crowded audience to his lecture.

## III. *On Sir H. Davy's Safety Lamp.*

(To Dr. Thomson.)

SIR,

Finding it stated by a Correspondent in the last number of your *Annals* that Sir H. Davy has never been able to account for the security afforded against the passage of flame by his wire-gauze fence, it is with some diffidence that I propose the following explanation, which appears so simple and obvious that I can hardly imagine that it would have escaped the notice of so distinguished a chemist had there not been some objection to it which I do not at present perceive.

It is evident that the rapidity with which a given volume of gas (in a state of inflammation, or otherwise) is cooled down to the temperature of the surrounding atmosphere will be in direct proportion to the surface exposed, and that in the transmission of air through a porous body, the quantity of surface will increase with the number of apertures through which it is made to pass.

Thus if a lamp surrounded with wire-gauze is fed with an atmosphere containing a large proportion of carbureted hydrogen, the latter gas, on coming in contact with the lamp, will inflame, and the expansion thus occasioned force a considerable portion of it in a state of combustion through the apertures of the wire-gauze, into the external atmosphere, as if nothing intervened.

In passing through these apertures, however, the gas is divided into so many minute jets, and so large a surface is, therefore, exposed to the external air, that it has its temperature considerably lowered, and is thus unable to communicate to any given portion of the external atmosphere heat sufficient to excite combustion.

I scarcely need remind your readers that a similar fact has been observed by Dr. Wollaston in the case of platina wire, which it

becomes more difficult to ignite when $\frac{1}{3000}$ than when $\frac{1}{2000}$ of an inch in thickness, the extent of surface by which it is cooled not diminishing in the same ratio with the diameter, and the radiation of heat from thence going on in consequence too rapidly to admit of a sufficient accumulation of caloric through the mass, to raise its temperature to the necessary point.

*Aug.* 9, 1816. PHILO-CHEMICUS OXONIENSIS.

## IV. *Observations on Mr. Holmes's Letter published in the Annals of Philosophy, vol. viii. p.* 130. By Mr. Knight.

(To Dr. Thomson.)

SIR,

It is not without feelings of regret that I read a letter published in your *Annals of Philosophy* by Mr. Holmes.

You will oblige me by inserting in your next Number the following endeavour to do away with any impression which that letter has made unfavourable to the credit of the Establishment with which I am connected.

Mr. Holmes speaks of a visit to the Gas Works in Dorset-street, for the purpose of trying experiments on Sir H. Davy's safety lamps, and mentions persons who were present at the trials; does he publish their names to attach importance to his liberal investigation? If he does, it is fair to ask what credit his experiments derive from the presence of Mr. Wheatcroft, who is employed there to pay the workmen. Mr. Morris, who superintends the works by night, and Mr. May, whom I never heard of before, obtain importance and publicity in Mr. Holmes' letter. How Mr. Holmes had access to the works I know not; neither myself nor any other proprietor would knowingly lend ourselves or the establishment to experiments not openly conducted. Whenever gentlemen, who are men of science, have wished to visit the works, an application has been usually made to me as engineer, and I have always been ready to afford free inspection, and give every facility to experiments; but in this instance I never heard of Mr. Holmes or his visit till I saw his own report in your *Annals*, and that only yesterday, when your last number was put into my hands. As the active partner and manager of the establishment, and having my name and credit more immediately connected with the works than any other proprietor, I feel myself called on to disclaim participation in any of the experiments conducted there by M. Holmes, with results so opposite to those of other persons, who with liberal motives have felt anxious to prove the security which Sir H. Davy's discovery promises, and with the benevolent hope of its application to the safety of the members of so valuable a class of society.

After what I have seen and heard of Sir H. Davy's lamps, my conviction of their security is not shaken by Mr. Holmes' report, which looks more like an endeavour to serve his friend Dr. Clanny than the public. Any means which our works in Dorset-street can afford to scientific and liberal men, of investigating further the appli-

cation of Sir H. Davy's, or any other discovery, will be granted with pleasure by,

Sir, your obedient humble servant,

*City Gas Works, Dorset-street, Aug. 20, 1816.* WM. KNIGHT, Engineer.

V. *Quantity of Rain which fell during seven Months,* 1816, *at Carbeth, in Stirlingshire.*

The rain-guage is made by Crichton, and is at an elevation of about 490 feet above the level of the sea :—

| | |
|---|---|
| January | 3·869 |
| February | 3·259 |
| March | 2·807 |
| April | 1·673 |
| May | 3·442 |
| June | 2·750 |
| July | 4·618 |
| Total | 22·418 |

At this place, on Sunday, July 21, there fell in 25 minutes, from five minutes before, till 20 minutes past, 2, P. M. 0·710. Barometer, at beginning, 28·81. End, 28·89. Thermometer, 68°. End, 65°.

VI. *Method of separating Tantalum from Silica.*

(To Dr. Thomson.)

SIR,

If you will state in your next number of the *Annals* how it is possible to separate oxide of tantalum from silex, both being in a state of alkaline solution, you will much oblige your constant reader,

*Sept.* 6, 1816. D. D.

I have never had an opportunity of making any experiments on this scarce metal. From Berzelius' experiments, published in the fourth volume of the Afhandlingar, from which my account of tantalum in the last number of the *Annals of Philosophy* was taken, it appears that its oxide is precipitated white by muriatic acid from its combination with potash. This property will probably furnish an easy method of separating oxide of tantalum from silica when both are held in solution by means of potash. Add to the solution muriatic acid. Filter to remove the oxide of tantalum ; then evaporate to dryness, in order to obtain the silica in the usual way.—T.

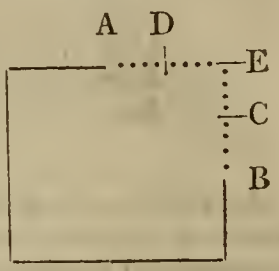
VII. *On the Demonstration that no Part of the Circle is a Straight Line.*

(To Dr. Thomson.)

DEAR SIR,

I perceived in your *Annals* a most ingenious attempt to demonstate that no part of a circle is a straight line. Now, Sir, although

the attempt deserves the highest praise for its ingenuity, I am nevertheless humbly of opinion that the conclusions drawn are by no means satisfactory. In the arc of a circle, the author takes a small part which he supposes to be a straight line. But if, says he, this part of it is a straight line, then another part (C B), which is equal to it, must also be a straight line. And, for the same reason, the part including the point where these two lines are joined must likewise be a straight line, and therefore the whole must be a straight line. Now this is just asserting that if the parts of a circle are composed of straight lines, the circle must be a straight line itself. By the same way of reasoning, we might assert that, because a square is not a straight line, therefore *no part* of it can be a straight line, viz. :—Of a part, A B, of a square, take a portion, B E, which suppose to be a straight line. But if B E is a straight line, A E, which is equal to it, will also be a straight line. For the same reason, C D, which includes the point in which A E and B E are joined, must likewise be a straight line; and therefore the whole part, A B, is a straight line, which is evidently absurd. This is precisely the train of reasoning followed by your Correspondent, which would lead us, *in this case*, to infer that, because a square is not a straight line, therefore no part of it can be one; a conclusion so absurd that few will be disposed to adopt it.




Should you think this observation worthy of a place in your work, you will highly oblige,

Sir, yours truly,

*Glasgow, July* 27, 1816. K———.

## VIII. *Answer to Mr. Lockhart's Observations.* By Mr. Henry Atkinson.

(To Dr. Thomson.)

SIR,

When your correspondent, Mr. Lockhart, first published that he had discovered a new cube root of 64, I took it for granted that he expected and wished to have the subject freely and fairly discussed. Among others, I examined it, and pointed out a part of his investigation, where I thought he had overlooked an ambiguity in the sign of the quantity $\left(t^2 - \frac{b}{3}\right)$. The equation which he first used was one that could be reduced by Cardan's rule, and I corrected his formulæ for such equations. But, instead of meeting my objections fairly, he entirely evaded them, by bringing forward quite a different equation, one belonging to the irreducible case. I then pointed out the distinction between the formula belonging to the *reducible* and

*irreducible* cases; and also attempted to show *why* this distinction should be made; but in this latter part I failed, from considering the quantity $\left(t^2 - \frac{b}{3}\right)$ as a negative quantity in reducible equations; whereas the fact is, that it is only that *part* of it which is not imaginary that is negative. This mistake Mr. Lockhart has laid hold of with avidity, and seems to infer that, because I have made one unfounded assertion, all that I have advanced must be false, while all that he has advanced must be true; for if this be not the inference which he wishes to be drawn from the exposure of my error, he has again evaded the main point; as he has not assigned even so much as the shadow of a reason why he takes $\left(t^2 - \frac{b}{3}\right)$ in preference to $\left(\frac{b}{3} - t^2\right)$ as the square root of the quantity $\left(\frac{b^2}{9} - \frac{2\,b\,t^2}{3} + t^4\right)$; and yet this *blind*, and even *improper*, preference is the only thing on which his *new* cube root of 64 depends for its existence, as will appear from the following general investigation.

In equations of the form $x^3 - b\,x = c$, the roots of which are represented by $x$, $-t$, and $-v$, it is well known that $x = (t + v)$, $b = (t\,x + v\,x - t\,v)$, and $c = t\,v\,x$. It is also well known that if the equation be reducible, the two roots $-t$ and $-v$ must be of the form $-m - \sqrt{-n}$ and $-m + \sqrt{-n}$. Now let $m$ and $n$ be such quantities that $t = m + \sqrt{-n}$ and $v = m - \sqrt{-n}$, then will $x = 2\,m$, $b = (3\,m^2 - n)$, and $c = (2\,m^3 + 2\,m\,n$. Hence the equation $\pm\left(t^2 - \frac{b}{3}\right. \times \pm\sqrt{\left(\frac{t^2}{4} - \frac{b}{3}\right)} = \pm\sqrt{\left(\frac{c^2}{4} - \frac{b^3}{27}\right)}$* will become $\pm\left(-\frac{2\,n}{3} + 2\,m\sqrt{-n}\right) \times \pm\sqrt{\left(-\frac{3\,m^2}{4} + \frac{n}{12} + \frac{m}{2}\sqrt{-n}\right)} = \pm\sqrt{\left(3\,m^4\,n + \frac{2\,m^2\,n^2}{3} + \frac{n^3}{27}\right)}$ or which is the same $\pm\left(-\frac{2\,n}{3} + 2\,m\sqrt{-n}\right) \times \pm\sqrt{\left(\left(\frac{1}{36} + \frac{m}{6\,n}\cdot\sqrt{-n} + \frac{m^2.(-n)}{4\,n^2}\right) \times 3\,n\right)} = \pm\sqrt{\left(\left(m^4 + \frac{2\,m^2\,n}{9} + \frac{n^2}{81}\right) \times 3\,n\right)}$. And by extracting the square root we have $\pm\left(-\frac{2\,n}{3} +\right.$

* For the method of finding this equation, see Mr. Lockhart's letter of May 9, 1815. I have here supplied the double sign, for the purpose of investigating what the sign of $\left(t^2 - \frac{b}{3}\right)$ ought to be: for it is well known that any one of the three quantities to which the sign $\pm$ is prefixed, when taken singly, may be either $+$ or $-$; and therefore the sign which ought to be prefixed to the quantity $\left(t^2 - \frac{b}{3}\right)$ can only be determined by considering its connexion with the other quantities.

$2\,m\,\sqrt{-n}) \times \pm (\frac{1}{6} + \frac{m}{2\,n} \cdot \sqrt{-n}) \times \pm \sqrt{3\,n} = \pm (m^2 + \frac{n}{9}) \times \pm \sqrt{3\,n}$. Now if we multiply the two quantities $\pm (-\frac{2\,n}{3} + 2\,m\,\sqrt{-n})$ and $\pm (\frac{1}{6} + \frac{m}{2\,n}\,\sqrt{-n})$ together, the equation will become $-(m^2 + \frac{n}{9}) \times \pm \sqrt{3\,n} = \pm (m^2 + \frac{n}{9}) \times \pm \sqrt{3\,n}$, an identical equation, as it ought to be, if the under sign of the quantity $(m^2 + \frac{n}{9})$ on the right hand side of the equation be used; but if the upper sign be used, the equation is evidently false, whenever $n$ has any value; and consequently the equation $\pm (t^2 - \frac{b}{3}) \times \pm \sqrt{(\frac{t^2}{4} - \frac{b}{3})} = \pm \sqrt{(\frac{c^2}{4} - \frac{b^3}{27})}$, from which it is derived, is likewise false, if the upper sign on the right hand side be used: so that $\pm (t^2 - \frac{b}{3}) \times \pm \sqrt{(\frac{t^2}{4} - \frac{b}{3})}$ can only be equal to $-\sqrt{(\frac{c^2}{4} - \frac{b^3}{27})}$, and $\mp (t^2 - \frac{b}{3}) \times \pm \sqrt{(\frac{t^2}{4} - \frac{b}{3})}$ can only be equal to $+\sqrt{(\frac{c^2}{4} - \frac{b^3}{27})}$: from which, by proceeding as Mr. Lockhart has done,* we obtain $-\frac{t}{2} + \sqrt{(\frac{t^2}{4} - \frac{b}{3})} = \sqrt[3]{(\frac{c}{2} - \sqrt{(\frac{c^2}{4} - \frac{b^3}{27})})}$ and $-\frac{t}{2} - \sqrt{(\frac{t^2}{4} - \frac{b}{3})} = \sqrt[3]{(\frac{c}{2} + \sqrt{(\frac{c^2}{4} - \frac{b^3}{27})})}$: which are the very same roots as were given in my letter of Aug. 12, 1815, for the *reducible case.*

But if the equation belong to the irreducible case, it is well known that $t = m + \sqrt{n}$, $v = m - \sqrt{n}$, and $x = 2\,m$. Hence $b = (3\,m^2 + n)$, and $c = (2\,m^3 - 2\,m\,n)$, and consequently, by substitution, the equation $\pm (t^2 - \frac{b}{3}) \times \pm \sqrt{(\frac{t^2}{4} - \frac{b}{3})} = \pm \sqrt{(\frac{c^2}{4} - \frac{b^3}{27})}$ will become $\pm (\frac{2\,n}{3} + 2\,m\,\sqrt{n}) \times \pm \sqrt{(\frac{m^2\,n}{4\,n^2} - \frac{m}{6\,n}\,\sqrt{n} + \frac{1}{36})} \times \pm \sqrt{-3\,n} = \pm \sqrt{(m^4 - \frac{2\,m^2\,n}{9} + \frac{n^2}{81})} \times \pm \sqrt{-3\,n}$, and by extracting the square root we have $\pm (\frac{2\,n}{3} + 2\,m\,\sqrt{n}) \times + (\frac{m}{2\,n}\,\sqrt{n} - \frac{1}{6}) \times \pm \sqrt{-3\,n} = \pm (m^2 - \frac{n}{9}) \times \pm \sqrt{-3\,n}$. Now let the two quanti-

* See his letter of May 9, 1815.

ties $\pm \left(\frac{2n}{3} + 2m\sqrt{n}\right)$ and $+ \left(\frac{m}{2n}\sqrt{n} - \frac{1}{6}\right)$ be multiplied together, and the equation will become $\pm \left(m^2 - \frac{n}{9}\right) \times \pm \sqrt{-3n} = \pm \left(m^2 - \frac{n}{9}\right) \times \pm \sqrt{-3n}$; which is an identical equation, as it ought to be. But had we taken the other root of the quantity $\left(\frac{m^2 n}{4n^2} - \frac{m}{6n}\sqrt{n} + \frac{1}{36}\right)$, viz. $-\left(\frac{m}{2n}\sqrt{n} - \frac{1}{6}\right)$, and then proceeded as before, we should have obtained this equation, viz. $\mp \left(m^2 - \frac{n}{9}\right) \times \pm \sqrt{-3n} = \pm \left(m^2 - \frac{n}{9}\right) \times \pm \sqrt{-3n}$, which is evidently false, whenever $n$ has any value: consequently $\pm \left(t^2 - \frac{b}{3}\right) \times + \sqrt{\left(\frac{t^2}{4} - \frac{b}{3}\right)} = \pm \sqrt{\left(\frac{c^2}{4} - \frac{b^3}{27}\right)}$ would be false if the quantity $\sqrt{\left(\frac{t^2}{4} - \frac{b}{3}\right)}$ had the *negative* sign before it, but is true as it stands. And by proceeding with this as before, we obtain $-\frac{t}{2} + \sqrt{\left(\frac{t^2}{4} - \frac{b}{3}\right)} = \sqrt[3]{\left(\frac{c}{2} + \sqrt{\left(\frac{c^2}{4} - \frac{b^3}{27}\right)}\right)}$ and $-\frac{t}{2} - \sqrt{\left(\frac{t^2}{4} - \frac{b}{3}\right)} = \sqrt[3]{\left(\frac{c}{2} - \sqrt{\left(\frac{c^2}{4} - \frac{b^3}{27}\right)}\right)}$: which are the same roots as I gave for the *irreducible* case in the letter above referred to.

And, in a similar manner, may the roots connected with $x$ and $v$ be found to be the same as were given in the same letter.

From the above investigation, it appears that Mr. Lockhart's formula connected with $t$ is *correct*, when applied to *irreducible* equations, as I formerly observed; but *incorrect* when applied to *reducible* ones. It likewise appears that what he imagined to be a cube root of 64 is in reality a cube root of 8.

I hope, Sir, that your Correspondent will now be convinced that I have proved something, viz. that he was mistaken when he asserted that he had discovered a *fourth* cube root of 64. But should it be found that I have again committed some error, and have really proved nothing, this can never avail him any thing; for it will still be necessary for him to assign some *good* reason why he has taken $\left(t^2 - \frac{b}{3}\right)$ in preference to $\left(\frac{b}{3} - t^2\right)$ as the square root of the quantity $\left(\frac{b^2}{9} - \frac{2bt^2}{3} + t^4\right)$; and till this be done, which I believe will not be soon, there cannot be even the shadow of a proof that he is right. He will also have to show *why* he has taken $\left(\frac{b}{3} - v^2\right)$ in preference to $\left(v^2 - \frac{b}{3}\right)$ as the square root of

the quantity $\left(\frac{b^2}{9} - \frac{2\,b\,v^2}{3} + v^4\right)$; which he must have done, in order to obtain his formula connected with $v$. He will likewise have to show how he reconciles these two operations with one another. Nay, he will have even more than all this to do; he will have to show that *simple* equations admit of *more* roots than *one*; for if it be admitted that simple equations have only *one* root, it has been *demonstrated** that every equation must have *just as many roots* as it has dimensions, and *no more*. And should he still be inclined to hold his opinion, new or old it matters not, that "the number of cube roots belonging to numerical quantities is infinite," it will be necessary for him to point out *other* roots of the quantity $\sqrt[3]{(36 \pm \sqrt{784})}$ besides the *six* which I have exhibited,† and which it ought to have, according to the commonly received opinion. If the number of roots be really *infinite*, he certainly can have no difficulty in finding *other* six or seven.

I cannot conceive, Sir, how Mr. Lockhart could suppose it possible for me, or any person else, to dismiss imaginary quantities from the question, when the question itself was about imaginary quantities, viz. the imaginary cube roots of numerical quantities: and if he could not suppose it possible, for what purpose could he introduce the following sentence into his last letter:—"Much less has he dismissed from the question those imaginary quantities which perplex the science of algebra, and which on that account I much wish discarded from it for ever." If Mr. Lockhart ascribes any part of the perplexity which has taken place in the present discussion, to imaginary quantities, he certainly does them injustice; for it is manifest that whatever perplexity may have arisen, in this case has been solely occasioned by the ambiguity in the sign of the square root of a quantity; and surely he would not wish to discard the sign $\pm$ from the science of algebra. Besides, as several very curious and useful *theorems* have been discovered by the use of imaginary quantities, which probably would not yet have been discovered, had they never been used, I should suppose it would even be *prudent* to retain them, as instruments of investigation at least, if not of demonstration: and I am the more inclined to this opinion, as I have yet to learn that they necessarily lead to a false result, in any one instance, when legitimately applied.

With respect to the mistake which I made in my last letter, in stating that $\left(t^2 - \frac{b}{3}\right)$ is negative in reducible equations, I believe it will not be necessary for me to make any apology: being convinced that not one of your readers would ever suppose that it was done knowingly and intentionally; and that they will, without my telling them so, believe that I am very sorry for it. I might,

* See Bonnycastle's Algebra.

† See my letter of June 16, 1815.

perhaps, have kept it more in the back ground; but I have always considered it as the most honourable way to acknowledge my mistakes at once, without attempting to make any paltry excuse.

Yours, &c.

*Newcastle, Aug.* 21, 1816. HENRY ATKINSON.

## IX. *New Metals from Barytes, Strontian,* &c.

The following are extracts of two letters which I have received from Dr. Clarke, of Cambridge:—

(To Dr. Thomson.)

DEAR SIR, *Cambridge, Aug.* 23, 1816.

If Dr. Wollaston have not yet left London for the Continent, he will probably have informed you of some remarkable results which I have lately obtained by burning a highly condensed mixture of the gaseous constituents of water. Lest, however, this should be the case, I beg to state, as briefly as possible, that I have succeeded in fusing all substances hitherto described as *infusible.*

Having obtained *barytes* in the *metallic* state, without the aid of any electrical agent, I this day repeated the whole experiment, and exhibited the *new metal,* in the presence of our Professor of Chemistry, and the Rev. Mr. Holmes, and the Rev. Mr. Hughes, and other Members of this University. It is somewhat *ductile,* for it admits of being filed. Its lustre is as great as that of *silver,* and the purest specimens of it resemble *silver* in colour. It does not immediately become oxided by exposure to the atmosphere. One specimen has retained its lustre during three days and nights; but this does not always happen. I have ventured to name this new metal *plutonium,* because we owe it entirely to the *dominion of fire,* because nothing can be more absurd than to name a metal from any derivative of βαρυς, whose *specific gravity* is inferior to that of *manganese* or *molybdenum.*

I have the honour to be, dear Sir, yours faithfully,

E. D. CLARKE.

I have also obtained *strontian* in the *metallic* state, and am now proceeding with the other earths.

(To Dr. Thomson.)

DEAR SIR, *Cambridge, Sept.* 9, 1816.

I forwarded a letter to you, which I hope you received safe. It was to inform you that I had obtained metals from *barytes* and *strontian* by mixing these earths, in a pure state, with lamp oil, and then exposing them to the heat of an ignited gaseous mixture of the constituents of water; two parts of *hydrogen* gas, by bulk, being mixed with one part of *oxygen* gas. I have since obtained a metal from *silex.* If I should obtain further results worthy of being communicated to you, I will send you the earliest information.

I remain, dear Sir, yours faithfully,

E. D. CLARKE.

## X. *Plano-cylindrical Glasses.*

I am indebted to an eminent optician for the following valuable communication relative to this subject, which has attracted some attention in the preceding numbers of the *Annals of Philosophy* :—

(To Dr. Thomson.)

SIR,

When the account of the double plano-cylindrical lenses was first published in this country, I set myself to inquire into their properties, in order to find, if possible, something that might justify the expectation held out by the French inventor, of the advantages to be obtained by using these lenses in optical instruments. The result of my inquiry not being favourable to them, I thought no more of them till I read a letter in the *Philosophical Annals* for June last, in which some queries are proposed concerning them; this induced me to take the liberty of sending you the following investigation. I would have done it sooner, if I had not waited till an opportunity offered of executing one of the lenses in such a manner as to enable me to judge practically of its effect.

If sections are made of a common spherical lens in planes passing through the axis, all those sections are bounded by like circular arcs; and all rays of homogeneous light, incident upon the lens in those planes, and in directions parallel to, and equidistant from, the axis, are caused, by the refractive power of the glass, to unite in one point in the axis; the difference between the place of union of the central rays, and of those incident near the margin, being the aberration caused by the spherical form of the lens. Now, if one of the double plano-cylindrical lenses be cut in a similar manner, by planes passing through the axis, each section, except two, will be bounded on both sides by curve lines, which are portions of ellipses; but, in each of those two, one of the sides will be a straight line, and the other an arc of a circle.

The foci, or places of union of all the rays, both central and marginal, that are incident upon the cylindrical lens in the plane of the two latter sections, coincide with the foci of a plano-spherical lens, whose radius of curvature is equal to that of each surface of the double cylindrical lens, and the radii of curvature, (taken at the vertex or centre of the lens), of all the elliptical sections, will be such as to make the focus of central rays incident in the planes of those sections coincide also with the forces of central rays refracted by a plano-spherical lens of the curvature above-mentioned; therefore the focus of all the central rays refracted by the cylindrical lens may be considered as coinciding with that of a plano-spherical lens of equal curvature. Now if the sections of the cylindrical lens were all considered as bounded by arcs of circles, the radius of each arc being constant, there would be, evidently, the same alteration of the central and marginal rays as in the common spherical lens; but, from the nature of the ellipse, it is known that the radii of curva-

ture decrease as we proceed from the vertex of the conjugate axis, (which is that of the centre of the lens), to that of the transverse axis, therefore the radii of curvature of each elliptical section of the cylindrical lens are shorter near the margin than about the centre of the lens; consequently the refraction there will be greater, and the focal distance of the marginal rays shorter, than if the radii of curvature had been constant. From this it is evident that the whole quantity of the aberration arising from the form of the lens is greater, when rays are refracted by the cylindrical, than when they are refracted by the spherical lens, and therefore the former is so much the less proper for all optical purposes.

As it may give satisfaction to persons who have not had opportunities of seeing those lenses, to be informed of their effect, I beg leave to state that I ground one of them with great accuracy in a cylindrical tool, whose radius of curvature was four inches, and comparing the image formed by it, in a sort of camera-obscura, with that formed by a common spherical lens of the same size and focal length, I found the margin of the former image visibly more distorted, and the whole more indistinct than the image formed by the latter; the difference between them was so great that, after the most liberal allowance for any imperfection that might be suspected in the formation of the new lens, there could remain no doubt of the superiority of the old one. I am, Sir, your most obedient servant,

N.

## XI. *Models of Crystals.*

(To Dr. Thomson.)

SIR,

I have just executed some models to accompany Jameson's Characters; they are in two boxes, one of which contains 34 models of the first two plates of the above work, to illustrate Werner's Crystallographic method: the remaining are 19 primitives. They are numbered after the above plates, the first the letter F, and the others with P. The price of the whole is one guinea. They may be seen at Mr. Mawe's, Strand; Mr. Bate's, Poultry; and Mr. Jones', Holborn.

I purpose making a complete set to accompany Jameson's Mineralogy, in the same stile, consisting of 219 figures, for 4*l.* 4*s.*, with the names neatly stamped on each model.

I am, Sir, your most obedient servant,

14, *Gee-street, Somers Town,*
*Aug.* 13, 1816.

N. I. Larkin.

## XII. *Mr. Donovan's Defence of his Prize Essay against the Author of the Letter inserted in the Annals of Philosophy, vol. vii. p.* 473.

(To Dr. Thomson.)

SIR,

On looking over a paper in your last, entitled "Observations on Mr. Donovan's Essays," it appeared to me that the author has

incautiously decided on a matter which he did not understand. As far as I can collect, he labours to show that the Royal Irish Academy neither honoured me with the prize nor with their countenance: and of this the only evidence brought forward is, that they did not award me the greatest possible sum. In publishing the question, the Academy give notice that the greatest possible sum is 50*l.*; this cannot be exceeded, but may be diminished to any amount below it. Hence, however, we do not infer that the sum of 50*l.* enters into the definition of the Academic prize, and that the abstraction of 10*l.* alters the name and nature of the thing so completely. Were this the case, but one of the Essays in the last volume of Transactions could be said to have obtained the prize. Some of the authors obtained sums much under what was awarded to me, and by this I do not intend to insinuate that their merits were not superior to mine: the questions given to these ingenious persons were in the present fashion of knowledge of less interest than that which it was my chance to undertake. The real fact of the matter is, that any sum, awarded as a premium for an Essay, is *the prize*; and this is demonstrable from the volume lately published by the Academy. The Essays of Miss Kiernan, Mr. Phelan, and Mr. Carmichael are, in the contents, entitled "Prize Essays," yet none but one obtained the full sum. This, I consider a direct proof.

On all these accounts, I am surprised to find your correspondent venture upon an unqualified contradiction of my statements, and oppose to them assertions which do not even appear to prove any one point.

As to the fears expressed lest the Academy should be held accountable for the "startling novelty of my opinions," they prove a high degree of unacquaintance with academic usage. The Royal Irish Academy prefix to their transactions an advertisement, intimating that "as a body they are not answerable for any opinion, representation of facts, or train of reasoning, which may appear in the papers. The authors of the several essays are alone responsible for their contents." This being the case, it was not required, nor supposed, that the Academy should, "in the slightest degree, admit my conclusions," and therefore it was not "necessary to state that the Academy did not give Mr. Donovan *the* prize." The real statement of the case appears to be, that your correspondent fears what there was no danger of; that he contradicts true statements; and that he has not duly considered what he has hazarded in a public print. But these are the unquestionable prerogatives of anonymous writers.

There was another topic which with more appearance of justice might have been urged against me. The Essay, now published, is not *identically* that which obtained the prize, although the contrary is stated on the title. Much new matter has been added, both in the history, and in the examination of hypothesis: and the third part, containing the statement of a new hypothesis, is an entire addition. Whoever is disposed to censure this latter part of the

essay, cannot extend his censure to the Academy for having countenanced it, inasmuch as the part in question never came under the consideration of that learned body; and as if it did, the author, according to the usage of all such institutions, is alone responsible for his opinions. In my preface I took particular care to impress on the reader that matter had been added and expunged, and that many of my peculiar opinions did not appear in the original Essay. In short I endeavoured by every possible means to present a true statement of what I had done, and to prevent the unthinking from attaching to the Academy any part of the animadversions that might afterwards be made upon my opinions. Hence I conceived it unnecessary and awkward to enter into any such details on the title. The same mode of proceeding is continually adopted in the publication of other works, such as sermons, &c. which are stated to have been preached before certain assemblies or personages; and it is even thought unnecessary by their authors to acknowledge that they have been improved by the addition of new matter, or by the deletion of what appeared objectionable.

As to your correspondent's objections (if such they can be called) to my process for obtaining pure silver, nothing is left me to reply. He places his own *suppositions* in opposition to my experiments. To a chemist it would have been easy to ascertain the fact, instead of asking an opinion: to a person conversant with the modern method of philosophical reasoning it would appear absurd to attempt the subversion of experiment by conjecture: and, independently of any other consideration, politeness did not permit my accuracy to be publicly called in question, when my opponent was unable to assign the smallest grounds of doubt.

In your commentary, which accompanies the above-mentioned observations on my mode of obtaining pure silver, you mention from your own experience that the common process of reducing luna cornea by means of pearl ashes is as good as any other if properly conducted, and that the loss in your trials never exceeded a few grains per cent. At the same time you acknowledge that if the heat be too great, or too long continued, there is risk of losing a part, or even all, of the silver.

It was on this latter account, and some others, that I grounded the necessity of the process which I proposed. It is very difficult for even a tolerably experienced chemist to hit off the exact heat sufficient for the reduction of the whole, and not too great to volatilize a part of the muriate. Beside this difficulty, I objected to the great trouble and complexity of the process; and the same objection applies to Gay Lussac's, and to all others hitherto proposed. In my method there is nothing that cannot be accomplished by the most inexperienced, without risk of failure, in a short time, with little trouble, and no loss of silver. I conceive, therefore, that you not only coincide with me in opinion, but that you have given considerable weight to one of my objections, by setting it in a stronger point of view than I had done, and by virtually admitting the necessity of

my proposal. Hence, I look upon your opinion in the case as a great accession to the interest of my process.

I have the honour to be, sir, with respect,

*Dublin, July* 24, 1816. M. Donovan.

## XIII. *Queries respecting Petrifactions.*

(To Dr. Thomson.)

SIR,

In various univalve shells that are imbedded in the argillaceous lime-stone or blue lyas, there is a material similar to the white lyas, occupying nearly the space that the fish must have occupied. And in every such case the shell is encrusted on its outer surface with yellow martial pyrites, resembling, externally, the back of a toad. How are these appearances to be accounted for? Is there any truth in the supposition that the calcareous matter is the fishy substance, which in the case of the cornu ammonis is crystallized? or has the blue lyas changed its character by the deposition of its pyrites? The white lyas has been first formed in the order of strata, and in this instance lies at not less than 60 feet below the other; but surely we cannot imagine the portions found in the shells to be boulders.

Your opinion, conveyed through the medium of the *Annals*, will be esteemed a favour conferred on,

Sir, your most obedient servant.

*July* 15, 1816. I. C.

The queries in the preceding letter are highly worthy of attention, and can only be answered by a careful examination of the phenomena. It would be presumptuous in me to venture any opinion on the subject. It is sufficient to draw the attention of the numerous well informed geologists, who are at present occupied in the examination of this country, to a phenomenon which is likely to lead to some important deductions.—T.

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# Article XV.

## *Scientific Books in hand, or in the Press.*

Mr. Accum has in the press A Practical Essay on Chemical Re-agents or Tests, illustrated by a Series of Experiments. The work will comprehend a summary view of the general nature of chemical tests, the effects which are produced by the action of these bodies, the particular uses to which they may be applied in the pursuits of chemical science, and the art of applying them successfully. A list of all those substances for which there exist any appropriate tests will be added; and a portable chemical chest, containing all the chemical tests and apparatus necessary for performing the experiments described in the treatise, may also be had with the work.

## Article XVI.

# METEOROLOGICAL TABLE.

| 1816. | Wind. | Barometer. Max. | Min. | Med. | Thermometer. Max. | Min. | Med. | Hygr. at 9 a. m. | Rain. | |
|---|---|---|---|---|---|---|---|---|---|---|
| 8th Mo. | | | | | | | | | | |
| Aug. 16 | Var. | 29·81 | 29·59 | 29·715 | 65 | 52 | 58·5 | | — | ☾ |
| 17 | N W | 29·98 | 29·84 | 29·910 | 61 | 49 | 55·0 | | ·21 | |
| 18 | N W | 30·15 | 29·98 | 30·065 | 62 | 51 | 56·5 | | — | |
| 19 | N W | 30·16 | 30·10 | 30·130 | 67 | 55 | 61·0 | | ·13 | |
| 20 | N | 30·18 | 30·10 | 30·140 | 64 | 42 | 53·0 | | — | |
| 21 | N | 30·18 | 30·14 | 30·160 | 66 | 50 | 58·0 | | — | |
| 22 | N W | 30·14 | 30·10 | 30·120 | 68 | 53 | 60·5 | | | |
| 23 | N W | 30·10 | 30·10 | 30·100 | 66 | 56 | 61·0 | | — | ● |
| 24 | N E | 30·18 | 30·10 | 30·140 | 70 | 45 | 57·5 | | — | |
| 25 | N E | 30·20 | 30·18 | 30·190 | 69 | 44 | 56·5 | | | |
| 26 | N W | 30·18 | 30·15 | 30·165 | 67 | 47 | 57·0 | | | |
| 27 | E | 30·19 | 30·15 | 30·170 | 66 | 44 | 55·0 | | | |
| 28 | E | 30·19 | 30·17 | 30·180 | 70 | 46 | 58·0 | | | |
| 29 | N W | 30·17 | 29·95 | 30·060 | 64 | 52 | 58·0 | | | ☽ |
| 30 | N W | 29·95 | 29·36 | 29·655 | 61 | 53 | 57·0 | | ·23 | |
| 31 | S E | 29·46 | 29·30 | 29·380 | 59 | 46 | 52·5 | | 1·09 | |
| 9th Mo. | | | | | | | | | | |
| Sept. 1 | | 29·57 | 29·22 | 29·395 | 49 | 40 | 44·5 | | ·92 | |
| 2 | N W | 29·67 | 29·57 | 29·620 | 55 | 30 | 42·5 | | | |
| 3 | S W | 29·67 | 29·33 | 29·500 | 56 | 43 | 49·5 | 66 | — | |
| 4 | N W | 29·49 | 29·31 | 29·400 | 57 | 37 | 47·0 | 73 | — | |
| 5 | W | 29·86 | 29·49 | 29·675 | 60 | 40 | 50·0 | 75 | ·18 | |
| 6 | S W | 29·86 | 29·79 | 29·825 | 61 | 50 | 55·5 | 62 | | ○ |
| 7 | W | 29·79 | 29·77 | 29·780 | 67 | 52 | 64·5 | 52 | | |
| 8 | S W | 29·77 | 29·65 | 29·710 | 65 | 47 | 56·0 | 77 | | |
| 9 | S W | 29·48 | 29·38 | 29·430 | 60 | 54 | 57·0 | 60 | ·55 | |
| 10 | S W | 29·80 | 29·77 | 29·785 | 64 | 47 | 55·5 | 49 | ·18 | |
| 11 | W | 29·93 | 29·80 | 29·865 | 65 | 42 | 53·5 | 70 | | |
| 12 | S W | 30·13 | 29·93 | 29·980 | 65 | 41 | 53·0 | 70 | | |
| 13 | | 30·13 | 29·99 | 30·060 | 65 | 55 | 60·0 | 53 | — | |
| | | 30·20 | 29·22 | 29·872 | 70 | 30 | 55·29 | | 3·49 | |

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.*—19. Clear morning. 23. A little rain, evening. 24. Cloudy morning: smart shower at noon: a considerable appearance for thunder, evening. 25. *Stratus:* fine sun-set. 26. Much dew, a. m.: cloudy. 28. Foggy morning: fair: *Stratus.* 29. Very foggy: fair. 30. Cloudy morning. 31. Wet morning: stormy night, with heavy rain.

*Ninth Month.*—2. Hoar frost this morning: there is said to have been thick ice formed in several exposed situations. 3. Rain, with much wind, in the night. 4. A hard shower of hail, followed by rain, about noon. 5. Wet morning: fair day after. 6. a. m. Cloudy: misty to the S.: overcast day. 7. Maximum of temp. at nine, a.m.: fair day. 8. Fair: wind rising at S.W. in the evening, and *Nimbi* about. 9. It began to rain about seven, a. m. and continued till three, p. m.: after which a very stormy night, the lower clouds moving much faster than the higher. 10. Fair day. 11. Much dew: fair: *Cirrocumulus* for two days past. 12. Much dew: fair. 13. Dew: misty, a. m.: a few drops at mid-day, after much cloud and wind: the sky at sun-set appeared to be clearing gradually.

## RESULTS.

Winds variable in the fore part, westerly in the latter.

| | | |
|---|---|---|
| Barometer: | Greatest height ..... | 30·20 inches |
| | Least ..... | 29·22 |
| | Mean of the period .... | 29·872 |
| Thermometer: | Greatest height ..... | 70° |
| | Least ..... | 30 |
| | Mean of the period .... | 55·29 |

Rain, 3·49 inches.

*** The observations of this period up to the 31st ult. are those of my friend John Gibson, at the Laboratory, Stratford: with the present month they recommence at Tottenham, as usual.

Tottenham, L. HOWARD.
*Ninth Month,* 24, 1816.

ANNALS

OF

PHILOSOPHY.

*NOVEMBER*, 1816.

## Article I.

*On Flame.* By George Oswald Sym, M.A.

If a single wire be held horizontally across the flame of a wax candle, the brightness of the upper part of the flame will be visibly impaired. If two, three, or four wires, be made use of, instead of one, the effect will be proportionably increased. If a piece of wire-gauze having 36 meshes in a square inch be employed, only a few feeble streaks of red flame, darkened by abundance of smoke, will be seen to rise above it; and through wire-gauze having 64 meshes, or any greater number, in an inch, no part of the flame will ascend.

Again: if two pieces of paper be stuck on the opposite sides of a piece of wire-gauze, and one of them be set on fire, the other will remain uninjured. The same experiment will succeed with two pieces of linen, of fine cotton stuff, or of any ordinary combustible. It will succeed, also, whether the gauze to which the pieces are attached be held horizontally, vertically, or obliquely.

Hence wire-gauze seems to be a complete barrier against the progress of combustion. But there are limits, we may be sure, to this singular property; and these will appear, upon discovering its cause.

Having, by means of wire-gauze, suppressed the upper half of the flame of a candle, hold the palm of your hand directly over it, and as near as you can, without being pained by the heat; and if you now withdraw the gauze, your hand will be severely scorched; or substitute a thermometer for the palm of your hand, and upon the removal of the gauze it will show a rise of temperature from 90° to

170°, or in some such proportion. Wire-gauze, therefore, intercepts heat, as well as flame.

Again: let a piece of wire-gauze, which is just close enough in its texture to intercept the flame of a candle, when cold, be heated to redness, and the flame will now pass through it; or lay a shred of cotton stuff on such a piece of wire-gauze, and then place it, like a gridiron, over a common fire, and the stuff will be protected from burning only till the wires have become red-hot. Wire-gauze, therefore, loses its power of intercepting flame as soon as, by having its temperature sufficiently raised, it is rendered incapable of intercepting heat.

Hence we are led, in these cases of more moderate combustion, to the same principle upon which Sir Humphry Davy has explained the efficacy of wire-gauze in intercepting explosion. The wires of which the gauze is formed, being good conductors of heat, abstract so much of what is evolved by the flame, that the remainder, which is suffered to pass through the meshes, is not sufficient to bring a combustible situated on the opposite side to so high a temperature as that at which it is inflamed. But whenever the wires are already so hot as to have, in a great measure, lost the power of abstracting heat; or whenever they are either so small, or so distant from one another, that there is not in the given space a quantity of conducting matter sufficient to carry off the required proportion of heat; enough will remain and pass through to kindle the combustible on the opposite side. Such appear to be the cause and the limits of that power which wire-gauze possesses of barring the progress of combustion.

By certain applications of this power which occurred to me in the course of some experiments on the properties of the safe-lamp, I have been enabled to gain an insight into the internal structure of flame.

1. When 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 upon 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. Besides, the wick could not be seen at all through intervening flame, unless through an extremely thin film of it: for flame is an opake substance, as any one may satisfy

himself by trying to read a book through the upper part of the flame of a candle. The only conclusion that remains, therefore, 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.

2. When a flame is truncated in the manner that has been described, a stream of volatilized wax ascends through the wire-gauze, in place of the upper part of the flame; for of the whole quantity of wax which is continually given off from the candle, only so much is now consumed below the gauze as is usually consumed in the same portion of the flame when not truncated. Now, though a small part of the remainder is condensed around the wires, and blackens them, by much the larger part remains volatile, rises through the meshes, and flies off unconsumed in the shape of a column of smoke. But, though the progress of the combustion in pursuit of this fugitive fuel is arrested by the grating of wire-work, it may still be set on fire by means of a lighted match. It then continues to burn, as it would have done if the gauze had never been introduced; and the flame which it affords is of a conical shape, and resembles the upper part of the entire flame of a candle. In this state of things, therefore, the gauze no longer appears to truncate the flame, but to bisect it. There is an interval, however, between the two segments: for the upper one does not rest upon the gauze, but is entirely insulated, flitting up and down, and becoming larger or smaller, according to the varying rapidity with which its fuel is evolved. In consequence of this disjunction of the two segments, an opportunity is afforded of inspecting the upper one from below, and of thus obtaining another peep into the interior of the flame.

Now the appearance here presented is perfectly conformable to that already described as belonging to the lower segment. There is still an obscure disk, surrounded by a luminous ring; and in fact the floating flame is perceived to have the structure of a cap, distended and supported by the ascending column of vapour. The film of flame of which this cap consists seems to be extremely thin at the lower edges, but to become gradually thicker till it terminates in a solid apex. It is always in motion, extending itself in various directions, according to the gentle impulses of the surrounding air; and sometimes it curls up its edges, so as to assume the shape of a bell, in which case its hollow structure is prettily enough displayed, inasmuch as it has a tendency to turn itself inside out.

3. If the upper segment of a bisected flame be truncated in its turn, an intermediate segment, or one which is neither connected with the wick below, nor terminated in an apex above, will be obtained. For this purpose let a piece of wire-gauze be bended double, so that the two parallel folds may be distant from each

other somewhat more than half an inch; truncate the flame of a candle with this doubled gauze; and set fire to the ascending stream of vapour, both between and above the two folds. If the vapour be sufficiently abundant, the appearance will be that of a trisected flame; but it seldom is so abundant as to maintain all the three segments permanently. However, neglecting the upper segment, and allowing a small portion of the vapour to escape unconsumed, there will be no difficulty in obtaining a permanent flame between the two folds; and this will have all the appearance of a middle cut of the entire flame.

Now this intermediate segment will be clearly perceived to have the structure of a short tube, through which, as through a conduit, the column of residuary vapour ascends. The tube, indeed, does not always embrace the whole circumference of the column; for sometimes it splits and opens lengthwise, so as to uncover its inside, and show that it is no more luminous, or of the nature of flame, than the common air with which it is now brought into contact. In short, no person who observes the appearance attentively can doubt that the real living flame is a superficial film.

4. Let us now put these several observations together, or reconstitute the whole from the parts into which we have divided it. To the cap of flame, which we obtained below, let us adapt the tube of flame which we obtained in the middle, and let us cover this tube with the cap of flame which we obtained at the top. It will then appear that the entire flame is a hollow substance, that the actual combustion is confined to the surface, and that the internal part is filled with volatilized wax. In short, the flame of a candle is an elliptical bubble.

5. I am persuaded that any person who will take the trouble to repeat these simple experiments will be satisfied that there can be no illusion in the appearances which have been described. But as all of them are derived from the same process of dissection, the satisfaction may be more complete if the mode of examination can be varied. For this reason I shall mention three proofs of the hollowness of flame, which are of a different kind.

As long as the snuff of a candle is confined entirely within the flame, it remains black throughout. But let it be allowed to run up to the apex, or let it be made to curl out to the side, and that part of it which comes into contact with the surface of the flame will immediately become red.

Secondly: When an iron wire is held horizontally across the flame of a candle, it becomes red-hot at the two points at which it crosses the surface. But that part of it which is in the inside may, by truncating the flame, be generally perceived to remain black.

Lastly: When a piece of wire-gauze, after having been employed to truncate a flame, is withdrawn, there is always found a circular black spot upon it, the wires being incrusted with soot and wax. But if this spot be held just over the apex, or any where in contact

with the surface of the flame, it will be dissipated by the superior heat, and the wires will remain quite clean.

These appearances are all accounted for by supposing that the internal part of the flame is comparatively cool; the actual combustion being diffused over the surface, and concentrated at the apex.

6. There seems, then, to be little doubt that the fact is so; but perhaps we may advance a step further, and assert that, from the nature of the thing, it could not have been otherwise; for the flame of a wax candle consists of the light and heat which are given out in the process of combination between the volatilized wax and the oxygen of the atmosphere. Now the wax rises, at first, in a dense column, and does not for some time diffuse itself among the surrounding air. This may be observed in the case of a truncated flame. Hence if a combination is to take place between the wax and the oxygen, it must take place at the surface of this column, where alone they come into contact with each other. The actual combustion, therefore, must necessarily be superficial, and the flame hollow.

Wax is given off from the whole length of the snuff, or blackened part of the wick. It may be seen boiling out from the very summit of it. Hence the quantity evolved, or the thickness of the column, will reach its maximum just around the top of the wick; and accordingly it is precisely there that the circumference of the flame is the greatest. Above this point, the supply of fuel receives no increase, while the consumption of it is progressive; consequently the combustion gradually encroaches on the thickness of the column till at length it closes over it. Hence the tapering and acuminated shape of the flame; and hence the superior intensity of heat and light at the apex.

7. If this be the true explanation of the structure of the flame of a candle, a similar structure ought to be found in almost every variety of permanent flame with which we are acquainted; for the explanation is applicable to all such flames as are produced by a gradual combination between the oxygen of the air and an ascending stream of inflammable vapour or gas. I have made the experiment in a few of the most ordinary cases, so as to satisfy myself of the generality of the fact.

If a piece of wire-gauze be held across the flame of an Argand lamp, the flame will be truncated; there will be seen two concentric circles of light, instead of one broad ring; the appearances of bisection and trisection may be exhibited; and the segments will be perceived to be hollow, just as in the flame of a candle.

If a large piece of wire-gauze be held over a common fire, either of wood or coal, it will truncate all the flames, and show that they are all hollow. In this case lines of light will be seen running out and returning in various directions, and forming many salient and re-entering angles; but all of them will constitute one outline, and enclose a continuous space, though as capriciously shaped as the kernel of a walnut. Hence all the complicated flames of a common

fire appear to inosculate into one another, and to contain one common nucleus of inflammable gas.

The flame from carbureted hydrogen, or the ordinary gas light, may be truncated by the same means, and will exhibit the same appearances of hollowness as the flame of a candle, but in a neater and more pleasing manner. I have little doubt that, by forcing out the gas with sufficient rapidity, not only three segments of flame, but a greater number, might be permanently supported; but this I have not tried.

It is needless to multiply examples in order to prove a similarity of structure where no cause of difference can be supposed to exist. The general conclusion is, that wherever a stream of inflammable vapour or gas rises gradually into contact with the surrounding air, and combines with its oxygen so as to produce flame, that flame is and must be hollow.

8. But there are many instances of inflammation in which the combustible and supporter do not come into contact thus gradually and in distinct volumes. A gaseous mixture of oxygen with hydrogen, or with any of the kindred inflammable airs; or a solid compound of charcoal with a nitrate or an oxymuriate; contains within itself the elements of combustion in such a state of intermixture that only an elevation of temperature is wanted to enable them to burst into flame, independently of the access of atmospheric air. In such cases, therefore, the flame ought not to be hollow, but of uniform substance throughout.

In order to illustrate this, let a glass phial be filled with pure hydrogen gas. Then turn up the mouth of the phial; so that the hydrogen, from its superior lightness, may rise into contact with the air, and fire it with a lighted taper in its ascent. The result will be a permanent flame at the mouth of the phial; which flame may probably be proved, by the methods already described, to have the structure of a hollow bubble.

Fill the same phial with the same gas a second time; but apply the taper to the mouth of it, without turning it up. Then, as the lighter gas cannot descend into the heavier, the only surface of contact between the hydrogen and the oxygen of the air will be just across the mouth of the phial. Accordingly a small flame will be seen to commence there, and to ascend along the phial, in proportion as the inflammable air is consumed. Here the usual order is reversed, as it is now the supporter which rises to meet the combustible; but still their contact is only superficial; and, therefore, the flame, though not hollow, but flat, is still only a thin film.

But, instead of filling the phial with pure hydrogen, let it next be filled with an explosive mixture of oxygen and hydrogen; and if a taper be now introduced into it, there will be no gradual and local combustion, but a sudden and thorough explosion. This will happen indifferently whether the mouth of the phial be up or down; and it would happen equally, although it were hermetically sealed. Here, then, both the reason of the thing, and our perception, as far as it

goes, lead us to conclude that the flame has not been superficial, but solid.

Hence as an ordinary flame is a permanent hollow combustion, we may define an explosion to be a momentary solid combustion; and this difference in their structure will account for the difference of their intensity.

9. If the permanency of a hollow flame could be combined with the intensity of a solid one; or, in other words, if a lasting explosion could be produced; a powerful agent would be obtained for the chemist and for the artist. Now I know not whether this has been already done; but it seems to be quite practicable.

Into Newman's blow-pipe let there be compressed a large quantity of the most explosive mixture of oxygen and hydrogen, and then let the stop-cock be turned, and the gases fired as they rush out. The smallness of the bore will prevent the inflammation from extending up the tube; and the rapid and constant emission of the gases will prevent it from being extinguished; so that the result ought to be a permanent explosion.

I suspect that this, or something of the same kind, is the mode of producing that heat (the most intense hitherto devised) by which barytes and strontites are said, in a late newspaper, to have been decomposed without the aid of compound affinities; for if the heat of a lamp be prodigiously increased, when the superficial combustion of the volatilized oil is merely quickened, by supplying oxygen gas more rapidly, and in a more condensed state, than it can be obtained from the atmosphere; how great must be the intensity when a more combustible substance than oil is intimately mixed and forcibly compressed with pure oxygen, and is thus made to burn solidly instead of superficially? There ought to be no more comparison than between a bucket of water and a bubble.

On this part of the subject, however, I have forborne to make experiments, lest I should intrude upon the path of another.

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## Article II.

*Objections to the Hypothesis of Mr. Campbell on the Upright Growth of Vegetables.* By the Rev. Patrick Keith, F.L.S.

(To Dr. Thomson.)

SIR,

In an article on the upright growth of vegetables in one of the late numbers of your *Annals*, the author has thought proper to state and to denounce, in very peremptory terms, a suggestion, or conjecture of mine, on a subject involved in the above title. This I should not have thought it necessary to take any notice of, if it had not been that it seems to me calculated to lead the reader into a

mistake with regard to the actual scope of my hypothesis; for, although the note of reprobation to which I allude is expressed with sufficient accuracy, yet as it evidently exhibits my hypothesis in the light of occupying a position opposite to that of the author's, it may possibly be thought by the reader that they are identical in their extent, and that I ascribe the continued and upright growth of the plant to the principle of *instinct* also, as well as the direction assumed by the radicle and plumelet respectively in the developement of the seminal germ. Now the hypothesis which I have advanced has nothing to do with the upright growth of the plant beyond that of introducing the plumelet into the open air—the medium fitted to its future developement—which may be creeping, or climbing, or winding, or upright, according to the natural and peculiar character of the species, and may depend entirely upon other causes.

But if my hypothesis, even with my own explanation of it, should still remain inadmissible, it will not follow that the hypothesis either of Mr. Campbell on the upright growth of the stem, or of Mr. Knight on the downward growth of the root, is, therefore, to be admitted. Mr. Campbell does not, indeed, say much on the descent of the radicle; but from what he does say, it is evident he adopts the theory of Mr. Knight; namely, that of the principle of direct gravitation as founded on the experiment of the revolving wheels. In my paper on this subject published in the Linnæan Transactions, I thought I had shown that there are some strong, if not insurmountable, objections to the adoption of this theory, although I did not enter very minutely into the detail of them. But as Mr. Campbell has either overlooked them, or thought them of no value, I will now take the liberty of briefly recapitulating them, and of adding some that are new.

With regard, then, to the boasted experiment of the revolving wheels, I must say that it has never appeared to me to be of any great value; because the same result might have happened even upon the supposition that the descent of the radicle is effected, not by gravitation, but by some other cause; inasmuch as the agency of that cause might have been affected and counteracted by the experiment as completely as the agency of gravitation itself. Suppose I let fall a ball at the height of six feet from the ground, with a view to illustrate the law of gravitation; I expect it is to descend in a perpendicular line; but some one hits it in its passage, and forces it to descend obliquely. Suppose I then say, this ball shall still descend in a perpendicular line, for I will guide it with my hand; I take the ball in my hand, and am letting it down as nearly in a perpendicular line as I can, but the same wag lays hold of my hand, and forces it down in an oblique line still. The result is the same in both cases with regard to the deviation of the ball; but the causes that have been overcome are very different. In the former case it was the power of gravitation; in the latter, it was the muscular force of my arm. Mr. Knight is not, therefore, entitled to conclude that gravitation is the cause of the radicle's descent, unless he

can show that his experiment could not have counteracted any other cause, or that there could not possibly have been any other cause in the way to be counteracted by it. If this last is assumed to be the fact, then I will beg leave to put the following questions:—

If gravitation is the sole cause that gives direction to the radicle or root, why does the germinating radicle of the misseltoe bend itself either downwards in conjunction with the agency of gravitation, or upwards in opposition to the agency of gravitation, according to the circumstances in which accident may have placed it, till it reaches the bark of the tree upon which it is to feed? Will it be said that this is the effect of accidental attraction, like that of a large mountain making the plumb-line to deviate from the perpendicular? Then it ought, for the same reason, to attract the plumelet also, which it has never been known to do. Further, if gravitation is the sole cause that gives direction to the radicle or root, how is it that so few roots are in effect perpendicular, even where there is no perceptible obstacle to mar their descent, and so many, on the contrary, horizontal, as in the case of *pteris aquilina*, or common brakes; and, I may add, even ascending; that is, where the surface of the soil that surrounds them is ascending, as in the case of trees planted by the sides of banks or mounds of earth? If I am now told that the radicle does in fact almost universally descend, and that the *rationale* of its descent is rendered evident upon the principle of gravitation from its peculiar mode of elongation; then I reply that, for the very same reason, the root of the vegetating plant ought also universally to descend, as they both elongate in the same way, and both receive additions in length at the extremity only. Also, if gravitation is the sole cause that gives direction to the radicle or root, why did my experiment of July 24, 1812, as related in my paper already alluded to, and instituted with a view to put to the test the hypothesis of Dr. Darwin, give the result that is there detailed? That is, why did the radicle both of the bean and grain of wheat, after having elongated by descent for about half an inch, till they had extended beyond the lower extremity of the tube, turn themselves suddenly from the perpendicular direction, and creep in a horizontal direction along the under surface of the earth, with which the tube, open at both extremities, was filled, with the points even ascending?

If these facts do not furnish sufficient evidence of the existence of an energy exerted by the vegetating plant that is capable of giving direction to the root, and of counteracting the agency of gravitation when necessary, then I am ready to confess that I do totally misapprehend the subject. But if, on the contrary, they furnish the evidence for which I contend, then it will follow undeniably that Mr. Knight's conclusion is a manifest *petitio principii*, and that his hypothesis is without foundation. Perhaps it may be said that these are only a few exceptions to a general rule. But why should there be any exceptions to it at all? Who ever heard of any real exception to the law of gravitation as applicable to the solar system? And if

these are exceptions to the law of gravitation, as applicable to vegetables, let Mr. Knight or Mr. Campbell show how and why they are so.

The next topic to be considered is that of the upright growth of the plumelet and stem, which I had erroneously supposed Mr. Knight did not account for—a supposition that arose merely from a lapse of memory, and not from any wish to exhibit an unfair representation of Mr. Knight's hypothesis. But the work which I had thus left imperfect, Mr. Campbell has completed, by exhibiting not only an accurate view of Mr. Knight's hypothesis, but also a complete refutation of it. It must be observed, however, that the refutation does not regard the primary principle of the hypothesis, which Mr. Campbell most cordially adopts, but merely the mode by which that principle acts. According to Mr. Knight, it is the agency of direct gravitation, causing the sap to accumulate on the under side of a deflected germen, in consequence of which the under side elongates, and the point turns up, and the plant, after a series of corrections in bending from side to side, acquires its upright growth. According to Mr. Campbell, it is the agency of resisted gravitation affecting the tender shoot, or, perhaps, the whole plant. This hypothesis he grounds on some facts, the result of minute observation, on the young shoots of a spruce fir, beech, and thorn, and on the germination and growth of a garden bean; from which he is satisfied that the shoots in the process of straightening exhibited the same phenomena which would naturally have resulted from the influence of gravitation, whether direct or resisted—the shoots acquiring their verticality, "not by any recurvative at the top, but by a general and progressive movement of the whole," in direct opposition to the series of corrections that seems to be essential to Mr. Knight's hypothesis, but which is altogether incompatible with the principle of gravitation.

Now on this statement I will beg leave to offer a few remarks. I do not mean to say that Mr. Campbell's observations were not minute, or that he has not faithfully and accurately stated facts as they occurred to him; but I must say that his observations have been rather too few, and the number of species examined rather too small, to serve as the groundwork of an hypothesis that is to supersede all other hypotheses on the subject. For what do they amount to after all? Three or four observations, in the course of two months, on three or four plants; whereas there should have been as many observations in the course of every day. And if Mr. Campbell did actually make no more observations than are stated in his paper, then he has had but a very imperfect view of the process by which the verticality of the plant is effected, and will have reason to think himself particularly fortunate if he has not, in consequence, been betrayed into one conclusion or other inconsistent with fact.

Such are the remarks that I have thought it proper to premise; and to show that they have not been thrown out at random, I will

now take the liberty of stating a few counter observations, that the reader may have an oportunity of contrasting them with those of Mr. Campbell, as represented in the plate that accompanies his paper.

In the month of July, 1812, I observed for several successive mornings that the top shoot of a spruce fir, growing in the front of my study, was uniformly bent towards the east. The shoot was at the time at least a foot and a half in length, and the origin of the bend about six or eight inches from the summit, which was much depressed. It occurred to me that the bending of the shoot was perhaps the effect of what is usually termed *nutation.* If so, it was to be expected that it would look towards the west in the evening. An evening observation was accordingly resolved on; but being omitted by some accident on the evening immediately succeeding the resolution, it became afterwards unnecessary; for on the following morning the shoot, instead of being bent towards the east, as before, to meet the rising sun, was found to be bent towards the west. This satisfied me that the phenomenon was not to be ascribed to nutation; and as I had no further object in view but the ascertaining of that fact, my intended observations were consequently stopped. But I recollect enough, merely from the casual observations that unavoidably occurred from occasionally looking out at the window, to be able to say, without the least fear of being contradicted, that the direction of the bend was changed again and again, and in a variety of different ways, before the shoot assumed its ultimate and vertical position.

On the 13th of August last (the day on which the number of your *Annals* containing Mr. Campbell's paper first reached me) I began to look out for some shoots whose growth was yet vigorous, that I might have an opportunity of making a few observations before the season was past. At seven o'clock in the evening the top shoot of a spruce fir of about 15 inches in length was bent by its upper half towards the south-west, so as to form an angle of about 75° with the horizon.

On the 14th, at seven o'clock, A.M. the weather being damp and cloudy, the shoot was quite erect, except that the lower part was somewhat zigzag in its ascent.

On the 15th the shoot was as before, confirmed, as it appeared, in its erect position, and no longer affording any scope for observation, the season of its growth being past. In short, it never more condescended to bend its head.

On the 16th I found that some shoots of a plum-tree, known in Kent by the name of the muscle plum, were still in vigorous and luxuriant growth, the tree being planted by a wall with a north-east aspect, but not nailed. At four o'clock, P.M. immediately after a most tremendous thunder-storm, the day having been altogether showery and cloudy, a shoot which measured about 20 inches in length was bent almost horizontally for about three inches at the

top. At seven o'clock the bent part had recovered itself considerably, and stood at an angle of 45°.

On the 17th, at eight o'clock, A.M. the shoot was erect, and continued so all day, though there was much rain.

On the 18th, at eight o'clock, A.M. the weather being very cloudy, the shoot was bent for about three inches from the summit, at an angle of 75°. At four o'clock, P. M. it had begun to recover its upright position, the weather being still cloudy, with some rain. At seven o'clock it was vertical.

On the 19th, at seven o'clock, A.M. the shoot was erect, and the sun shining. At nine o'clock it was bent towards the sun, so as to form an angle of 75°. At one o'clock, P.M. it was again vertical, the sun being obscured with clouds. At two o'clock it had passed the vertical line, and formed an angle of about 75° in an opposite or north-west direction. At four o'clock it had again bent back to the south-east, at about an angle of 80°, the weather being fine, with sunshine, but the tree in the shade.

On the 21st, at eleven o'clock, A.M. the weather being fine, the shoot was bent towards the sun at an angle of 75°. At five o'clock, P.M. it was erect, though in the shade. At six o'clock it had passed the vertical line, and was bent along with two others that I had now begun to observe in the direction of the setting sun, which does not actually reach the wall, but throws in the greatest glare of light from the north-west.

On the 22d, at five o'clock, A.M. the shoots were bent towards the east, or rising sun, though obscured by clouds. At nine o'clock they were erect. At ten o'clock one of them had past the vertical line, and was bent to the north-west. At one o'clock, P.M. they were all bent to the east at an angle of 75°. At two o'clock they were depressed to an angle of 45°. At six o'clock they had changed their direction, and were bent to the north-west.

On the 23d, at ten o'clock, A. M. two of the shoots were bent towards the sun, and the other towards the north. At five o'clock, P. M. they were all nearly erect.

On the 25th, at ten o'clock, A. M. the shoots were all bent to the sun, and the weather fine.

On the 28th, at nine o'clock, A.M. one of the shoots was bent at an angle of 45°, and the others were nearly erect.

On the 30th, at ten o'clock, A. M. the shoots were bent a little towards the sun. At five o'clock, P.M. one of them was erect, and the others bent at an angle of 75°.

On the 1st of September, at eight o'clock, A.M. in a tremendous storm of wind and rain, that had raged all night, the most pliant shoot was much bent down, the origin of the bend being at least six inches from the summit. At six o'clock, P.M. it had begun to resume an erect position by the lower half, though in a sort of zigzag, or rather serpentine line.

On the 2d, at eleven o'clock, A.M. the weather was fine, and

the lower part of what was bent on the 1st stood quite erect, though still in the serpentine line, which appears to be the last stage of the process of the erection of the shoot. The summit was slightly bent for about two inches, and the shoot was found to have grown at least four inches since the commencement of my observations.

On the 4th the waving line was a little higher, and the summit bent only by an inch.

On the 6th the waving line was quite obliterated, and the shoot vertical. It became pendant no more.

If we look at the scope of the above observations, we shall find it to be so far from coinciding with that of Mr. Campbell, that it scarcely comes into contact with it at all; and seems rather to countenance the hypothesis of Mr. Knight, by demonstrating that a series of corrections does actually take place in the process of the elevation of the pendant shoot, and that the conclusion of Mr. Campbell with regard to that process is palpably contradicted by facts. Perhaps I might here legitimately put a stop to the discussion altogether, and contend that the question is decided against Mr. Campbell's hypothesis, inasmuch as he admits that a series of corrections is incompatible with the principle of gravitation. But as this would be to dismiss the subject without once looking at the superstructure which Mr. Campbell has erected on a foundation deemed impregnable, and to sink the main body of the building without even inquiring of what materials it is composed, we will not take our leave of it without some further inspection.

Now the elevation of the pendant shoot upon the principle of resisted gravitation, and in the manner supposed by Mr. Campbell, that is, by a general and progressive movement of the whole, may be effected, as Mr. Campbell thinks, by means of the agency of a "buoyant principle bearing up the plant in which it is enclosed, as the hydrogen gas bears and presses upwards the body of a balloon; or by drawing it upwards in consequence of its adhesion, as the balloon rendered buoyant by the gas carries up the car that is attached to it." Such is the possibility. And the next inquiry is, are there any such gases to be found in the vegetating plant? Hydrogen, which is of all known gases the most buoyant, and consequently the best fitted to Mr. Campbell's purpose, is known to exist in plants, at least in a state of combination; and Mr. Campbell has been at some pains to ascertain whether it does not also exist in them in a free state. But of this he can find no satisfactory evidence; and is obliged, as a last resort, to have recourse to the agency of evaporation.

This theory, it may be observed, is not altogether a new one; for it is suggested and sanctioned by no less an authority than that of Dr. Colin Milne in his Botanical Dictionary, which was originally published at least 40 years ago; though I believe Mr. Campbell to be the first who has undertaken to explain it, and to establish its sufficiency. Mr. Campbell sets out by stating the fact of the great quantity of water that is daily thrown off in evaporation

by the vegetating plant, which he ascribes chiefly to the agency of the caloric that is disengaged during the formation of carbonic acid in the process of vegetation. The caloric that is thus disengaged seizes the water, and converts it into vapour; and the vapour in flying off communicates its buoyancy to the plant, and gives it an upright position.

That a great quantity of water is daily thrown off in evaporation by the vegetating plant, we are ready to admit, whether it be chiefly in the way supposed by Mr. Campbell or not. But we believe that the phenomena of vegetation do not countenance the supposition of its producing the effect which Mr. Campbell ascribes to it; for

1. It has been ascertained that the principal organs of perspiration are the leaves, and that a plant deprived of its leaves perspires but little. Hence we may infer that there can be but little of evaporation during the process of the germination of the seed, when the leaf is yet unfolded, and the plumelet buried under the surface of the earth. Particularly the process of evaporation will be impeded under such circumstances, if the plumelet is besides invested with some other organ which it has not yet been able to throw off; as in the case of the germination of the seed of the beech and fir-tree; the former of which has the plumelet invested with the cotyledons and integuments; and the latter, with the cotyledons, albumen, and integuments, till the stem is considerably elevated above the surface. They come up quite erect, and with a load upon their tops; and though their leaves are yet unfolded, and unfavourably circumstanced in other respects, Mr. Campbell has no other means of giving verticality to the incipient stem, but that of evaporation.

As this is the only point in which my hypothesis comes at all into contact with Mr. Campbell's, I will beg leave to state a fact or two more, before I take my leave of the subject. On Sept. 2, I filled a small tub with fine garden mould, and planted a few horse-beans in it, at the depth of two, four, six, and eight inches, and raked some in on the top. On the 9th, two of the most superficial beans had protruded their radicles to the extent of about an inch downwards, but the plumelet had not yet disengaged itself from the cotyledons. On the 12th I took up all the beans. The radicles of the uppermost measured from an inch to $1\frac{1}{2}$ inch in length; the enlarged and expanded plumelets being nearly extricated, and of a green colour, and the stem just appearing. The radicles of such as were at the depth of two inches measured from three to four inches in length; the plumelets, which were elevated on stems that brought them quite to the surface, though considerably deflected, beginning to assume a tinge of green. The radicles of such as were planted at six and eight inches deep measured about three inches in length, the plumelets, which were still yellow and unexpanded, being elevated on stems of from two to three inches in length, and bent down from the summit of the stems in the form of hooks. These stems would, no doubt, have elevated the unexpanded plumelets to the surface of

the earth, if they had not been thus prematurely taken up. And what power is it that enabled them to force their way to the point they had gained? Mr. Campbell will, no doubt, say it is the power of evaporation. But circumstances are certainly much against the supposition; for evaporation cannot surely act with such force at the depth of six or eight inches, as upon the surface; so that if it should be able to elevate a pendant shoot or plumelet in the midst of a body of air where the resistance from the superincumbent mass is as nothing, it may not be able to elevate the same parts through the midst of a body of earth where the resistance from the superincumbent mass must certainly amount to something, particularly at the depth of six or eight inches. It is true that the formation of carbonic acid gas must have been going on from the very commencement of the process of germination, and consequently a disengagement of heat. But is there none of that heat wanted to give temperature to the moisture that is continually absorbed by the radicle, and to the parts that are continually added to the plant? And will not the heat that is thus formed diffuse itself by radiation, as in other cases, rather than fly up vertically to the top of the stem? And, finally, if evaporation is, after all, the true and efficient cause of the elevation of the stems in question, to what cause, whether chemical or mechanical, am I to attribute the election of the plantlet in developing, first a stem, and then the plumelet, in the case of the beans that were planted the deepest; and in developing, first the plumelet, and then a stem, in the case of the beans that were the most superficial? Mr. Campbell himself will not say that this was the effect of evaporation.

2. In the case of plants whose leaves are completely evolved, the following facts have been established by Millar, Hales, or Guettard. In a hot and dry day plants transpire the most, and in a wet and damp day they transpire the least. In a hot and dry night they transpire a very little; but in a wet or dewy night they do not transpire at all. Further, if transpiration is too abundant through excess of heat or drought, the plant suffers, and the leaves and shoots droop, but revive again during the night. Also if my observations on the shoots of the plum-tree are taken into the account, they will be found to tally precisely with the above facts. Now if evaporation is the cause of the upright growth of vegetables, and elevation of the pendant shoot, is it not very strange that the plant should hang its head when the operation of the uplifting cause is the greatest, and rear it again when it is the least?

3. The annual shoots of the weeping willow and ash do not so much as elevate even their summits in obedience to the law of gravitation, though there exists no apparent reason why they should not yield to that law as well as others. The hop, honeysuckle, and vine, do generally elevate their summits, in obedience, as it may be alleged, to the law of gravitation; and yet the stem itself can never be made to stand erect without the assistance of a prop. The peduncle of the crown imperial is pendulous when the plant is in

flower, and evaporation, as we may suppose, at its *maximum*; and yet the same peduncle becomes erect when the plant is in fruit, and evaporation approaching to its *minimum*. If the corolla is furnished, like the leaves, with organs of perspiration, it ought to elevate the peduncle; and if not, then I would ask what it is that elevates the corolla of the delicate meadow saffron, on its slender tube, even though it originates at the depth of six or eight inches below the surface of the soil? I would beg leave to ask, also, whether the agency of evaporation will account for the following fact? The top shoot of a spruce fir, which was at its height on the 13th of August, being a foot in length, and perfectly straight, though not vertical, was on the 10th of Sept. found to form, not a straight line, as before, but a waving line. For about two inches at the bottom it retained its original direction, to the south-west; for about four inches from that it took a turn to the north-west; and for the remainder, it was again inclined, like the base, to the south-west. It was an effort of the living principle of the plant to give verticality to the shoot. But will any one say that it was the effect of evaporation? The position cannot possibly be maintained; for if so, then you may just as well pretend to account for any other phenomenon of vegetable life, by ascribing it to the agency of evaporation.

4. The branches of plants are not, in fact, vertical, though the stem may be so; but are extended in almost all directions from the upright, which means almost perpendicular upwards, to the reflected, which means perpendicular downwards. They seem to follow the direction of the angle at which the bud was originally protruded from the stem, rather than to assume a vertical position. The same thing may be said of the leaves also, the peculiar organs of evaporation, which, if they should not always be able to give verticality to the plant, might at least be expected to take it to themselves.

But if the phenomena of vegetation were even more favourable to the hypothesis of Mr. Campbell than they are thus found to be, it does not appear to me that the agency of evaporation as a cause is adequate to the production of the effect ascribed to it. If we make a calculation according to the quantity evaporated from the surface of a whole plant in a day, as ascertained by Hales, then we can easily determine the quantity evaporated from any given portion of surface in any given time. The surface of the leaves and stem that was the subject of Hales's experiment was 5616 square inches; and the mean rate of transpiration $1\frac{1}{3}$ lb. in a day, which we will take at 16 hours of sun light. Hence each square inch will give

| | Gr. |
|---|---|
| In an hour, about ..... | $\frac{1}{10}$ |
| In a minute ..... | $\frac{1}{600}$ |
| In a second ..... | $\frac{1}{36000}$ |

Such is the very minute quantity that comes to the share of a square inch. Leaves are, indeed, generally thin; and it may be

said that a very small quantity of vapour is sufficient to raise them. But as the fact is that they are very seldom vertical, either the quantity which they do evaporate is too little to elevate them, or evaporation does not tend to elevate the plant at all.

Or we may make our calculation upon a different principle. Suppose the bent part of a large shoot of a spruce fir to weigh $\frac{1}{2}$ oz. Troy (which is no extravagant supposition); and if we suppose that it evaporates in the course of 16 hours of sun light, a quantity of water equal to its own weight (which is allowing quite enough to the shoot of an evergreen), then we may ascertain what quantity is evaporated by the whole in any given time, which will be as follows:—

| | Gr. |
|---|---|
| In an hour ..... | 15 |
| In a minute ..... | $\frac{1}{2}$ |
| In a second ..... | $\frac{1}{240}$ |

Now it is impossible that $\frac{1}{240}$ of a grain of water in vapour, especially at the temperature of the plant, should ever be able to raise a weight equal to $\frac{1}{2}$ oz., since it is known that it would require upwards of a cubic foot of hydrogen gas to do so, and consequently much more of vapour. But Mr. Campbell urges the facility with which it may elevate the shoot from the fact of its acting with the advantage of a lever power. This advantage will readily be granted, and yet much will not have been gained; because the power is for the greater part disadvantageously applied, from its being diffused over the whole surface of the weight to be raised. Mr. Campbell says, indeed, that its efficacy does not depend so much upon any present or single effort as upon its being "incessant in its operation." Now it is rather singular that a writer so well skilled in the principles of mechanics as Mr. Campbell seems to be, should have committed this slip with regard to the agency of a lever power, as it must be obvious to any one that will give it a moment's reflection that it is instant force, and not incessant operation, that is to produce the effect in question; since the lever does not, like the screw or wedge, lessen at each effort the amount of the resistance to be overcome. The power applied must be capable of raising and sustaining the whole of the given weight without stop or intermission (unless you are furnished with props to prop it as you go on), till it is brought to the point desired; otherwise there is no progress made. If there is a weight of 100 lb. to be raised, and the application of the force of my arm is insufficient to raise it at one effort, it will not become sufficient merely by a number of reiterated trials. Or if an aëronaut has constructed a balloon which requires 5000 cubic feet of hydrogen gas to elevate the apparatus and himself; and if he cannot produce more than 4000 feet, he will never be able to gratify the curiosity of the spectators with the view of the ascent of his balloon merely by the incessant operation of the 4000 feet, however long he may detain them. For the same reason, if the

force of evaporation will not elevate any given shoot in the instant in which it has reached its *maximum*, it will not ultimately elevate it, even by the incessant operation of a whole season. Hence it evidently follows that the agency of evaporation is insufficient to account for the elevation of the pendant shoot, and upright growth of plants; and that there is, at least, one reason more than Mr. Campbell had imagined, to despair of its ever being accounted for upon principles that are merely either chemical or mechanical.

If these arguments should appear to you to have any weight, the insertion of them in your *Annals* will much oblige,

Sir, your most obedient, humble servant,

*Bethersden, Sept.* 16, 1816. P. Keith.

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## Article III.

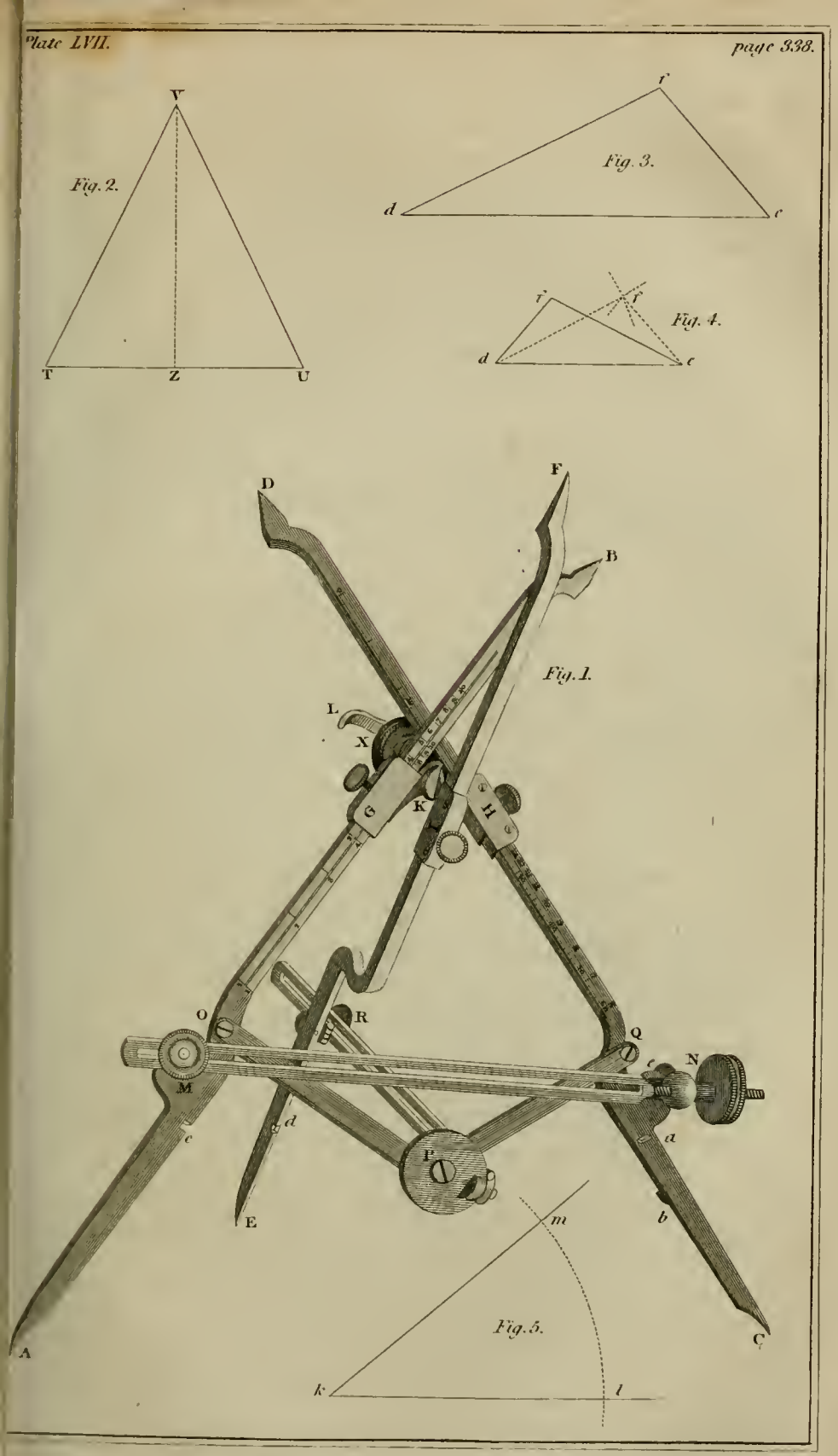
### *Description and Use of the Triangular Proportional Compasses.* By Mr. I. Narrien, Optician, St. James's-street, London.

(With a Plate.)

This instrument consists of three legs of brass or other metal, about nine inches long, all of which are moveable about a common centre, which is itself capable of being moved through half the length of the compasses, for the purpose of enabling the three points at one end to be opened to an extent which shall be in any required proportion to that at which the three points at the other end are opened, just as is the case with the opposite points of the common proportional compasses.

Fig. 1, Plate LVII., is a perspective representation of the instrument when extended; A B, C D, E F, are the three legs (formed as in the figure), which fit into and slide through the sockets, G, H, I. These sockets have tongue pieces proceeding from them, which are perforated in order to receive the axes which connect them together, and which serve as the centres of motion of the legs. The tongues belonging to the sockets G and H are made to lie over each other, and a solid cylindrical axis about a quarter of an inch in diameter, the end of which appears at K, is made to pass through both holes. Upon this axis the legs A B, C D, turn round in planes parallel to each other, and may be opened to any extent required. Across the end, K, of this cylinder a notch is sawn down about a quarter of an inch in the direction of the axis of the cylinder. Into this notch the tongue of the socket I is inserted, as it appears in the figure, and is held there by a steel axis passing through the cylinder, and through the perforation in the tongue, at right angles to the axis of the cylinder.

By these means the leg E F has two motions; one of which, in




Mr. Nairne's Triangular proportional Compass.

Engraved for Dr Thomson's Annals for Baldwin, Cradock & Joy, Paternoster Row, Novr 1 1816

NATURAL HISTORY

a plane always perpendicular to that of the other legs, is obtained by turning it round upon the steel axis which goes through the cylinder, and the other by turning the cylinder itself round upon its own axis. To perform this latter motion conveniently, a lever, part of which is seen at L, is fixed at the other end of the cylinder.

When the legs of the instrument are all shut together, the points A E C, and the points D F B, are made to coincide with each other respectively, the common centre of motion of the three legs falls in the line joining the points at the opposite ends of the instrument, and the steel axis passing through the cylinder is precisely at right angles to this line.

Small pins of steel are placed at *a* and *b*, and corresponding notches are made at *c* and *d*; so that when the three legs of the compasses are brought together, the pins, *a*, *b*, fall into the notches, *c*, *d*, and keep the whole instrument steady, that the legs may not slip while the centre piece, K, is moved wherever it may be required.

A perforated steel bar, M N, with an adjusting screw at N, is fixed to the two legs, A B, C D, by which means the points, A C, may be moved with precision to any given points; and by lightening the finger screw, M, the legs may be fixed in that position.

O P, P Q, are two other steel bars, which turn freely upon the pivots, O, P, Q; and being of equal length, and fixed at equal distances, from the centre of motion, when the leg E F is made to lie over the centre, P, the plane of this leg bisects the angle formed by the other two legs. At P is fixed a circular plate, which turns upon its centre, and to which is attached, by a universal joint, one end of a steel bar. This bar applies to the leg E F at R, and serves to keep it fast in any position in which it may be placed, being tightened there by a screw in a universal joint for that purpose.

When the instrument is in its case, the bar, M N, is detached from the legs, for the sake of portability. Therefore, when the instrument is to be used, it must first be replaced. This is done by putting the pillar at N into a hole made in the leg, C D, to receive it, where it is retained by tightening a little finger screw which just appears at *e*. The bar is made to lie over a pillar at M, where a screw is also applied to make it fast when required.

The usual scales of lines, planes, solids, and polygons, together with a new one for laying down angles, are placed upon the two legs, A B, C D. These scales are to be used as they would be when applied to the common proportional compasses. But in these latter we are limited to the determination of a single line by each operation with the instrument; whereas by the compasses now described, we are able to obtain, as far as concerns the reduction of lines and superficies, three lines at once, with nearly the same facility as one is found by the other instrument. For while the legs of the compasses are closed, the sockets, G, H, I, may be moved together to any part of the legs, according to the proportion required to subsist between the distances of the three points, A, C, E,

at one end, and the distances of the three points, B, D, F, at the other. Then if the socket G is brought to any one of the divisions of the line of lines, on opening the compasses so as to make the three points at one end coincide with any three given points, the three points at the other end will form a triangle, whose sides will have, to the sides of the one given, a proportion indicated by the number of the division at which the socket is placed.

For example, if it were required to construct an isosceles triangle, the lengths of whose sides should be equal respectively to half those of a given isosceles triangle, as T V U, Fig. 2. Proceed thus: move all the sockets together, till that extremity nearest to A, of the socket G, comes to the second division of the scale of lines; and there make them fast by means of the little finger screw placed in each. Open the points, A, C, to the extent of T U, the base of the given triangle; and make it fast by tightening the screw at M. Then if the third leg, E F, does not lie directly over the centre of the joint at P, it may be brought into that position by turning the cylindrical axis by means of the lever at L. When this is done, its plane bisects the angle formed by the two legs, A B, C D; and it may be kept in this plane by making tight a screw, whose head appears at X (Fig. 1), which is placed on the cylinder for that purpose. The leg E F may now be moved by hand in this plane; so that if the points A and C coincide with T and U, the point E will move in the direction of Z V, perpendicular to T U and the three points, A, C, E, will always form the sides of a given isosceles triangle, as T V U. When the point E coincides with V, the leg E F may be fixed in that position by turning the bar, P R, till it lies by the side of the jointed piece at R, to which it may then be made fast by tightening the finger screw placed there for the purpose. If the compasses are now inverted, so that the points B, D, F, are brought to the paper, those points will form a similar isosceles triangle whose sides will be respectively equal to half of those of the given triangle.

When it is required to reduce an oblique triangle, as *d e f*, Fig. 3, in any proportion, the two points, A, C, may be brought to *d* and *e*, and the point E to *f*. Then the three points, D, B, F, being applied to the paper will form the three sides of a triangle similar to the former, but in a reversed position, as *d′ e′ f′*, Fig. 4. This reversion of the figure may, in some cases, be considered as an inconvenience; but it may be remedied by using the following process:—Apply the point B to *d′*, and with the point E strike an arc about *f″*. Then applying the point A to *e′*, with E across the former arc in *f″*, the triangle *a′ e′ f″* will be precisely similar to the one given.

In a similar manner, by bringing the socket to any division on the line of planes, a triangle may be found whose superficies will be to that of the given triangle in a proportion indicated by the number on the scale.

Maps or plans of any kind may be enlarged or reduced in any

proportion by means of this instrument with great facility, either drawing, or supposing to be drawn, over them a number of isosceles triangles, and setting the instrument so as to obtain other triangles similar to them, by which means the position of every point required may be determined. For example, extend the legs of the compasses till the points, A and C, lie on two of the points given in the original plan. Then having brought the leg E F into a plane bisecting the angle formed by the other two legs, as before shown, move it in this plane till the point E falls upon some other point in the given plan, as a town, the bend of a river, or any other object. Then the three opposite points of the compasses will form a similar triangle, which may be transferred to the intended drawing.

It has been already said that there is a scale of degrees added to this instrument for the purpose of laying down angles on paper. The method of using it is as follows:—Slide the index of the socket to the division on the scale of angles, which expresses the number of degrees in the required angle (suppose 40°). Then open the points, A, C, of the compasses to any extent at pleasure, and setting one foot in the given line *k l* (Fig 5), at the place intended for the angular point, with the other foot strike an arc, as *l m*. Then set one of the points, B or D, in *l*; with the other cross the arc in *m*, and draw the line *k m*; the angle *m k l* contains 40°.

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## Article IV.

*Further Account of the Aluminous Chalybeate Spring described in the Annals for July,* 1816. By Thomas Lauder Dick, Esq.

(To Dr. Thomson.)

SIR, *Relugas, near Forres, July* 9, 1816.

In your number for this month, which has just reached me, I observe you have done me the honour to insert my account of the aluminous chalybeate spring at Fountain Hall. In that communication I stated that a longer course of barometrical observations, contrasted with those made at the same time to mark the alternations of the spring, would be necessary to establish the theory I had ventured to suggest as an explanation of the phenomena attending it. My father having lately sent me the continuation of his register, which has been kept with the utmost accuracy according to the plan proposed, I am now enabled to send you the following copy of it. I need not here repeat my explanation of the letters employed as contractions, as they have been already given in your 43d number. I may mention, however, that the different quantities discharged by the spring in the space of a minute are here marked

in English pints, the water having been received in a pitcher for that length of time, and then measured. The observations were generally made at six o'clock in the morning, and four o'clock in the afternoon.

*Register of the intermittent Appearances in the Fountain Hall Chalybeate Spring, continued.*

1816.

March 20, M, (as given before) 10 in. T, 32 deg. B, 29·68 in.—A, 8 in. T, 48 deg. B, 29·68.

21, M, 3 in. T, 36 deg. B, 29·48 inches.—A, 4½ in. T, 43 deg. B, 29·52 inches.

22, M, 6 in. T, 31 deg. B, 29·70 inches.—A, 6 in. T, 44 deg. B, 29·79 inches.

23, M, 6½ in. T, 30 deg. B, 29·90 inches.—A, 5 in. T, 46 deg. B, 29·96 inches.

24, M, 3½ in. T, 36 deg. B, 29·88 inches.—A, 3½ in. T, 45 deg. B. 29·58 inches.

25, M, 2½ in. T, 34 deg. B, 29·79 inches.—A, 3 in. T, 45 deg. B, 29·79 inches.

26, M, 3 in. T, 36 deg. B, 29·88 inches.—A, 4 in. T, 46 deg. B, 29·90 inches.

27, M, 3 in. T, 36 deg. B, 29·90 inches.—A, 2½ in. T, 41 deg. B, 29·80 inches.

28, M, 2 in. T, 34 deg. B, 28·84 inches.—A, 2 in. T, 41 deg. B, 29·81 inches.

29, M, 2 in. T, 34 deg. B, 29·81 inches.—A, 2½ in. T, 37 deg. B, 29·85 inches.

30, M, 2½ in. T, 33 deg. B, 29·85 inches.—A, 2 in. T, 38 deg. B, 29·85 inches.

31, M, 3 in. T, 34 deg. B, 29·85 inches.—A, 2½ in. T, 40 deg. B, 29·85 inches.

April 1, M, 1 in. T, 30 deg. B, 29·77 inches.—Eleven o'clock forenoon, 3½ in.—A, F. T, 42 deg. B, 29·59 inches.—Six o'clock evening, R.

2, M, F. T, 34 deg. B, 29·48 inches.—A, ⅛ in. T, 44 deg. B, 29·48 inches.

3, M, 2½ in. T, 32 deg. B, 29·57 inches.—A, 3½ in. T, 43 deg. B, 29·68 inches.

4, M, 3 in. T, 34 deg. B, 29·72 inches.—A, 2½ in. T, 37 deg. B, 29·77 inches.

5, M, ½ in. T, 34 deg. B, 29·66 inches.—Two o'clock, A, R, but hardly perceptible.—Four o'clock, A, R, the thickness of a goose-quill. T, 42 deg. B, 29·47 inches.

6, M, discharge increased, and now equal to the thickness of the little finger. T, 37 deg. B, 29·27 inches.—Ten o'clock, R, above two pints.—Four o'clock, A, R, increased considerably. T, 44 deg. B, 29·18 inches.—Six o'clock evening, R, diminished.

7, M, R, increased. T, 34 deg. B, 29·04 inches. Ground thinly covered with snow.—A, R, diminished. T, 38 deg. B, 29·08 inches. Frequent showers of sleet.

8, M, R, hardly perceptible. T, 33 deg. B, 29·02 inches. Stopt a leak in the channel, when the R from the basin of the well increased to the thickness of a goose-quill.—A, R, 1½ pint. T, 37 deg. B, 29·00 inches. A great fall of sleet during the day.—Six o'clock evening, ½ in.

9, M, 4 in. T, 35 deg. B, 29·16 inches. A great fall of rain last night.—A, 3½ in. T, 40 deg. B, 29·23 inches.

10, M, 3 in. T, 36 deg. B, 29·25 inches.—A, 2½ in. T, 37 deg. B, 29·30 inches. A high wind at north-east, with a great deal of rain.

11, M, 3 in. T, 38 deg. B, 29·39 inches. A great fall of rain from

north-east, which still continues.—A, 3 in. T, 39 deg. B, 29·17 inches. Wind north-east. A fog, and drizzling rain.

12, M, 2 in. T, 37 deg. B, 29·52 inches. Wind north-east.—A, 2 in. T, 40 deg. B, 29·58 inches. A cold wind from north-east.

13, M, 1 in. T, 30 deg. B, 29·56 inches. Ground thinly covered with snow. High north-east wind.—Eleven o'clock forenoon, ⅛ in.—A, R. T, 35 deg. B, 29·38 inches.—Ten o'clock at night, T, 28 deg.

14, M, R, increased, and equal to a large goose-quill. T, 37 deg. B, 29·25 inches.—A, R, diminished. T, 37 deg. B, 29·24 inches.

15, M, ⅛ in. T, 30 deg. B, 29·27 inches.—A, F. T, 40 deg. B, 29·27 inches.

16, M, R. T, 34 deg. B, 29·14 inches. Sleet falling.—Ten o'clock forenoon, R, 4½ pints.—A, R, 7½ pints. T, 44 deg. B, 29·02 inches.—Seven o'clock evening, R, 8 pints. B, 28·76 inches.

17, M, R, 5 pints. T, 35 deg. B, 28·94 inches.—Ten o'clock forenoon, F. B, 29·00 inches.—A, 2 in. T, 42 deg. B, 29·10 inches. A heavy shower of hail.

18, M, 1½ in. T, 34 deg. B, 29·13 inches. Snow covering the ground, and still falling.—A, ⅛ in. T, 36 deg. B, 29·13 inches. A great fall of sleet, which still continues.—Seven o'clock evening, R. B, 29·09 inches.

19, M, 1 in. T, 30 deg. B, 29·10 inches. Snow two inches deep; trees, hedges, and bushes, completely powdered.—A, 2 in. T, 44 deg. B, 29·24 inches. Snow all melted.

20, M, 4 in. T, 34 deg. B, 29·49 inches.—A, 4 in. T, 46 deg. B, 29·50 inches.

21, M, ⅛ in. T, 38 deg. B, 29·42 inches.—Eleven o'clock forenoon, F.—A, R, a very little. T, 57 deg. B, 29·38 inches.

22, M, F. T, 37 deg. B, 29·37 inches.—A, ⅛ in. T, 53 deg. B, 29·42 inches.

23, M, 1½ in. T, 40 deg. B, 29·54 inches.—A, 1½ in. T, 47 deg. B, 29·60 inches.

24, M, 1 in. T, 39 deg. B, 29·66 inches.—A, ⅛ in. T, 42 deg. B, 29·70 inches. Wind east. A thick drizzling fog.

25, M, ½ in. T, 38 deg. B, 29·77 inches.—A, 1 in. T, 43 deg. B, 29·82 inches. A thick fog from the east all day.

26, M, F. T, 36 deg. B, 29·87 inches.—A, R, a little. T, 52 deg. B, 29·79 inches. A fog in the morning, but now dispersed.

27, M, R, but little. T, 43 deg. B, 29·72 inches.—A, R, increased a little. T, 43 deg. B, 29·70 inches.—A thick easterly fog all day.

28, M, R, not measured, but supposed to be 3 pints. T, 39 deg. B, 29·56 inches.—A, R, not less than 6 pints. T, 55 deg. B, 29·38 inches.

29, M, R, 9 pints. T, 45 deg. B, 29·25 inches.—Ten o'clock forenoon, R, 10 pints.—A, R, 9 pints.—T, 50 deg. B, 29·20 inches.

30, M, R, 5½ pints. T, 44 deg. B, 29·22 inches.—A, R, 4¾ pints. T, 49 deg. B, 29·27 inches.

May 1, M, R, 4 pints. T, 45 deg. B, 29·24 inches.—Ten o'clock forenoon, R, 3½ pints.—A, R, 3½ pints. T, 51 deg. B, 29·24 inches.

2, M, R, 1 pint. T, 42 deg. B, 29·30 inches.—A, R, 1½ pints. T, 50 deg. B, 29·35 inches. Thunder and rain.

3, M, R, 2½ pints. T, 43 deg. B, 29·35 inches.—A, R, 3 pints. T, 46 deg. B, 29·35 inches.

4, M, R, but quantity so small as not to be measurable. T, 42 deg. B, 29·50 inches.—A, R, stopped, but F. T, 52 deg. B, 29·58 inches.

5, M, R, not measured, but supposed to be 4 pints. T, 47 deg. B, 29·42 inches.—A, R, increased. T, 54 deg. B, 29·30 inches.—Seven o'clock evening, R, supposed not less than 8 pints.

6, M, R, 2½ pints. T, 47 deg. B, 29·42 inches.—A, R, 1 pint. T, 47 deg. B, 29·55 inches.

7, M, R, 4½ pints. T, 46 deg. B, 29·48 inches.—A, R, nearly 10 pints. T, 50 deg. B, 29·40 inches.—Seven o'clock evening, R, 11 pints. T, 46 deg. B, 29·28 inches. A wetting rain all day, and now very heavy.

8, M, R, 11½ pints. T, 43 deg. B, 29·20 inches.—A, R, 11 pints. T, 46 deg. B, 29·18 inches.—Seven o'clock evening, R, 11 pints.

9, M, R, 5¼ pints. T, 40 deg. B, 29·23 inches. A great fall of rain last night.—A, R, 3 pints. T, 42 deg. B, 29·30 inches.

10, M, R, 7 pints. T, 37 deg. B, 29·24 inches.—A, R, 10 pints. T, 41 deg. B, 29·14 inches.—Seven o'clock evening, R, 11 pints.

11, M, R, 11 pints. T, 35 deg. B, 29·10 inches. Snow lying in some places.—A, R, 11 pints. T, 40 deg. B, 29·09 inches. Frequent showers of hail and sleet.

12, M, R, apparently as last night. T, at five o'clock, 34 deg. B, 29·10 inches. The roof of the house and the grass white with snow.—A, R, apparently increased. T, 48 deg. B, 29·13 inches.

13, M, R, 13 pints. T, 38 deg. B, 29·20 inches. N. B. On Saturday last, the 11th, in the evening, the well was cleared out; and whilst the work was going on, the discharge evidently increased, and has since continued to do so.—A, R, 10½ pints. T, 50 deg. B, 29·29 inches.—Eight o'clock evening, R, 10½ pints.

14, M, R, supposed as last night. T, 42 deg. B, 29·38 inches.—Ten o'clock forenoon, R, 8 pints.—A, R, apparently the same. T, 53 deg. B, 29·44 inches.

15, M, R, 10½ pints. T, 45 deg. B, 29·50 inches.—A, R, 16½ pints. T, 55 deg. B, 29·49 inches.—Eight o'clock evening, R, 17 pints.

16, M, R, 18 pints. T, 49 deg. B, 29·48 inches.—A, R, 20 pints. T, 62 deg. B, 29·46 inches.—Eight o'clock evening, R, 20 pints.

17, M, R, 20 pints. T, 50 deg. B, 29·43 inches. Soft showers of rain.—A, R, 17¼ pints. T, 55 deg. B, 29·48 inches.—Eight o'clock evening, R, 17 pints.

18, M, R, 14 pints. T, 45 deg. B, 29·57 inches.—A, R, 15 pints. T, 52 deg. B, 29·60 inches.

19, M, R, apparently increased. T, 46 deg. B, 29·60 inches.—A, R, strong. T, 53 deg. B, 29·52 inches.

20, M, R, 20 pints. T, 48 deg. B, 29·52 inches.—A, R, 21 pints. T, 53 deg. B, 29·57 inches.

21, M, R, 19 pints. T, 49 deg. B, 29·60 inches.—A, R, 18¾ pints. T, 62 deg. B, 29·64 inches.

22, M, R, 18 pints. T, 45 deg. B, 29·69 inches.—A, R, 17 pints. T, 45 deg. B, 29·74 inches.

23, M, R, 20 pints. T, 44 deg. B, 29·76 inches.—A, R, 21 pints. T, 54 deg. B, 29·80 inches.

24, M, R, 22¾ pints. T, 46 deg. B, 29·77 inches.—A, R, 30 pints. T, 59 deg. B, 29·62 inches.—Eight o'clock evening, R, 31 pints.

25, M, R, 33 pints. T, 52 deg. B, 29·40 inches.—Ten o'clock forenoon, R, 36¾ pints.—A, R, 32¼ pints. T, 46 deg. B, 29·41 inches.—Eight o'clock evening, R, 27¼ pints. B, 29·50 inches.

26, M, R, apparently as last night. T, 45 deg. B, 29·59 inches.—A, R, not measured. T, 51 deg. B, 29·41 inches.

27, M, R, 17½ pints. T, 44 deg. B, 29·65 inches.—A, R, 20 pints. T, 63 deg. B, 29·80 inches.

28, M, R, 29¼ pints. T, 48 deg. B, 29·67 inches.—A, R, 24 pints. T, 58 deg. B, 29·70 inches.

29, M, R, 25½ pints. T, 48 deg. B, 29·69 inches.—A, R, 29¼ pints. T, 56 deg. B, 29·55 inches.—Eight o'clock in the evening, R, 34 pints.

30, M, R, 33 pints. T, 47 deg. B, 29·50 inches.—A, R, 30 pints. T, 58 deg. B, 29·50 inches.

31, M, R, 29½ pints. T, 46 deg. B, 29·49 inches. A, R, 28¾ pints. T, 57 deg. B, 29 69 inches.

June 1, M, R, 22 pints. T, 45 deg. B, 29·56 inches.—A, R, 25 pints. T, 55 deg. B, 29·53 inches.

2, M, R, evidently increased. T, 50 deg. B, 29·43 inches.—A, R, decreased considerably. T, 58 deg. B, 29·58 inches.

3, M, R, apparently increased, and measured 22¼ pints. T, 50 deg. B, 29·50 inches.—A, R, 20 pints. T, 56 deg. B, 29·59 inches.

4, M, R, 31 pints. T, 49 deg. B, 29·49 inches.—A, R, 31¾ pints. T, 46 deg. B, 29·46 inches.

5, M, R, 32 pints. T, 44 deg. B, 29·46 inches.—A, R, 30¼ pints. T, 48 deg. B, 29·42 inches.

6, M, R, 22½ pints. T, 42 deg. B, 29·50 inches.—A, R, 22¼ pints. T, 53 deg. B, 29·59 inches.

7, M, R, 31 pints. T, 50 deg. B, 29·37 inches.—A, R, 37 pints. T, 54 deg. B, 29·30 inches.

8, M, R, 36 pints. T, 44 deg. B, 29·23 inches.—A, R, 38¼ pints. T, 48 deg. B, 29·18 inches.—Eight o'clock evening, R, 38¾ pints.

9, M, R, apparently as last night. T, 43 deg. B, 29·17 inches.—A, R, diminished. T, 54 deg. B, 29·17 inches.

10, M, R, 18½ pints (being 20½ pints less than 8th A). T, 44 deg. B, 29·29 inches.—A, R, 12¼ pints. T, 56 deg. B, 29·60 inches.

11, M, R, 12¼ pints. T, 48 deg. B, 29·50 inches.—A, R, 19¾ pints. T, 55 deg. B, 29·59 inches.

12, M, R, 20½ pints. T, 54 deg. B, 29·55 inches.—A, R, 27½ pints. T, 58 deg. B, 29·48 inches.

13, M, R, 22¼ pints. T, 53 deg. B, 29·50 inches.—A, R, 28½ pints. T, 62 deg. B, 29·45 inches.

14, M, R, 15½ pints. T, 48 deg. B, 29·67 inches.—A, R, 25 pints. T, 57 deg. B, 29·70 inches.

15, M, R, 18¼ pints. T, 48 deg. B, 29·77 inches.—A, R, 19 pints. T, 60 deg. B, 29·77 inches.

16, M, R, visibly increased. T, 48 deg. B, 29·72 inches.—A, R, decreased. T, 60 deg. B, 29·69 inches.

17, M, R, 27½ pints. T, 46 deg. B, 29·67 inches.—A, R, 29 pints. T, 62 deg. B, 29·59 inches.

18, M, R, 31 pints. T, 51 deg. B, 29·50 inches.—A, R, 28½ pints. T, 58 deg. B, 29·50 inches.

19, M, R, 25 pints. T, 53 deg. B, 29·54 inches. A fine moderate rain last night and this morning.—A, R, 25¾ pints. T, 60 deg. B, 29·59 inches. Showers of rain continue to fall.—Six o'clock evening thunder, and a heavy rain, for near half an hour.

20, M, R, 18¼ pints. T, 50 deg. B, 29·69 inches.—A, R, 20 pints. T, 67 deg. B, 29·73 inches.

21, M, R, 18¼ pints. T, 54 deg. B, 29·76 inches.—A, R, 25 pints. T, 69 deg. B, 29·74 inches.

Many of the results in the foregoing extensive register of careful and accurate observations will appear, at first sight, to militate against a theory attributing the intermittent appearances of the spring in question to the variations of atmospherical pressure; as the depression of the water of the basin, or the increase of the discharge from it, are by no means invariably attended by an elevation of the mercurial column in the former case, or its fall in the latter. Many instances even of the very contrary arrangement will be observed. Nor will the following table of the averages of the height of the barometer during the different states of the water of the spring, appear altogether unequivocal, though a nearer approximation to a correspondence with the theory will be remarked in it.*

* In order to curtail the table as much as possible, I have classed the measures which have fractions attached to them with those of the number of pints under them; as, for instance, I have classed 18¾ pints amongst the results of 18 pints.

| No. | State of the water of the basin. | Number of observations. | Average of the barometer. |
|---|---|---|---|
| 1 | Running over 38 pints | 3 | 29·17 inches |
| 2 | ..... 37 | 1 | 29·30 |
| 3 | ..... 36 | 1 | 29·23 |
| 4 | ..... 34 | 1 | 29·70 |
| 5 | ..... 33 | 2 | 29·45 |
| 6 | ..... 32 | 2 | 29·43 |
| 7 | ..... 31 | 5 | 29·45 |
| 8 | ..... 30 | 3 | 29·51 |
| 9 | ..... 29 | 5 | 29·57 |
| 10 | ..... 28 | 4 | 29·53 |
| 11 | ..... 27 | 6 | 29·55 |
| 12 | ..... 25 | 6 | 29·58 |
| 13 | ..... 22 | 6 | 29·57 |
| 14 | ..... 21 | 3 | 29·65 |
| 15 | ..... 20 | 7 | 29 58 |
| 16 | ..... 19 | 3 | 29·65 |
| 17 | ..... 18 | 5 | 29·57 |
| 18 | ..... 17 | 3 | 29·62 |
| 19 | ..... 16 | 1 | 29·49 |
| 20 | ..... 15 | 4 | 29·61 |
| 21 | ..... 14 | 1 | 29·57 |
| 22 | ..... 13 | 1 | 29·20 |
| 23 | ..... 12 | 2 | 29·55 |
| 24 | ..... 11 | 7 | 29·15 |
| 25 | ..... 10 | 6 | 29·35 |
| 26 | ..... 9 | 2 | 29·22 |
| 27 | ..... 8 | 1 | 28·76 |
| 28 | ..... 7 | 2 | 29·13 |
| 29 | ..... 6 | 1 | 29·38 |
| 30 | ..... 5 | 3 | 29·13 |
| 31 | ..... 4 | 5 | 29·34 |
| 32 | ..... 3 | 4 | 29·36 |
| 33 | ..... 2 | 4 | 29·24 |
| 34 | ..... 1 | 6 | 29·20 |
| 35 | Running under 1 | 13 | 29·38 |
| 36 | Basin brim full. | 8 | 29·43 |
| 37 | Below the brim $\frac{1}{8}$ inch | 7 | 29·40 |
| 38 | ..... $\frac{1}{2}$ | 2 | 29·71 |
| 39 | ..... 1 | 5 | 29·58 |
| 40 | ..... $1\frac{1}{2}$ | 3 | 29·42 |
| 41 | ..... 2 | 7 | 29·55 |
| 42 | ..... $2\frac{1}{2}$ | 8 | 29·72 |
| 43 | ..... 3 | 6 | 29·55 |
| 44 | ..... $3\frac{1}{2}$ | 7 | 29·71 |
| 45 | ..... 4 | 4 | 29·51 |
| 46 | ..... 5 | 1 | 29·96 |
| 47 | ..... 6 | 2 | 29·74 |
| 48 | ..... $6\frac{1}{2}$ | 2 | 29·71 |
| 49 | ..... 8 | 1 | 29·68 |
| 50 | ..... 10 | 1 | 29·68 |

But notwithstanding the various anomalies afforded by this table, there still appears in it enough of correspondence between the rise of the barometer and the depression of the water in the basin of the well, and *vice versâ*, to satisfy us that atmospherical pressure is

the grand controlling cause of the phenomena; although I certainly feel inclined to attribute more influence now than I did formerly to the operation of those other modifying causes which, as I already hinted, must exist in the interior of the coal wastes. It is true that no regular gradation of increase in the altitude of the mercurial column is observable in the foregoing table; yet we perceive a general tendency in the barometer to rise, proceeding from No. 1 downwards. But what places the probability of the truth of the theory in a still stronger light, is the drawing of a dividing line in the scale of the whole alternations of the spring. Let us, for example, take what will probably be considered as the most natural point of division, viz. that which marks the basin as *full*, but not running over; and we shall find that the average height of the barometer applying to the whole observations which are stated to have been above this point, will be about 29·44 inches. On the other hand, the average of the barometrical notations corresponding in time with those observations denoting the water to have been below the point *full*, will be found to be about 29·57 inches, being $\frac{13}{100}$ of an inch more than the average referring to the whole observations which are above the point *full*, a difference by no means of trifling considerations.

We know very well that the barometer, when considered as an instrument for pointing out the changes of weather, is very often found to deceive, and is sometimes observed to be affected before the atmospherical alteration appears to take place; whilst, on the other hand, it is frequently depressed or elevated, after the cause has apparently gone by. It does not appear, however, that this well-known circumstance will apply to the case in question, so as to account for those numerous instances of anomaly which are to be noticed in the register. Indeed, if any weight is to be given to the theory at all, the spring itself can be viewed in no other light than as a barometer; and were it perfectly uninfluenced by those subterranean causes which are supposed occasionally to modify its alternations, its depressions would undoubtedly bear an exactly relative proportion to the elevations of the mercurial column; the basin of the well, A, (Pl. LI. Fig. 7, No. XLIII.) representing the basin of the barometer; and the waste, E D C, in which the column of water is supported by the atmospherical pressure, answering to the tube of that instrument. We must, therefore, seek for other reasons to account for the anomalous appearances which exist in the register; and as these, from their situation, are less easily got at, than that arising from the atmosphere, which appears to be the predominant cause, we can at best only form conjectures as to their nature

One of the first of these which presents itself is that of the increase and diminution of the quantity of water in the vast reservoir contained in the wastes of the mine, arising from the melted snow, and rain poured from time to time on the earth's surface. But although this may very well be set down as having its full share in producing the irregularity remarkable in the phenomena, yet even

the attempt to subject it to what may be called conjectural proof, would probably be perfectly unavailing. Varieties of situations, soils, and strata, produce corresponding differences in the length of time required to transmit surface water to such a depth. My father, who may be considered as a coal miner of some experience, writes me in the envelope of his last communication on this subject, that "colliers observe that, after a great fall of rain, or the melting of snow, it is six weeks or two months before it reaches them in their works below ground. This, no doubt, will vary according to the depth, and as the different strata are more or less impervious to water." Now it is evident that where the effect follows the cause at such a considerable interval of time, and where it may consequently come frequently into hostility with the effect arising immediately from atmospherical pressure, such a balance, or perhaps even temporary antipreponderance, may be at times established, as very often to produce those seeming contradictions to that supposition which ascribes the alternations in the flow of the spring chiefly to the weight of the atmosphere.*

Another cause having some little influence might originate from the small opening at the point *g* in the plate, and which I formerly described as having been cut by the miners of C, D, E, whilst their coal was yet working, to permit the escape of the water from their mine into the neighbouring levels of that underneath the adjacent property of Wood Hall. We might suppose that the loose materials deposited in it by the falling in of the roof, and which must have been the first occasion of the spring forcing itself up through B A, may be now and then partially penetrated, so as to allow a certain portion of the water to escape there, and so in some degree reduce the height of the water in the basin, A, when the state of the barometer at the time would appear to say that it ought rather, perhaps, to be the reverse. But to this cause I am rather unwilling to allow much activity; as it is highly probable that the hole, *g*, originally communicating between the two mines, must have been very completely packed by the first fall of a vast quantity of solid materials; otherwise it could not possibly have resisted so long the immense weight of water necessarily pressing upon it, which is capable of forcing up the column to its spring at A. And if the smallest outlet through *g* had been once made, it is quite natural to suppose that this immense pressure would have very speedily enlarged it to such an extent as to admit of the whole water of the reservoir in the waste running off in that direction; and so it would have been the means of at once annihilating the spring at A.

As my ingenious friend Mr. Scott has suggested in the same letter of which I formerly quoted a part, the falling in of rubbish, or of the roof, from the decay of its wooden props, or from other

* I may remark here, that there is a strong spring of very pure water close to the house of Fountain Hall, which sometimes becomes dry for months. My father writes me that it gave over running before the end of last summer, and did not again recover its strength till the end of February.

sources of decay in different parts of the wastes, may occasionally mingle with the other causes, by producing temporary obstructions to the subterranean water, in the course of its progress through the more distant or higher parts of the old mine; thus in the first place creating a diminution of the supply, and afterwards as they are *gradually* washed away, or *suddenly* burst through by an accumulation behind them, producing a corresponding *gradual* or *sudden* increase to the quantity of water in the reservoir more immediately near to the bottom of the old shaft A, B, which would naturally affect the spring above it.

There are, no doubt, various other causes which may present themselves to you, Sir, or to your readers, as having a probable aiding influence in producing the effects of alternation in this spring. But it would be almost bordering upon impertinence in me to search for more, as I have already perhaps drawn the subject greatly further than its interest warrants. I shall conclude, therefore, by observing, that in order to put my notion of the predominating influence of atmospherical pressure to the *experimentum crucis*, a shaft ought to be sunk, or bore put down, from the surface into the old waste at C, which, if my ideas are correct, would put a stop to the alternating appearances in the basin A, by establishing an equilibrium in the atmospherical pressure at these two points. But this would be rather too laborious an experiment for an object of such trivial importance; and were it even more easily attainable, it would perhaps be considered a pity to put a period to the existence of the phenomena.

I am, Sir, your obedient humble servant,

THOMAS LAUDER DICK.

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## ARTICLE V.

*An Essay on the Lodgement of Carbureted Hydrogen Gas in Coal Mines, and on the Cause of the Explosions or Blasts peculiar to these Mines.* By Mr. John B. Longmire.

*May* 1, 1816.

In Article II. of the *Annals of Philosophy* for September, 1815, I have described the probable source of the carbureted hydrogen gas, or the inflammable air, of coal-mines, and its mode of entry into the mine. There yet remain for me to show how it lodges in the workings when the air is still, and the phenomena of its combustion, when set on fire in such a state.

### 1. *On the Lodgement of the Inflammable Gas in Coal-Mines.*

When the inflammable gas escapes from the coal into the still air, at the forehead, or sides of a working near its forehead, it ascends to the roof, and collects into a flat body whose under surface

always inclines upwards from the forehead into the mine; and as the inflammable air continues to escape, this body increases in dimensions, by not only extending further into the working, but downwards to the pavement. After the gas has reached the pavement, and when its under surface has approached so near to a horizontal line, that the newly entered gas will not pass up it towards the roof, the further entry of the gas would cease, were it not separated from the coal with a force that enables it to displace, and push forward, the gas previously got into the working. The inflammable gas, therefore, cannot escape from flat coal-mines without filling them, nor can it fill them by its levity alone; * and I may say there does not exist a mine, even in the edge seams, whose workings *can be adapted* to permit its *natural* escape, without *some of them* being *first* filled. A small body of gas is no sooner formed than it begins to mix with the air. The mixing process, however, is often counteracted to a great degree by the working being filled with the newly entered gas almost as fast as the mixing process can extend itself. But this process is almost always discernible; and gives rise to the method adopted by miners to detect, by the candle symptoms, the existence of this gas while yet on its confines, and without setting it on fire. They are enabled to detect it by meeting with it at first mixed with the air in a very small proportion; but which proportion increases as they advance, sometimes so much that in a yard beyond where they first detect it they can set it on fire, and sometimes so little that they may advance 15 or 20 yards to reach the firing point. When the gas leaves the coal slowly, or after it has ceased to leave the coal, the body collected in a working gradually mixes with the air in a small proportion. The quantity of inflammable gas in the mixture is not equal throughout. It is greatest near the forehead, and least near the other extremity. Probably the mean proportion is not far short of what makes the most explodible mixture. Hence, as is too well known to colliers, the same quantity of inflammable gas makes a much more violent explosion after being left a few months in the mine than if fired immediately after it is collected.

## 2. *On the Explosion.*

When the inflammable gas is set on fire in a separate state, though in a large body, it burns slowly; but when mixed with the air in a certain due proportion, it is no sooner fired in one part than it appears to flame in every part of its volume.

* The levity of this gas will not carry it out of workings so well as persons may suppose who have not seen it in the mine. It will rise perpendicularly, and ascend angularly; but I have not been able to determine its lowest angle of ascension. I have seen a body of it, mixed indeed with the vapour of water, suspended in a working whose roof lay at an angle of 50°. The mixture of this gas with azote or carbonic acid will undoubtedly diminish the levity.

During the combustion of the carbureted hydrogen gas, the following chemical changes take place:—The hydrogen of the gas unites with the oxygen of the air, and water results; and the carbon of the gas is also joined to more of the oxygen, and carbonic acid is formed; while the azote, from which the oxygen was taken, is left in a separate state. The azote and carbonic acid gas are for some time diffused through the pure air; but they slowly separate in part, and the azote, being the lighter, ascends towards the roof, and the carbonic acid descends, and rests on the pavement; while the water, which is at first a vapour, cools slowly, and collects into small drops on the roof, sides, and bottom of the mine, and on the bodies of those miners who are lying on the pavement, either burnt to death, or so burnt and maimed by the mechanical violence of the chemical changes as not to be able to escape. Very soon after the combustion commences the flame spreads out much beyond the original volume of the mixture, but it principally extends in the direction of the coming air. This motion is owing to the expansion of the airs by the heat given out in the first part of the combustion; and the meeting of the coming air by the flame is to be attributed to the want of pure air in any other direction to supply *rapid* combustion; as the air in every other part of the adjoining hollows is contaminated with the noxious gases newly generated.

During the combustion of the gas that motion is given to the air which miners call a blast, and philosophers an explosion. It has been imagined that the united volume of hydrogen and oxygen is reduced, when changed from gases into water, so very much, and so very rapidly, that an extensive vacuum is suddenly formed, into which the air must rush rapidly from every quarter. But it is too well known that the blast flies *from the place of combustion*, and not towards it, as it ought to do were this reasoning true. Therefore it is evident that the heat given out greatly expands the surrounding air; so that the increase of the air's volume not only counteracts the deficiency made in the bulk of the united gases, by forming them into water, but also causes the first and greatest blast or explosion to move rapidly from the fire into other parts of the mine, till the heat has nearly distributed itself equally; and those counter-currents, or smaller blasts, which very often occur, are produced by the action of the intricate passages on the main blast during its motion through the mine.

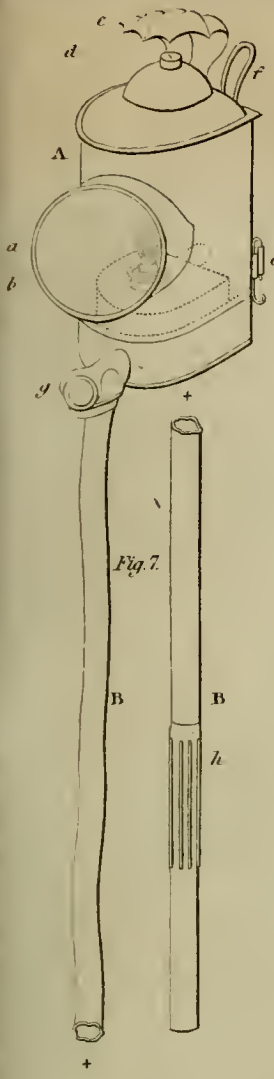
The explosion is a concussion in the air similar to that made by firing a cannon, though differently effected. The impinged particles move from the cannon through the space above the earth's surface, and soon exhaust their force on this expanse of particles; but in the mine their motion is confined to small hollows. Hence the same explosion that at 200 or 300 yards from the cannon could only break a few windows, would carry devastation through an extensive mine.

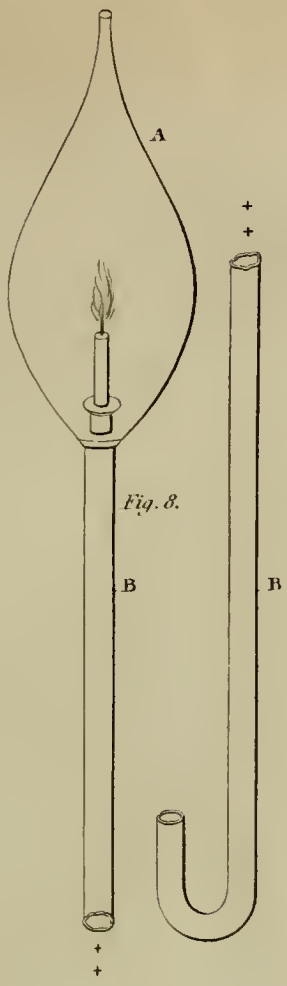
The following properties distinguish the explosion, and enable

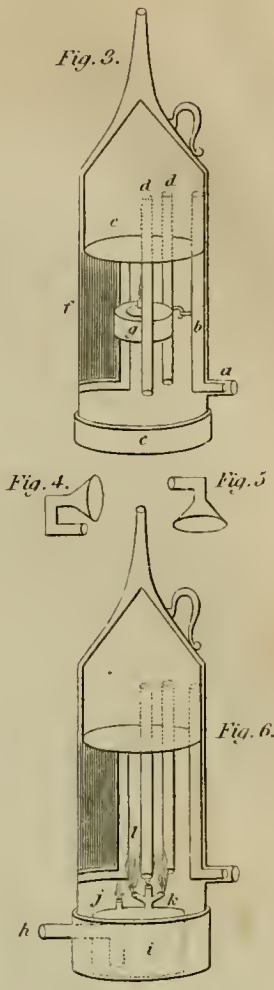
us to develope the action of the heat on the air, when an explosion is formed, and to account for its peculiar and extraordinary effects on the mine. Suppose an explosion happen at the far end of a long and straight tunnel, out of which the air cannot escape but at the mouth, and in which four men are placed at certain distances from one another. The explosive motion flies from the close end of the tunnel towards its mouth, and fells the first man to the ground, only blows out the second man's candle, merely bends horizontally the flame of the third man's, and is not felt by the fourth man. It thus appears that the explosive motion is greatest in both velocity and force at its commencement, decreases in both, and at last ceases to move. Again, the explosive motion, after flying some distance in a straight line, will pass openings in the side of a straight tunnel or working without losing much of its force, or communicating much motion to the air in them. Thus suppose the air moves rapidly in the direction *a b*, (Plate LVIII., Fig. 1,) on passing by the workings, *c, d, e, f, g,* it merely disturbs the air in them so much as to blow out a candle. Lastly, if the air in explosive motion, hitherto in a straight line, strike angularly the side of any working, it departs from the part struck, with a little less angle than that by which it approached. Suppose the air in motion fills the working, and moves along the part *a b*, Fig. 2, till it strikes the side *b f* at *b*; it leaves this side at an angle, *d b e*, which is smaller than its angle of approach, *a b c*, because the moving force gradually decreases, and the air loses a part of its velocity every time it strikes the side. The explosive motion then flies to the side *e g*, at *e*, at the same angle that it left the other side, but leaves it again at a little less angle, *g e f*. It therefore moves from side to side with a decreasing angle, and an increasing distance between the side impingements, till the motion ceases.

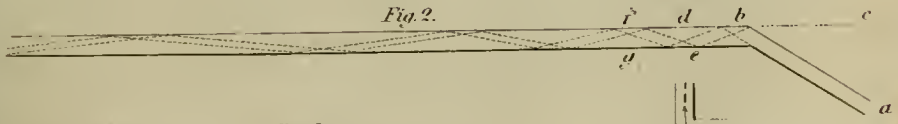
The foregoing properties of the explosive motion entitle us to give the following reasoning as to its cause. Let us suppose that a quantity of heat is given out at the forehead of a working; let us imagine the air to be divided into ranges of particles which are at right angles to the roof, sides, and pavement, and parallel to the forehead of a working; and let it be remembered that philosophers have shown the particles of air to be, not in actual contact, but at distances from one another, which indeed are too small to be sensible to us.

The heat at the forehead enters among the particles of the air, and increases the distances between them, swells out the volume of the part acted on, and moves the air in every direction, but in a little time the motion is only along the working. If, now, heat enters between two upright ranges of particles, it separates them to a greater distance; but that range which is nearest the forehead cannot be moved backwards, because the heat is equally great on that side of it; but the other range is moved forward, strikes and drives before it the second range, before the heat can reach it. This last range moves the third, the third moves the fourth, and so

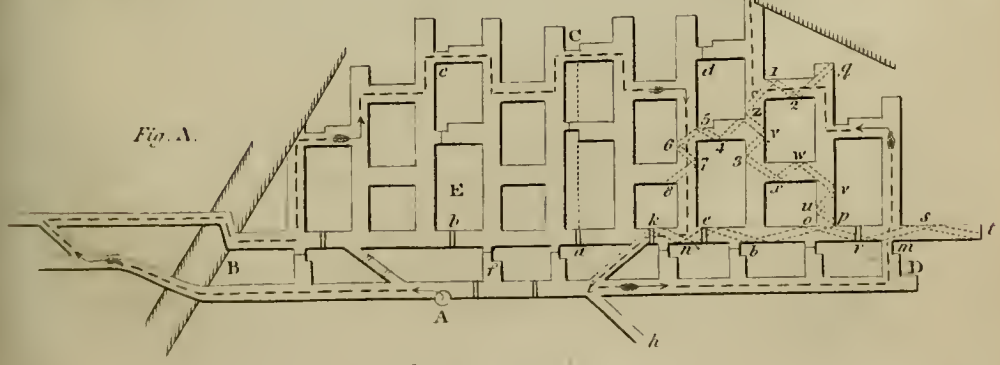







Safety Lamps &c.

Engraved for W. Thomson's Annals — for Baldwin Cradock & Joy Paternoster Row Nov. 1 1816.

on; but as a small part of the force with which the first moved the second is spent upon the latter range, this range moves the third with a force that is a very little less than that by which it was moved itself, so that in a given distance the motion ceases altogether. The first portion of heat being followed by another, passes on through the second range of particles, separates the particles in this range as far from one another as the first and second ranges are asunder; of course, drives a certain number of particles out of it, and then moves forward them and those in the third range; and as these particles impinge those before them, the explosive motion is continued. Again, as these portions of heat are followed by another portion, the first of them passes on from between the first and second range into the third, and the other through the third range into the space between the third and fourth ranges, driving before it a certain number of particles out of the third range, and pushing on them and those in the fourth range; and as these particles strike others, the impetus already given to the air is continued so long as the gas burns. The rapidity with which the heat acts, in this way, on the air, puts it into that violent motion so characteristic of an explosion. When the combustion ceases, the heat given out distributes itself through the surrounding parts of the mine, the motion is dispersed through those parts, and its violence is at first divided, and then gradually reduced.

It would be impossible to foretel how the air's explosive motion thus described and accounted for, would act on the mine in every instance of combustion; because, as the parts affected bear relations to one another, and to the centres of motion, which differ in every case, so the effects on these parts will be varied accordingly. But I will, in my next communication, select one explosion, and describe such particulars relating to it as will apply in every instance of combustion, by being modified to suit the peculiarities of every situation.

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## Article VI.

### *Practical Observations on Safety Lamps for Coal Mines.*

By Willian Reid Clanny, M.D.

(With a Plate.)

At the request of my friends, and from a desire to comply with the wishes of those persons who have been using for some time the original safety lamps, the steam safety lamps, and the gas light lamps, I am induced to publish these concise directions; and if I shall have occasion to advert to some other particulars connected with the subject, I trust the reader will find an excuse for me, when

I inform him that explosions in coal mines have occupied my unremitted attention for the last eight years.

### *The Original Safety Lamp.*

These lamps have been known to the scientific world, and to those concerned in coal mines in this district, for several years. The first printed account of them appeared in the Philosophical Transactions of the Royal Society for 1813: since which time all the respectable journals and periodical publications have given most satisfactory reports of their value; partly drawn from authenticated sources, and partly from their actual practical utility in coal mines, where fire-damp abounded.*

For this lamp in an improved state I had the honour to receive, Dec. 3, 1813, the unanimous thanks of the Society for preventing Accidents in Coal Mines.

I had the pleasure, in October last, accompanied by J. H. H. Holmes, Esq. and Mr. Patterson, of the Herrington Mill Pit, to take the *first light* into a field of fire-damp at the exploding point, which before that time was considered by most persons concerned in coal-mines as an impossibility. This lamp has been in constant use wherever great danger was apprehended from fire-damp since the above period. In a word, the originality and priority of my idea of an insulated light for coal-mines, the construction of a safety lamp, and the establishment of the safety and utility of that lamp in coal-mines greatly infested with fire-damp, are now universally acknowledged.

It was for a *modification* of this original safety lamp that the Society of Arts, with their usual munificence, voted to me a medal in May last, which was the more gratifying, as in 1813 the *original* lamp had been presented by me to the Royal Society.

### *The Steam Safety Lamp.*

Several persons concerned in coal-mines, for whose opinions I entertain much respect, expressed a desire that a safety lamp might be constructed which would feed itself with atmospheric air for combustion without the aid of bellows; and in order that such a desideratum might be supplied, I had the pleasure in November last (1815), to discover, when experimenting with the original safety lamp, in an atmosphere of fire-damp at the Herrington Mill Pit, that when I accidentally used hot water, the fire-damp burned silently at the wick of the oil lamp, and did not explode within the original safety lamp, as formerly, which was the principle of its safety. I accordingly instituted a series of experiments, and invariably found that, by the intervention of steam, the fire-damp might

* *Annals of Philosophy.* Phil. Mag. Mo. Mag. New Mo. Mag. &c. More recently, Holmes on Coal Mines. In this work there are engravings of the original safety lamp and its modification, to which the reader is referred.

be burnt, without explosion, to any extent, at the wick. This extraordinary and unexpected discovery induced me to turn over the leaves of the seventh edition of Dr. Henry's Elements of Experimental Chemistry, in which I observed a reference to some experiments by Von Grotthus upon hydrogen gas, the original of which are therein stated to be inserted in the 82d volume of the Annales de Chimie; and by the kindness of a distant friend I was afterwards enabled to peruse that volume, in which I found a complete corroboration of my experiments upon steam and hydrogen.

In the month of December of the same year, after many tedious experiments, I constructed my steam safety lamp, which I then showed to the Society for preventing Accidents in Coal Mines, for which I had also the honour to receive their unanimous thanks. I intimated to the Society, at the same time, that in this lamp the light was given partly from oil, and partly from the fire-damp when used in the mines.

The steam lamps are constructed of the strongest tinned iron, with flint glass in front, $\frac{3}{8}$ of an inch in thickness; from which it will readily be understood, that these lamps will bear any sort of usage. The steam safety lamps require no trouble or particular attention from the miner when he is using them, and are by no means expensive, and exceedingly durable: and I will venture to assert that with these lamps no accident whatever can arise in any place, or under any circumstances, let the state of the mine be ever so deplorable, from fire-damp.

In the steam safety lamp, the atmospheric air of the coal-mine passes in a current through a tube, and is mixed with steam before it can possibly arrive at the light: by this means the fire-damp burns silently and steadily at the wick of the lamp alone, for any length of time. Should it exceed the due proportion of atmospheric air for supporting combustion, the light of course goes out; but in this lamp such an event will seldom happen. It has also the valuable quality of keeping cool throughout every part, and under all circumstances, by reason of the steam, which is constantly extricated and kept in motion within the lamp. And as steam is merely water 1800 times expanded, there is no cause to dread the want of a sufficient supply of this useful agent.

No current of air containing fire-damp, and suspending coal dust, gunpowder, or pyrites in powder, can do any mischief, which may at once be understood by a slight examination of the steam safety lamp, whilst the light is always uniform, steady, and bright. It is now well known that this lamp burns most brilliantly in an atmosphere of fire-damp after the wire-gauze lamps go out, and even after the original safety lamp has had the fire-damp exploded within it, as acknowledged by Messrs. Watkin and Wood, viewers, and Mr. Patterson, to have occurred not long since at the Engine Pit in this county. For without my safety lamps the workmen must have been left in darkness in that *well known* pit.

No lamp supplying itself with air can be considered as perfectly

secure without steam: and it is a curious circumstance that, as water was the medium of safety in the original lamp, so water in the form of steam has since become most useful for our present purpose, in affording a permanent light, wherever there is a sufficient quantity of oxygen to support combustion.

My steam safety lamps have been much used in the Herrington Mill Pit and the Engine Pit, where their value is duly appreciated.

I beg it may be understood that my letter, No. V. at p. 130 of Mr. Holmes' book on Coal Mines, was a *private communication*, and never intended to meet the public eye; but, by a mistake, it was inserted unknown to me, for which I am truly sorry, as I never intended to wound the feelings of any individual. Mr. Holmes of his own accord has expressed his determination to leave this letter out in the next edition of his work.

Upon the 6th of this month I had the honour to receive spontaneosuly and unanimously the thanks of the Society for preventing Accidents in Coal Mines, for my general services.

From the increasing confidence with which my steam safety lamps arereceived by those Gentlemen who are concerned in the management of coal mines, I have reason to expect that in a short time they will be universally employed.

*The Steam Safety Lamp.* (See Plate LVIII.)

Fig. 3, *a* represents the short tube, by which the air enters into the tube, *b*, and this tube supports the water cistern, *c*, at the top, being fitted into the tube *a*, at the bottom, so as to be taken out and replaced when the water is to be put into or removed from the cistern, *c*. The air which ascends the tube, *b*, mixes with the steam of the water cistern, and passes down the two lateral tubes, *d*, *d*, to support the combustion of the flame, and afterwards ascends by the sides of the cistern through the chimney of the lamp. The perforations are useful in retarding the progress of the fire-damp, that it may be properly mixed with the steam before it arrives at the flame; *e*, the bottom fitted air tight; *f*, the glass; and *g*, the oil lamp. These lamps are 12 inches in height, exclusive of the chimney, five inches in diameter, are very convenient and portable, and may be made by any tinman for 12*s*.

Fig. 4 represents a conducting tube which is to be put on at *a*, when the lamp is to be carried from one place to another.

Fig. 5 is another conducting tube, to be used with the aperture downwards when descending a shaft or staple, and it may be reversed when ascending.

These lamps should be cleared of water, &c. and well dried after they have been in use, that they may be rendered thereby more durable. When the lamp is first lighted, it is needful to establish a current, which is best effected by turning the lamp so that the tube *a* may receive the advantage of the current of air whichever way it blows: this will be effected in five minutes, and the lamp will afterwards continue to burn regularly and uniformly.

Fig. 4 represents the gas light lamp, which is intended, and has been used, for burning the fire-damp in a coal-mine, in lieu of oil, and thereby in a *board* infested with fire-damp, or at a *blower* of tolerable magnitude, it will be the means of expending, most usefully, that hitherto noxious gas. These gas light lamps may be constructed of any size and power, and in any number, where a volume of fire-damp is to be destroyed. This lamp is similar to the steam lamp just described, except at the bottom, where at *h* is a tube which passes through the bottom into the vessel, *i*, which contains oil or water. *j* represents the oil lamp burning as in the steam safety lamp; *k*, the three small tubes for burning the fire-damp; *l*, the three jets of the fire-damp burning very briskly: but when the fire-damp is to be burnt, the oil lamp is of course extinguished. The bottom of this lamp may be turned round so that the tube, *h*, through which the fire-damp is to be driven, may stand in any direction required.

With this lamp I have frequently burned fire-damp in a diversity of ways in an explosive atmosphere, with perfect safety.

*Bishopwearmouth, Aug.* 20, 1816.

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## Article VII.

*Some Observations respecting the new Metals obtained from* Barytes *and* Strontian; *also of a pure Metal observed in the Decomposition of* Boron; *together with other Remarks on the Means of Analysis afforded by burning a highly compressed Mixture of the Gaseous Constituents of* Water. In a Letter to the Editor from the Rev. Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge.

(To Dr. Thomson.)

SIR,

The interest you have been pleased to express in the success which has attended my analytical researches, since I adapted an explosive mixture of *hydrogen* and *oxygen* to aid the operations of the *blow-pipe*, induces me to offer for the consideration of your readers the further result of my experiments. A communication which I made to the Editor of the Journal of the Royal Institution, since printed in the third number of that publication, will render unnecessary any repetition, at present, with regard to the apparatus necessary for these experiments. Your own valuable suggestions, made with a view to facilitate my inquiries, have tended towards one of the most remarkable results which I have to mention.

When you informed me that you had received the *metals* of *barytes* and *strontian*, which I sent to you in *naptha*, and had submitted them to the proper tests, whereby you were enabled to con-

firm the truth of my previous account of those *metals*, you added, that you recommended "a trial of the *nitrate of strontian* instead of the *earth* in the state of powder." For this purpose Mr. Holme accompanied me to my laboratory; but we began with the *nitrate of barytes*, the more fusible body of the two. I prepared this substance by dissolving the *carbonate of barytes* in *nitric* acid, filtering the solution, and evaporating to dryness. After this I placed some of the *salt*, already becoming moist, from its deliquescence, within a cavity, at the end of a stick of *charcoal*; and exposed it to the flame of the ignited gas. As it became fused, it began to boil, with vehement ebullition: at this moment, *metallic* globules were clearly discernible, in the midst of the boiling fluid, suddenly forming, and as suddenly disappearing. Mr. Holme now requested that the operation might be checked, in order to examine the result of this fusion. To our amazement, the interior surface of the *charcoal*, within the cavity, appeared studded over with innumerable globules of a pure *metal* of the most brilliant lustre and whiteness; resembling the appearance exhibited by globules of *mercury*; or of the purest *platinum*, after fusion. We repeated the experiment often with the same result; although not always with the same success. Sometimes the globules were not visible; in which case we supposed that we had continued the heat too long; as dense white fumes, escaping, denoted the volatilization of the *metal*; like that of *silver*, and of many other *metals* after undergoing the same temperature. These globules were exceedingly minute; although no *metal* with which we are acquainted ever exhibited a greater degree of splendour, or a more brilliant whiteness. Mr. Holme succeeded in detaching two of these globules; which we placed in *naptha*; and they are now sent for your inspection. No file is here requisite, for developing a surface with *metallic* lustre; because the *metal* is in its purest state. It is the same *metal* for which I proposed the appellation of *Plutonium*; and I am glad to find the appellation generally approved; because, being among the *lighter* metals, we should have rendered ourselves liable to merited sarcasm if we had named it from a derivative of βαρυς, signifying heavy. Some other of the globules we caused to fall from the *charcoal* into water; the consequence was, such a rapid decomposition of the *water* that the *hydrogen* flew up in a continued stream; and we expected to see it take fire; but this did not happen.

We now made trial of the *nitrate of strontian*. Deliquescence not having commenced, as in the preceding experiment with the *nitrate of barytes*, we mixed with it a little water, so as to form a paste, and proceeded according to our former plan. It proved to be almost infusible: the upper surface exhibited very little alteration; but when the entire mass, now desiccated and coherent, was removed from the *charcoal*, we found that the lower surface had been imperfectly reduced to the *metallic* state, and admitted the action of the file. We then renewed this experiment; making use of *charcoal*, in powder, mixed with the *salt*; but did not succeed in

obtaining any globules of *strontium* like those of *plutonium*. We next mixed the *nitrate of strontian* with *oil*, and obtained *strontium* in the metallic state, after aiding the fusion with a little *borax*; but no globules of pure *metal* were apparent.

These experiments, as it must be evident to you, were made in consequence of your own suggestion. You also mentioned the probability of my "decomposing the *boracic acid*, and procuring *boron*; recommending a trial of *calcined borax* mixed with a little *charcoal*." Having by me some *calcined borax* prepared in a *platinum* crucible by my lamented friend the late Professor Tennant, I reduced this to powder, and mixed with it a small quantity of *charcoal*; adding also *water*, and rubbing the whole together in a porcelain mortar. The *water* was then evaporated, by heating the mortar, to dryness; and the mixture taken out in a coherent mass. Some of this being placed within a cavity in *charcoal*, as before, was exposed to the ignited gas, and readily fused. When it began to boil, the full current of the gas was gradually let loose upon it; and the utmost intensity of the heat communicated. A dense white fume began to be exhaled, the common indication of *volatilization* in *metallic* bodies; perceiving which, the valve was turned, and the flame extinguished. We now examined the surface of the *charcoal*; it was partly covered with a pitchy *black* looking substance, which by the action of the air soon became *white*, and presently exhibited the very remarkable appearances of innumerable aggregated crystals glittering in the sun's rays, and looking like snow-white *pearl-spar*. These crystals are of course those of the *boracic acid*; forming upon the black oxide of its *metallic* base, which has been called *boron*. That this base is undoubtedly *metallic*, will appear by the sequel; for, upon renewing the experiment, there appeared among the *boron* as brilliant a *metallic* globule as any of those of *plutonium* to which I have so recently alluded. I showed this globule to Mr. Holme; but, not having the smallest expectation of such a result, I suspected that it was either a globule of *platinum* or of *nickel*, adhering to a mass of *charcoal*, before used. That this, however, could not have been the case, is made evident both in the fact of my having that day selected several sticks of fresh *charcoal* for my experiments, and also having cut off at least an inch from the extremity of that piece which I had used for a support. I have been the more particular in stating these trivial circumstances, because, unfortunately, expecting that more globules would appear by a subsequent application of the flame, I exposed again the whole result to the ignited gas; and the metal became dissipated in consequence of the heat. Herein consists one of the few difficulties which persons who use the same apparatus will have to encounter. The theory of the reduction, whether of *borax* to its base *boracium*, or of the *earths* to their *metallic* state, is thus ingeniously explained by Mr. Holme: "the whole is entirely due to the attraction, which, at such an exalted temperature, *hydrogen* has for *oxygen*;" the reduction is, therefore, often instantaneous; but if the heat be continued a

single instant beyond the point of reduction, volatilization instantly ensues; and the pure *metals*, which existed in the form of such minute globules, disappear. The temperature is so extraordinary, that, having no means of measure whereby its limit may be ascertained, an idea can alone be formed of it from its effects. In a former publication I stated that the drops of *platinum*, which fell during the fusion and combustion of *platinum* wire, weighed *five* grains; but they are often much greater; the melted metal drops in such a liquid state that after a fall of about an inch and a half perpendicular height it divides, like globules of *mercury*, and runs about the table. Mr. Harrison, a distinguished Member of this University, now of the Royal Institution, was with me when we collected some of these drops weighing *six* grains: he considered the heat of the ignited gas as being much greater than that of any *galvanic* battery.

I shall now state, as briefly as possible, a few other trials to which I have submitted some very refractory substances, by means of the same powerful apparatus; that is to say, by mixing and condensing two parts, by bulk, of *hydrogen*, added to one part of *oxygen*, in one of Newman's blow-pipes; igniting the gaseous mixture at the extremity of a thermometer tube adapted to the same. If an excess on either part be admitted, it is better that it should preponderate on the part of the *hydrogen*; because this, owing to its extreme volatility, is apt to escape; and the smallest loss of *hydrogen*, by altering the proportion between the gases, deteriorates the mixture; causing it to become extinguished after *ignition*, when propelled in a full stream.

*Sept.* 19.—I fused *plutonium* with an alloy of *silver* and *gold* upon *charcoal*. The metals imperfectly combined. By continuance of the heat the *silver* began to escape in heavy dense white fumes; and also to burn, with a slight degree of scintillation.

*Sept.* 20.—I placed a bead of *plutonium* weighing one grain by the side of a bead of *platinum* of equal weight upon *charcoal*. The two *metals* were fused, and ran together into an alloy of a *bronze* colour weighing two grains. Observing that pure *platinum* still existed in the centre of this alloy, it was again brought before the ignited gas, when the *plutonium* began to burn with its *chrysolite* green flame. This alloy preserved its *metallic* form during 24 hours, when it fell into a *reddish* powder resembling the red oxide of *platinum*. *Plutonium*, if fused in contact with *platinum*, always tarnishes the latter; making it look like *brass*; and if alloyed with it, the colour is that of dark *bronze*.

*Sept.* 21.—Upon the surface of *palladium* from a former experiment there remained small beads of *plutonium*; which, when filed, possessed such silvery whiteness, that I believed these beads to exhibit an alloy of the two metals. However, after retaining their *metallic* lustre for 24 hours, they became oxided upon their surfaces; the *metallic* lustre being again developed by the renewed action of the file.

*Sept.* 23.—Formed an alloy of *plutonium* and *iron*; the colour jet black; and the alloy brittle.

*Sept.* 24.—Obtained *plutonium* from *barytes*, by fusing it, per se; this metal is *infusible* before the common blow-pipe; but it may be fused with *borax*; in which it dissolves like *barytes*; evolving all the while gaseous bubbles, and giving to *borax* its *chrysolite* green colour; but disclosing *metallic* lustre upon the action of the file, so long as a particle remains undissolved.

*Sept.* 25.—I made an alloy of *plutonium* with *copper*. This alloy is of a *vermilion* colour; but, when acted upon by the file, the *metallic part* exhibits the lustre and colour of *gold*. Before the common blow-pipe it is decomposed by means of *borax*; the *plutonium* dissolves, and a bead of pure *copper* is obtained.

*Silex*, which fuses with difficulty when in the form of *rock crystal*, became easily fused by the following process:—It was mixed with lamp oil, and made into a paste; this being placed upon *charcoal*. The fusion of the *silex* was now rapid; it ran into beads of various colours; sometimes these beads resembled *corundum* rubies; at other times they had a paler hue, or were of a *green* colour. When the *silex* was mixed with water they were *white* and limpid as *rock crystal*.

During the fusion of pure *silex*, innumerable gaseous bubbles are constantly escaping.

*Plutonium* placed in contact with a bead of *iron* and fused upon *charcoal*, forms an alloy with *iron* which when filed exhibits a *leaden* aspect. Externally this alloy is of a dark *bronze* colour. In this experiment the *plutonium*, in bulk, was double that of the *iron*.

*Sept.* 26.—Formed an alloy of *silicium* and *iron*; by fusing *silex* first into a bead, and then heating it in contact with *iron*. The alloy appeared as a black slag-like brittle substance; but it admitted the action of the file, and disclosed a *metallic* surface somewhat whiter than pure *iron*.

Proceeded in the same manner with *magnesia*. This *earth* is exceedingly difficult of fusion. It tinges flame of a pale red colour. By mixing it with oil, and cautiously confining it, it ran at last into a white opake glass; which being fused in contact with *iron*, intumesced, and exhibited an alloy of *magnesium* and *iron*, of a slag-like appearance, somewhat brittle; but admitting the action of the file; and disclosing *metallic* lustre.

These experiments tend to confirm an opinion of Sir H. Davy, that cast *iron* is made *malleable*, by expelling the *metals* of the *earths*; because, *vice versâ*, *malleable iron* became *brittle*, by being made to contain them.

*Sept.* 27.—In reducing *strontian* to the *metallic* state, I observed that the following process answers best:—

1. Mix the *earth* into a paste with lamp *oil*; it is almost infusible, *per se*, even before the ignited gas.

2. Place it within a cavity at the end of a stick of *charcoal*.

3. Suffer the ignited gas to act upon it until it coheres and is hard enough to be taken out in a mass.

4. Expose it without *charcoal* before the ignited gas; supported in *platinum*, or *iron* forceps; taking care that the forceps be not acted upon by the flame; until a partial fusion has taken place.

5. Place it again upon the *charcoal*, and assist the fusion by as little *borax* as possible. It will now become partially vitrified, and perhaps appear to be entirely so.

6. Expose this vitrified substance again by means of forceps, without *charcoal* to the flame; it will now melt into a jet black shining metal, which by the action of a file will disclose metallic lustre equal to that of polished *silver*.

During this last experiment, care should be used that the white fumes which escape sometimes while the *strontium* is forming, be not inhaled upon the lungs; they are acrid and suffocating.

*Sept.* 28.—Made an alloy of *platinum* and *silver*; which was so malleable that a large bead of it was extended by means of a hammer into a circular plate, without a single fracture towards the edges. This alloy is easily made before the ignited gas upon a piece of *charcoal*, and might prove very serviceable for *coinage*; owing to its incorruptibility and hardness. Its lustre when polished is equal to that of pure *silver*.

Exposed ten grains of *Kupfer nickel* upon *charcoal* before the ignited gas. At the first action of the flame the *arsenic* came off in copious fumes, and appeared in the *metallic* state boiling upon the ore. As soon as this had been expelled, finding that the residue was difficult of fusion, *borax* was added, and the whole force of the heat applied. A brilliant white metal having all the lustre of pure *silver* was now seen flowing from the ore. By continuance of the heat it began to be dissipated in white fumes. It was therefore taken out and examined. By filing it disclosed a most brilliant surface. It was also malleable; and became flattened by slight pressure. But by a smart blow from a hammer it separated and exhibited a splendid lamellar structure. Before the common blowpipe it still diffused a slight smell of *arsenic*, and was fusible until this was driven off. It gave colour to *borax*; first *green*, afterwards a *brownish red*; the *first* of these colours it exhibited to the *prussiate of potash*, after solution in *muriatic acid*; the *second*, after solution in *nitric acid*.

My last experiments in the month of September were made on the 30th, upon crystals of *siliceo-calcareous titanium*. I kept one of these exposed to the most intense heat of the apparatus until I had exhausted a bladder containing five pints of the gaseous mixture. I then added *borax*. The metal appeared boiling and flowing about upon the *charcoal* used as a support. When taken out, it exhibited a reddish mass; and this, examined with a lens, appeared to consist entirely of *spiculæ*, which were minute needle crystals crossing each other like net-work, in all directions. In some parts

the *spiculæ* stuck out like hairs. This was evidently an *oxide of titanium*; answering to the appearance frequently exhibited in *rock crystal*, called by the French mineralogists "*Cheveux de Venus.*" The colour of the pure metal is not red, although it has been often so described in books of chemistry. Upon bringing this substance before the ignited gas without *charcoal*, it was reduced to a pure *metal*; and this metal, with a black surface, exhibits after being filed the lustre and colour of pure polished *iron*, and is perhaps as malleable.

I have now brought my observations to their conclusion. In the beginning of this letter I have stated the result of my experiments during the present month. Having been occupied, during almost a quarter of a year, in unwearied attention to the effects produced by the most powerful engine which *chemistry* ever yet employed, I cannot take my leave of the subject without congratulating every lover of the science upon the means which it will put into his hands. To use your own language, "this new method of analysis will facilitate the production of various new bodies hitherto obtained with considerable difficulty; and while the experiments now made show the small confidence that can be placed in analogical reasoning in chemistry, they have brought to light a new series of important facts." The conjecture of Lavoisier, and the death-bed declaration of Pelletier, confided so many years ago to his friend Dolomieu, have now been realised.* "*La baryte pourroit bien être d'une nature metallique*—and when we add the marvellous fact that all the other *earths* seem to be susceptible of a similar reduction; that the *metal* of *strontian* is before our eyes; and that *boron* has also exhibited a *metalline* base; this truth becomes almost an axiom; that all the constituents of created nature are *combustible*; and although it be not permitted to the *chemist*, "who finds his endeavours to comprehend the works of *creation* checked at every turn,"† to pry into "His ways, which are past finding out," yet, as a philosopher, he will probably call to mind the remarkable prediction which ordains, for the final dissolution of the material world, that "THE EARTH, AND THE WORKS THAT ARE THEREIN, SHALL BE BURNT UP."

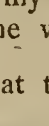
I have the honour to be, &c. &c.

*Cambridge, Oct.* 5, 1816. EDWARD DANIEL CLARKE.

[P. S. I consider it as necessary, for the sake of my chemical readers, who will doubtless feel anxious to repeat the curious and important experiments of Dr. Clarke, to insert here the following paragraph of a letter which I have received from him since this paper was written.—T.]

* Haüy, Traité de Mineralogie, tom. ii. p. 217. Paris, 1801.

† Watson's Chemical Essays, vol. i. p. 89. Cambridge, 1781.

This morning (Oct. 7) I had been exhibiting the reduction of *barytes* to some chemical friends; and the gas being exhausted, I filled the reservoir as usual. I had some *nitrate of barytes* upon *charcoal*, but as it was in a very deliquescent state, I wished to let as little of the gas ignite as possible, that it might not be blown away by too powerful a blast. I had therefore my left hand upon the copper box, slowly turning the handle of the valve until it was at about this angle with the tube  : at this moment, without any apparent retrograde motion of the flame, although I had a tube of four inches in length, the whole of the highly condensed gas exploded. A piece of the plate copper was driven between me and my servant, and struck with the force of a cannon shot against the chimney at the end of the room—all the rest of the copper was torn and distorted—the stop-cock, piston, and bladder, were blown into the air.

I shall also be obliged to you to add a note to the paper I sent, respecting the precipitate afforded by *Plutonium* in *nitric* and in *muriatic acids*, to the *prussiate of potash*. I made experiments with acids perfectly pure, with distilled water to dilute them, and with crystallized *prussiate of potash*. First I tried the acids without the metal—there was no colour produced by the *prussiate of potash*. When the metal was added, the colour of the precipitate in *muriatic* acid was *emerald green*; and in *nitric* acid it was also green, but of a different hue; like *chrysolite green*, or rather darker, inclining to brown.

Very faithfully yours,

E. D. Clarke.

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## Article VIII.

### *Account of the late Earthquake in Scotland.*

By Thomas Lauder Dick, Esq.

(To Dr. Thomson.)

SIR, *Relugas, Sept.* 21, 1816.

I believe the public are, ere this, informed by the newspapers of most of the particulars of the late earthquake in Scotland; but these happy realms are so rarely disturbed by similar convulsions, that you will probably consider it as an event worthy of being registered in the more permanent pages of your Journal. With this idea I now offer you the following detail, partly drawn up from my own observations, and private information received from different places; and partly from the notices which have appeared in the public

prints. I beg you, however, to understand, that I shall be happy to resign my claim to your attention, in favour of any paper on the same subject, which may reach you from a more intelligent quarter.

Although earthquakes have been at no period very frequent in these kingdoms, yet instances of them have not been altogether wanting, as we learn from some of our older writers, as well as from the more distinct and better authenticated accounts of those which have happened in more modern times. But happily the shocks have been always comparatively of the mildest nature; and even the late instance, though remarkable for its greater intensity, when compared with those recorded as having hitherto affected Great Britain, would have probably passed almost unnoticed in other countries, where similar phenomena are more common and more terrible. Perhaps a short enumeration of some of those terrestrial convulsions which have agitated this empire at different times, without attempting any complete catalogue, may not be unacceptable as a preliminary part of this paper.

An old manuscript chronicle, written by several citizens of Perth, mentions that, "on July 23, 1597, there was a great earthquake in sundry places and parts of this realm."

On Nov. 8, 1608, about nine o'clock in the evening, the people of Aberdeen were dreadfully alarmed by an earthquake, which, being considered as a manifestation of God's wrath against the city for the sins of its inhabitants, the magistrates and clergy ordered the next day to be set apart for fasting and humiliation. It is curious enough to observe that the sin of which the bishop seems most particularly to have complained, and against which he conceived the wrath of the Almighty to have been more immediately directed, was that of fishing for salmon on Sunday: and accordingly the proprietors of salmon fishings being called before the Session (an ecclesiastical court), "some promist," says its register, "absolutely to forbear, both bi themselffs and their servands in tyme cuming; others promeist to forbear upon the condition subscreyvant; and some plainlie refussit any way to forbear; and sum were not thoroughlie resolved."

The last shocks of earthquake felt in London were those on Feb. 8, and March 8, 1749. These, however, seem to have had no other effect than that of frightening the inhabitants.

On Sept. 30, 1750, an earthquake was felt in various parts of England; the centre of its action seemed to be about Daventry, in Northamptonshire.

The dreadful earthquake of Lisbon, Nov. 1, 1755, was felt in some degree over a great part of Europe. In these islands it produced very sensible effects. At Beelsborough, in Derbyshire, a great noise was heard; and a large body of water, called Pibley Dam, rose and fell in a most wonderful manner. A similar noise was heard, and a like effect produced, in a canal at Busbridge, in Surrey; and in a pond at Cobham, in the same county. At Dunstal,

in Sussex, the water of a pond rose gradually for some minutes into the form of a pyramid, and fell suddenly down again, like a water-spout. At Eaton Bridge, in Kent, the water of a pond opened in the middle, so as almost to show the bottom. At Shirebourne Castle, in Oxfordshire, the water of a moat was much affected. At Whiterock, Glamorganshire, the tide rushed in with great violence, driving the vessels before it, and almost oversetting them. At Kinsale, in Ireland, there was a tremendous rush of water into the harbour, which did much damage to the shipping. At the same time, in Scotland, Loch Lomond rose and fell violently; and a large stone, lying a considerable way within the lake, but in shallow water, was forced out of its place, and driven upon the dry land, leaving a deep furrow to point out the track in which it had been moved. Lochs Ness and Oich also underwent similar agitations to those observed in Loch Lomond.

Some time about the year 1763 a shock of earthquake was felt in the parish of Logierait, in Perthshire. Its direction was from E. to W., and its duration was about a second or two.

About the year 1776 a slight shock of earthquake seems to have been experienced at Inverness.

In 1777 there was an earthquake at Manchester, the sensation of which extended 140 miles. The bells were heard to toll twice. In this case the rumbling noise was loudest, and most distinct, in the vicinity of conductors of electricity.

During the Calabrian earthquakes of 1782, the barometer in Scotland sank within a tenth of an inch of the bottom of the scale, and the waters of many of the Highland lakes were much agitated.

On Sept. 12, 1784, about nine o'clock in the morning, the air being perfectly calm, the water at the east end of Loch Tay ebbed about 300 feet, leaving so much of its channel dry. It then gradually accumulated, and rolled on about 300 feet further to the westward, where, meeting a similar wave rolling in a contrary direction, both united, rose to a perpendicular height of five or six feet, producing a white foam on the top. The water then took a lateral direction southward, rushing to the shore, and rising upon it four feet beyond the highest water-mark. It then returned, and continued to ebb and flow every seven minutes for two hours; the waves gradually diminishing every time they reached the shore, until the whole was quiescent. The same appearance occurred every day for an entire week, but with less violence, and at a later hour.

On March 11, 1785, the river Tiviot, in Scotland, suddenly disappeared, and left its channel dry for two hours, when it again began to flow with its usual current.

On June 16, 1786, a smart shock of earthquake was felt at Whitehaven, in Cumberland, which extended to the south-west parts of Scotland, to the Isle of Man, and even to Dublin.

On Aug. 11, 1786, a very alarming shock of an earthquake was

felt, about two o'clock in the morning, in Northumberland and Cumberland; and in Scotland it extended across the island, and as far north as Argyleshire. The extent of this shock was above 150 miles from south to north, and 100 miles from east to west; and it was every where felt at the same moment.

On Jan. 25, 1787, the river Tiviot again became suddenly dry, and continued so for four hours, when it again began to run as usual.

On Jan. 26, 1787, about ten o'clock in the morning, a smart shock of an earthquake was felt in the parishes of Campsie and Strathblane, ten miles north of Glasgow. A rushing noise from the south-east preceded the shock. The night before, a piece of ground with a mill on it, near Alloa, suddenly sank about a foot and a half. On the same day with this earthquake, the river Clyde, above Lanark, became almost dry for two hours, so as to stop the mills.

On July 8, 1788, an earthquake was felt in the Isle of Man; and on the same day the sea at Dunbar suddenly receded.

On Sept. 30, 1782, the same day with the earthquake at Borgo di San Sepolchro, in Italy, three distinct shocks of earthquake were felt at the house of Parsons Green, on the north side of the hill of Arthur Seat, near Edinburgh.

During the autumn of 1789, the inhabitants of Glenlednaig, in the parish of Comrie, in Perthshire, were alarmed by several smart shocks of earthquake; and on Thursday, Nov. 5, of the same year, a smart shock of earthquake was felt, between five and six o'clock in the evening, at Crieff and Comrie, and for many miles round that district. At Lawers House a rumbling noise, like that of distant thunder, had been heard at intervals for two months; and at the time of the shock, a noise like the discharge of distant artillery was distinctly heard. The house was shaken as if its foundation had been struck by an immense mallet. At the village of Comrie, the inhabitants left their houses, and ran to the open fields. On the forenoon of Nov. 11, of the same year, a still more violent shock was felt in the same place, accompanied with a hollow rumbling noise; and the ice on a piece of water near the house of Lawers was shivered to atoms.

On Nov. 10, 1792, three repeated smart shocks of earthquake, accompanied by a noise like that of distant thunder, were felt on the banks of Loch Rannach, in Perthshire.

In 1792, the neighbourhood of Comrie, in Perthshire, was again at different times disturbed by several smart shocks of earthquake, much more distinctly sensible and alarming than any previously experienced in that quarter.

On Jan. 17, and on Feb. 24, 1799, earthquakes were again felt at and near Comrie. The motion of the earth was from west to east. The shocks lasted about two seconds, but the subterraneous noise lasted much longer.

About the year 1800, another slight shock of earthquake seems to have been felt at Inverness.

On Sept. 7, 1801, a smart shock of earthquake was distinctly felt, at six o'clock in the morning, at Edinburgh, Leith, and the neighbourhood. It lasted for two or three seconds, but was unaccompanied by any noise. The sensation within doors seemed as if the house had been gently lifted upwards, and then violently shaken in a direction from north to south. On this day the gable of an old barn near Edinburgh fell in upon some reapers, crushing two women to death, and dreadfully bruising a third. A large tenement in Paterson's Court also sank so considerably as to render the immediate abandonment of it by its inhabitants, and its ultimate condemnation by the magistrates, necessary for safety. It appears, however, to have been doubtful, whether these accidents were casual, or whether they originated from the earthquake. The centre of this shock seems to have been at Crieff and Comrie. To the west it was felt at Loch Earn Head, Killin, Tynedrum, and Glenfinlas, extending southwards to Glasgow. It ran down towards Perth, and was sensibly felt at Callendar, and on both sides of the Forth at Grangemouth, Toryburn, Culross, Dunfermline, &c. It is somewhat remarkable that, although it was distinctly felt in the New Town of Edinburgh, it was not at all perceived in the Old Town, or to the south of it.

This list might no doubt be very much increased by persons having better opportunities of research than I can at present command; and it is even not unlikely (though I am not aware of any) that some sensations may have been experienced in Great Britain between the shock of 1801 and that which occurred lately, and which has been proposed as the object of this paper.

This earthquake took place on the evening of Aug. 13 last, at a quarter before eleven o'clock: such at least is the precise time in which most of the evidences agree; for a variation of the clocks and watches of different places have led to a corresponding variation as to the time of the commencement of the phenomena, to the extent of nearly half an hour between the extremes of all the different accounts. But there is nothing in any of these to lead us to suppose that the shock was not every where perfectly simultaneous. As it appears to have been infinitely more violent, so its influence seems to have been of wider extent, than that of any of those which have hitherto agitated Great Britain. Taking as our guides the names of those places from which there have been certain accounts of its having produced some kind of sensation; and in order to mark the surrounding outline of the field of its action on the map, we should begin at the Pentland Firth, and, tracing round the coast to the westward and south, we should touch at Gairloch and Loch Carron; and proceeding by Loch Lochy south-eastwards to Glasgow, and afterwards to the river Tweed at Coldstream, we should return northwards by Edinburgh and Leith, and so along the

whole eastern coast of Scotland to the point from whence we at first set out. Perhaps, indeed, the shock may have been sensible at points even without this line, to the westward and south, but such is the result of an inspection of the various notices which I have seen. The two diameters of the space enclosed by this extensive boundary, within which its effects were manifested in a greater or lesser degree, may at a rough calculation be supposed to measure, the one about 240 miles from north to south, and the other about 160 miles from east to west. But as the sensation appeared to have been but slightly felt at Glasgow, Coldstream, and Edinburgh; and as it hardly affected that portion of Scotland where these towns are situated, we must confine the range of its more active exertion to the country between the Tay and the Pentland Firth. All the different details seem to agree that the direction of the concussion was from north-west to south-east. But there is not the same uniformity of opinion as to the duration of the phenomena; some accounts making it only one or two seconds, whilst others are of opinion that it lasted as many minutes. I have no doubt that as difference of situation might produce a very considerable difference in the intensity, so it might also in the continuance of the vibration. Yet after mature reflection, and a due comparison of the various testimonies, I am inclined to think that the whole of the phenomena attending it, could not any where have occupied a much greater portion of time than one minute. Although its violence was differently modified in different situations, and those too at no great distance from each other, yet its grandest efforts were unquestionably exerted immediately under the town of Inverness; from which point as a centre, speaking generally, its force seemed to diminish, with the exception of some anomalies, in every direction towards the line of circumference already defined. But your readers will form a better idea of the nature and strength of its operations, from an exhibition of the substance of some of the more prominent and interesting accounts given by observers in different quarters, which I shall proceed to lay before them in succession, after describing in the first place what were my own personal observations and feelings on the occasion.

The situation of this place is a rocky peninsula, composed almost entirely of gneiss, between the rapid and deep bedded rivers Findhorn and Divie; the latter describing nearly three-fourths of a circle round the site of the house, which stands on a thick bed of gravel lying over the subjacent rock; isolated on every side by the ground declining from it, excepting on the east, where a little hill rises over it. The house itself consists of two stories, rising directly from the ground, being built in the villa style. I am thus particular, because I conceive that a knowledge of the various situations, and a comparison of the variety of intensity of shock at different places, might possibly tend to give some little aid in throwing light on the theory of earthquakes. At the time of the convulsion, my family were variously disposed of. I happened to be sitting at a

table in a room on the ground floor, in the north-western angle of the building, busily employed, when in an instant I was alarmed by a tremendous rumbling noise, which I can alone compare to that occasioned by a number of cannon and artillery waggons being driven furiously over the pavement of a vaulted court-yard. As the house stands perfectly solitary, and surrounded by a large open space of dressed gravel, my astonishment, and that of a lady who was in the room with me, was very considerable; nor was it diminished by the otherwise extreme calmness and silence of the night. This noise lasted, so as to afford us sufficient time to offer several conjectures to one another about the cause; when in a moment it appeared to be suddenly transferred from the gravel to the floors of the rooms above our heads. The doors, windows, and furniture of every kind in the house, seemed to be instantaneously and violently assailed, clashing and rattling tremendously. The whole fabric of the mansion now began to shake from its foundation; and the floor, and the chair on which I sat, were several times moved powerfully up and down in quick succession, whilst along with this vertical motion I felt the chair rapidly agitated backwards and forwards horizontally, as if some herculean person had taken it up with both hands from behind, and shaken it with violence. Of this compound motion I was perfectly sensible. The table and the lamp at which I was employed seemed to dance before my eyes; and, upon reflection, I distinctly heard the bells in the kitchen ring very loudly, though at the time I very naturally attributed their sound to their having been pulled by a lady who had retired to her bed-room two or three minutes before, and whose alarm I conceived had led her to have recourse to them. I had no sooner felt the first impulse of the tremulous motion, than I exclaimed that it was an earthquake, and immediately took out my watch, in order to mark the time of its occurrence and duration; but as it did not tell the seconds, I must confess that an inspection of it could only enable me to form a rude guess as to the latter point. The lady who was by me had twice experienced earthquakes on the Continent, and immediately agreed with me in attributing the sensations we then felt to that cause. The whole house was now in confusion, and exclamations of wonder and alarm were heard from every quarter of it. I went into the lobby; where, to my very great surprise, although all the more sensible effects were already over, I remarked, that such of the doors of the upper rooms entering from the landing-place as happened to be open were moving pretty rapidly backwards and forwards, creaking upon their hinges; and this motion they continued to exhibit for a considerable time. Two maid servants who lay at the top of the back part of the house were awakened by the noise from the deep sleep generally ensured by labour. One of them thought that there were people murdering her companion, and lay trembling in silent horror till the violent movement of the floor and their beds made both of them jump out, and run down stairs undressed as they were, pale as corpses, and shrieking with alarm,

being firmly persuaded that it was the Day of Judgment. A young gentleman who had just gone to rest, in another part of the house, called out that his bed and the room were moving with him; and he afterwards described that he had been turned first over upon one side, and then upon the other, for several times alternately. A man servant who sleeps in a paved room on the ground floor, at the back part, or north-eastern angle, of the house, having extinguished his candle to go to bed, just as the shock commenced, was heaved up and down, and to and fro, in the same way as I was in the north-western angle. In the nursery there were two maids asleep with the children. It is situated in the south-eastern angle of the house, where the sensation of the shock was certainly much less than in the northern and western parts of it. But even there it was very sensible. One of the girls was awakened by it; and attributing the motion to that fancied feeling which sickness sometimes induces, she became very miserable, supposing that she had been suddenly taken dangerously ill. It was odd enough that, although the other girl did not awake, her companion heard her distinctly exclaim in her sleep, in a tone of great distress, that she had thrown down all the tea-things. During the extreme violence of the shock, the rocking was such that I felt firmly persuaded it was quite impossible the house could stand it much longer. But I am since perfectly satisfied, from the trifling effects it produced, even on the kitchen utensils, not one of which seemed to have been deranged, that a well compacted building would withstand a great deal more. By comparing in my mind the length of the conversation that passed during the continuance of the phenomena, I am inclined to think that I at first estimated the time of the duration of the violent part of the concussion, at rather more than the truth. But I have no hesitation in saying that, from the commencement of the rumbling noise, till the doors of the room ceased to vibrate, considerably more than a minute must have elapsed; and the more violent part of the shock certainly lasted not much less than 10 or 15 seconds. The night was clear, and the moon bright. The barometer, which was about 29·20, did not appear to be affected; Fahrenheit's thermometer stood at 54 degrees. Although the whole summer had been very wet and stormy, the previous day, and particularly the evening, had been remarkably fine and still; and towards sun-set the sky was serene, with light clouds stretching in horizontal lines across it; and if I rightly understand Mr. Howard's nomenclature of clouds, it might have been perhaps defined by the meteorological term *Cirrus*. The shock was followed by the same stillness which had preceded it. The following morning was calm, but gloomy; and a thick rain came on, which continued to fall incessantly for above 60 hours; and, indeed, until within these few days, we have hardly had any thing like fair weather.

In the houses of all the neighbouring families the same sensations were felt in a greater or lesser degree; and in almost all of them the furniture rattled, the bells were set aringing, and the rooms and

beds shook. Terror and alarm were every where universal. One gentleman, who was asleep in bed at the moment, having been suddenly awakened by it, imagined he had been seized in the night with palsy, and was afraid to try to move hand or foot for some time after the shock, lest he should be convinced of the melancholy truth by his inability to do so. In most of the cottages, also, throughout the neighbourhood, it was distinctly sensible, and their inhabitants were awakened, and in many places dreadfully alarmed. Although I am inclined to think, from what I have heard, that low built houses were not in general so much affected as loftier ones, yet I have been informed of one instance in which the family in the more spacious house of a gentleman were perfectly unconscious of it, whilst the cottage of his gardener, standing within a few yards of the larger mansion, was most violently affected. I have also heard of some cottages where the plates, &c. were absolutely shaken down from the wooden shelves. One farmer felt the shock very intensely in his cottage; whilst his son, who was returning home on horse-back, and within a few paces of the house at the moment, was not in the least aware of it. This might probably arise from the motion of the horse, which, though he felt the agitation of the earthquake himself, would not communicate any unusual feeling to his rider. As a proof of this, one gentleman who happened to be riding was surprised by his horse suddenly starting in great alarm, and found much difficulty in getting him to move forwards. This was afterwards accounted for by his discovering that it occurred at the very time of the shock. The coachman, passengers, and guard, of the mail-coach, which was on the turnpike road to Forres at the time, were not in the least conscious of any thing extraordinary. I had a very distinct account, from a man who chanced to be travelling on foot in the mountains near Loch-an-Dorb, 10 miles to the south of this house. He said he was first alarmed by a sudden and tremendous noise of a rushing wind, which came sweeping up the hills like a roar of water. This was instantly followed by the rumbling sound, or *rhombo*, as the Italians emphatically call it, and the ground was then sensibly heaved up and down under his feet. The morning after the earthquake I was particularly careful in examining the surface of the ground in various places; but I could not discover the least vestige of a crack; not even in such spots in the garden, or shrubbery, where the surface, having been carefully raked, must have at once betrayed the most trifling appearance of separation in the soil.

In the neighbouring towns of Nairn, Forres, Findhorn, Elgin, Fochabers, Grantoun, &c. the shock produced similar sensations and appearances to those we experienced here. The bells rung; doors were opened; and various pieces of furniture were visibly shaken against the walls. The fire-irons clashed; and glasses were jingled against one another. Dogs howled; and poultry on the roost manifested the greatest dismay. Many small birds, such as linnets and canaries, were thrown down into the bottom of their

cages, and some of them actually killed. A parcel of pea-fowls belonging to a family in Nairnshire, were so much frightened, that they continued screaming the whole night afterwards.

At Inverness, which was certainly the focus of its action, the earthquake not only produced the most violent effects, but also created the greatest alarm. In the article from that town the convulsion is distinctly stated to have lasted about 20 seconds, and to have been really very tremendous. The bells in many houses rung for more than a minute, and several of the inhabitants who had retired to rest were fairly tossed out of bed. The concussion on the houses was dreadful; and such was the terror it inspired, that they were all in one moment evacuated. Infants were torn from the cradle; and men, women, and children, of all ages and ranks, many of them just as they had risen from their beds, and almost naked, were seen rushing into the streets, which were instantly filled with the most doleful female shrieks and lamentations. Under the dreadful apprehension of a second and more violent shock, which might perhaps bury them under the ruins of their houses, the motley and terror-struck groups of inhabitants crowded in various streams through the different outlets leading towards the country, where many of them remained all night in the fields. Partly from fear, and partly from curiosity, few I believe occupied their homes or their beds until day-break, and many did not return to them till next evening. By fortunate accident, the streets had been almost deserted on the night of the earthquake, and before the shock, at an unusually early hour; and it was equally lucky that the violence of the concussion was in a great measure over before the people had time to crowd into them again; for so very thick was the shower of large stones which were precipitated from the chimney tops, as well as of slates and tiles, which were shaken in great numbers from the roofs of the houses, that, if the streets had not been empty, many deaths and dreadful accidents must have occurred. The thundering noise made by the stones in falling added to the other horrors of the night; many of them were projected completely across to the opposite side of the way. It is rather remarkable, that it was chiefly from the newer houses that the stones were thus thrown; many of the older ones having entirely escaped this dilapidation. It was not, however, until the morning's light that the most decisive proof of the violence of the shock was displayed. No sooner had day dawned than the beautiful spire which is attached to the county jail was observed to have been rent through at the distance of several feet from the top; and the part which was above the fracture appeared twisted round several inches in a direction towards the north-west. This circumstance appears to be very satisfactorily accounted for by a gentleman at Inverness, who remarks, that "the motion of the undulation towards the south-east being communicated to the lower sooner than to the higher parts of the building, those parts of the latter whose cohesion was not sufficiently strong would naturally be left behind, and projected in a

north-west direction." It is not impossible, however, that electricity, which, if not sometimes the cause of the sensation of earthquake, at least appears very generally to accompany such convulsions, might have had some share in producing this injury. Notwithstanding its vicinity to Inverness, and although it was agitated during the great earthquake of Lisbon, yet there is no account of Loch Ness having been affected on the late occasion. But it is not unlikely that it may have displayed some commotion, though from the lateness of the hour it would necessarily escape observation. Three gentlemen who at the time of the earthquake happened to be approaching Inverness from the west, when at a considerable distance from the town, distinctly heard the large bell toll twice. This circumstance was entirely unnoticed by those who were in the streets or houses of the place; people of every description having been too much alarmed, and too much occupied in providing for the safety of themselves and their families, to remark it. It appears to have been admitted by many gentlemen of Inverness, who had resided long in foreign countries, particularly in the West Indies, where such convulsions are very frequent, that they had never before felt so smart a shock.

From Tain, Dingwall, Dornoch, Wick, and all the towns to the northward of this, there were similar accounts to those given of Forres, and the other towns already mentioned. I had several very interesting and intelligent letters from Sutherland. One gentleman describes the sensation he and his party felt to have been just as if they had been all suddenly launched in a boat from dry land to sea. At first he supposed, for a moment, that one side of his chair, and the wall against which he was leaning, had suddenly given way. The hens made a prodigious noise on their roost; and a pointer dog howled for a considerable time afterwards. On looking out immediately afterwards, this gentleman remarked that the night was warm, and quite clear, but rather dark; the atmosphere heavy, and forming one cloud, except on the eastern and south-east horizon, where it had the appearance generally observable before sun-rise. Another gentleman, who was on the road near Brora, in a gig, writes me that he was not in the least sensible of any thing, and was quite ignorant of the shock, until he heard of it on reaching home, where he found his family had been alarmed. A lad who was standing on a rock in the middle of the country, at the time of the convulsion, declared that it moved up and down under him like a quaking bog.

At Aberdeen, Montrose, Dunkeld, Perth, Pitmain, and the other places intervening between this and the river Tay, the earthquake seems to have been generally felt, with equal violence, making allowance for variety of situation. At Aberdeen, a person who had been present during the earthquake in Lisbon on June 6, 1807, described the late shock as exactly resembling the commencement of it. In many houses the bells were set aringing, and the wires countinued to vibrate for some time after their sound had ceased.

The houses were shaken to their foundations, and the heaviest articles of furniture were moved. A second, but more slight and partial shock, was felt about half an hour after the first; and this was also remarked by some individuals in almost every quarter where the chief one had been experienced. At Parkhill, the seat of General Gordon, near Aberdeen, a circumstance occurred which deserves particular attention. The sluice-gate of a piece of water, weighing several tons, was raised from its foundation about 12 inches; and some large stones having accidentally rolled underneath it, kept it up in that situation till most of the waters escaped before it could be replaced. Several instruments have been from time to time proposed for measuring the degree of force of the shock of earthquakes; but here was one perfectly fortuitous, which, though perhaps it did not mark the utmost extent of its energy, proved that the power of the late one had been at least equal to an elevation of 12 inches. In the neighbourhood of Montrose a very amusing occurrence happened. Two excisemen having lain down, in concealment, on the ground, to watch for an expected party of smugglers, when the shock took place, one of them started up, exclaiming to his comrade, "There they are! for I feel the ground striking under their horses feet." In the town of Montrose, the inhabitants felt their beds move, first in a horizontal direction, and then return to their former situation; after which a tremulous motion was felt, as when a body, after being agitated, settles gradually upon its basis. Some compared it to the slight rolling of a ship at sea. The bells in houses were rung, and the furniture shaken, as in other places, and the greatest alarm prevailed. A vivid flash of lightning was observed to follow after the shock.

The article from Perth speaks of two distinct shocks, the second occurring at an interval of a minute after the first. In other respects the effects there appear to have been similar to, and nearly as powerful as, those at Aberdeen and Montrose. At Dunkeld, a young man, who was stepping into bed at the moment of the shock, was nearly thrown down on the floor; and in one house the liquor in the glasses was nearly spilt by the concussion. A small meteor was seen to pass from east to west just about the time of the earthquake.

A gentleman, who has been for some time on a visit to this neighbourhood, who has resided long in Italy, and who tells of himself that he has always had a kind of luck for meeting with earthquakes, asserts that, whilst sitting at breakfast, about three days before the late shock occurred, he distinctly felt a slight concussion; which, from the recollection of what he had experienced abroad, gave him very considerable alarm, but which he did not wish to communicate to his friends at the time. This gentleman was also perfectly sensible of the second and slighter shock, which followed on Aug. 13, at an interval of half an hour after the more decidedly violent convulsion. In this family, too, we all of us felt this second concussion. But although we noticed it to each other at the time,

yet I then suspected it to be nothing more than the sensation of the first shock, which still remained with us; as one is accustomed to think he feels the motion of the waves of the sea for a good while after he has landed from a ship. There cannot be any doubt, however, of the reality of this second movement of the earth; it having been noticed by some individual or other, and at the same interval of time, in almost every quarter where the more intense shock was experienced.

There is one fact which I conceive to be so peculiarly striking, that I cannot allow it to escape notice, having not only been very sensible of it in my own person, but having also learned, by inquiry of others, that the feeling was by no means a solitary one, but remarked pretty generally by a number of individuals. Immediately after the shock of the earthquake commenced, I felt myself assailed by a kind of faintishness, which did not altogether leave me until after I was asleep in bed, about two hours afterwards. This sensation was perfectly different from that generally attending the apprehension of immediate danger. Indeed, no such feeling could possibly be present with me; for I no sooner knew it to be an earthquake, than all sense of dread was absorbed in the delight I felt in being so very lucky as to have my curiosity satisfied, by the actual experience of so rare a phenomenon, the extent of which I naturally supposed, at the moment, might perhaps be confined to the narrow district around me. I have known several persons, quite incapable of being influenced by fear of any kind, who have remarked a similar sensation in themselves during the time of a thunder-storm. This faintish feeling, on the late occasion, was in some people attended by a very slight degree of sickness.

Perhaps it might not have been altogether without its use to have given in this place a slight and general geological sketch of the various rocks composing the different parts of the extensive range of country throughout which the late earthquake was experienced in the greatest intensity. But if I could even venture to draw more largely on your patience, in order to make such an attempt, I do not feel sufficiently confident in possessing ability or information enough to enable me to do justice to the subject. I may only remark, that every geognostic denomination of country seems to have submitted to the influence of the agitating power: that rocky positions have in general been much shaken, and in some instances (as in that of this very house) more so than those less decidedly of that character. We have hardly any data to enable us to say whether the primitive or the floetz rocks yielded most easily to the vibratory motion. But the alluvial site of the town of Inverness, under which I believe there is also a great deal of peat moss, seems clearly and decidedly to have manifested by far the most violent appearances of convulsion; which, if my information be correct, was even by no means so great on the eminences in the immediate neighbourhood. As we have thus the most prominent example of the power of the earthquake, displayed upon an alluvial deposit; so

we have reason to decide, from the body of the evidence, that almost all alluvial positions were in general more violently convulsed than the more stable formations in their close vicinity; although at the same time we find several anomalies militating against such a conclusion.

Upon the cause of earthquakes, to find a perfect solution of which has been a matter of difficulty to philosophers of all ages and countries, I do not dare to throw out any new speculation. I am, however, rather inclined to adopt that explanation which assigns it to the rarefaction, and conversion into steam, of large bodies of water, at considerable depths beneath the earth's surface. It is a general remark, in all countries where earthquakes are common, that they are preceded by the fall of copious rains. Such, for example, was the case with that of Lisbon, as well as with those of Calabria. In the domestic instance in question, too, we have had the same precursor in sufficient abundance; such a rainy summer as the past having been hardly remembered by any one. The rain water, gradually percolating into the bowels of the earth, may be converted into steam, by a combustion, to which a variety of causes may give excitement. Amongst these, the moistening of large beds of pyrites may perhaps be offered as one of the most simple explanations. Our late earthquake, however, may have not improbably had some remote connection with a subterranean volcanic influence; and an account which appeared from Naples, informing us that, on Aug. 7 last, Vesuvius was again in action, renders this last idea the less unlikely. Although, perhaps, not caused by electricity, it is very evident that this subtle agent was not entirely absent on the late occasion, as may be not only considered apparent from some of the effects produced, but is also proved by the flash of lightning seen to accompany the other phenomena at Montrose. The electric theory of earthquakes has been supported by Dr. Stukely, in his papers in vol. xlvi. of the Philosophical Transactions; and the Chevalier Vivenzio supposes the same cause to have operated in producing those of Calabria in 1783. But I cannot conceive electricity to have been the primary agent in producing the shock of Aug. 13 last; otherwise it must have certainly left more unequivocal effects behind it. Having, however, endeavoured in this paper to bring before your readers most of the facts and appearances connected with the late earthquake, an opportunity may perhaps be afforded to you or to them, either to strengthen one or other of the old theories, or to offer some new and still more rational explanation of a phenomenon which cannot fail highly to interest the enlightened and reflecting mind, as well as to impress it with the most profound admiration of the power of the Deity.

I remain, Sir, your obedient humble servant,

THOMAS LAUDER DICK.

## Article IX.

### *Proceedings of Philosophical Societies.*

#### ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Since the last annual report, the Society has devoted its labours to objects of intrinsic and permanent utility, and their success offers the best eulogium upon the enterprise and ability with which they have been conducted.

The Council has seen with great satisfaction the considerable progress which has been made in the construction of a Geological Map of the county. The interesting hundreds of Penwith and Kirrier are already finished. The rocks which constitute their surface are distinguished by appropriate colours; and their successions, relative positions, and various junctions and transitions into each other, are traced and delineated with an accuracy and detail which cannot fail to render the map a most acceptable present from geology to agriculture and the arts. To agriculture, as it shows the interesting connection between the various soils, and the subjacent rocks from whose decomposition they have been derived, and more particularly as it explains the local circumstances which are friendly or hostile to their improvement, and at the same time directs the agriculturist to the different mineral substances which are associated together in their vicinity, and which may contain materials highly useful in correcting their natural sterility. To the arts it will prove an acquisition of no less value, by showing the products peculiar to particular districts, and by leading to the discovery of those which are the more immediate objects of economical industry. The completion of this great *desideratum* may be confidently anticipated before the next annual meeting; but the Council begs to remind the Society that it will require the social and united exertions of its members; and they rely with confidence upon the co-operation of those gentlemen who are more remotely resident; for although some of the characteristic features of the eastern parts of the county have been gleaned during casual journeys, yet to trace the exact lines of junction through tracts of cultivation requires careful and repeated observation, occasionally facilitated by the removal of alluvial matter, which too frequently throws its veil over the very spots which it is most essential to examine.

The geological department of our museum has been enriched by a valuable accession of illustrative specimens which have filled up the several chasms in the series, and rendered it susceptible of a more perfect arrangement, which has been effected according to the general order of superposition which the different rock formations observe in nature. The Council is well aware of the many objections to such an arrangement; but it has been deemed preferable to

that which throws the specimens into insulated divisions according to the different districts from which they were derived, as being less likely to destroy the unity of the collection, upon which its value as a point of reference depends. The latter arrangement is also rendered the less necessary, as the geological map records the *habitat* of every specimen by a corresponding number, and furnishes a key by which the products of any particular district may be as easily selected as if they were deposited in regular succession.

A descriptive catalogue is in preparation, interspersed with such remarks as may be requisite for the purposes of elucidation, and of directing the student to a profitable examination of the specimens.

During the year several original communications have been received; but as they will shortly appear in a volume of Transactions before a public tribunal, the Council declines passing any opinion upon their merits.

Having concluded the notice of the progress which the Society has made in the scientific and useful pursuits, the Council feels that it has another important duty to perform—to recal the attention of the Society to the melancholy accidents which still occasionally happen from the use of the iron tamping bars, and to remind them of the simple and unobjectionable method devised for preventing them. The testimony of Mr. Chenhalls, who has the superintendance of many considerable mines, appears so satisfactory, that it is deemed a duty to give to it every possible publicity, especially as some of the eastern mines still maintain an unjust prejudice against the use of the safety bar. The Council has heard with regret that, within the last month, four fatal accidents have occurred at the United Mines, as well as several others in that district. A prejudice so blind and fatal calls for the powerful and steady intervention of this enlightened Society; a prejudice which has been proved to owe its origin solely to error, and its continuance to the apathy of those whose duty it is to correct it. If the chief boast and ornament of science is its power of increasing the comforts, and of diminishing the evils of life, surely to avert the perils with which those who labour for our benefit are beset, to rescue the fathers and supporters of families from death, or injuries worse than death, is an occupation which must cast a lustre on a Society professing to cultivate a science pre-eminently capable of applications to the useful purposes of mankind.

## *Mr. Chenhall's Letter.*

SIR, *St. Just, Sept.* 6, 1816.

In compliance with your desire, I most readily send you an account of the success of the new alloyed tamping bar. You already know that the one which Sir Rose Price first proposed was wholly composed of the alloy, which made it an expensive instrument; and, after several trials, was found too soft to stand the work, as it

bent and enlarged at the end. After repeated experiments, in which Sir Rose Price and yourself took so active a part, the proper proportions of tin and copper have been hit upon; a cap composed of which is soldered upon the bottom of the common iron bar. Thus constructed, it is quite free from every objection; and has now for 12 months past been constantly used by 400 miners, to my knowledge, without an accident of any description having happened, which is very gratifying to us all; for before the Geological Society recommended the trial of the safety bar, scarcely a month elapsed without some dreadful explosion sending the miner to an untimely grave, or so injuring him by blowing out his eyes, or shattering his limbs, as to render him a miserable object of charity for the rest of his days. So that now our most experienced miners are as much attached to the new bar as they were formerly prejudiced against it upon the ground of novelty; and I do not believe, after the evidence which they have had of its safety, that any thing would induce them to return to the use of the old or common bar. The common bar cost about 1*s.* 1*d.*, and the safety bar will cost about 1*s.* 8*d.*, which will last a man in constant use for 18 months, or more, and (when done) can be recapped again for a few pence. The shocking list of accidents which you have collected and presented to the Society, and the number of wounded men, widows, and orphans, which are every year thrown upon the different parishes for relief, are sufficient to prove the dreadful extent of the evil to which the use of the iron bar exposes us; and if the Geological Society which you formed had never produced any other good than the distribution of this safety bar through the mines of Cornwall, you would have had the satisfaction to know that it had not existed in vain.

I am, Sir, &c.

*To John Ayrton Paris, M.D. &c.* W. Chenhalls.

*Number of Members at the last and on the present Anniversary.*—Last anniversary, 141; withdrawn and dead, 8; elected this year, 20; total, 153.

The Treasurer reports that the necessary expense attending the collection of specimens, and the formation of the Geological Map, induces him to hope that the Members will not relax in supporting the resources for continuing them.

The following papers have been read this year:—

1. On the Geological Structure of Cornwall, with a View to trace its Influence upon, and Connection with, the Fertility of its Soils, accompanied with a Series of illustrative Specimens. By John Ayrton Paris, M.D. F.L.S. &c.

2. Historical Account of Copper and Copper Mines. By Joseph Carne, Esq.

3. On a new Arrangement of the Objects of Geology. By John Ayrton Paris, M.D. F.L.S. &c.

4. On Elvan Courses, accompanied with a Series of Specimens. By Joseph Carne, Esq.

5. Observations on a remarkable Change which Tin undergoes, under peculiar Circumstances, and on its partial Conversion into a Muriate of Tin. By the Rev. Wm. Gregor.

6. An Account of the Produce of the Copper Mines in Cornwall, in Ore, Copper, and Money, for the Year ending June 30, 1816. By Joseph Carne, Esq.

7. An Account of the Quantity of Tin produced in Cornwall in the Year ending with Midsummer Quarter, 1816. By Joseph Carne, Esq.

8. On the existing Evidences of a Catastrophe having at a remote Period formed the Mounts Bay. By Henry Boase, Esq.

## ROYAL INSTITUTE OF FRANCE.

*Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year* 1815.

MATHEMATICAL PART.—*By M. le Chevalier Delambre, Perpetual Secretary.*

### MEMOIRS APPROVED BY THE CLASS.

### ANALYSIS.

(*Continued from* p. 304.)

*A Memoir on the Motions of Fluids in Capillary Tubes.* By M. Girard. Commissioners, MM. Lefevre-Gineau, Poisson, and Prony, Reporter.

This memoir is divided into four parts, the three first of which constitute the special object of the report of the commissioners. They offer a systematic view of the new experiments, the simple exposition of which is sufficient to show the utility.

To determine the conditions of the uniform motion of fluids, in the state of our actual knowledge, nothing was wanting but to assign the expression of the retarding forces, which, in this motion, counterbalance the acceleration produced by gravity. On account of the canal of Yvette, M. de Chezy gave a formula in 1775, by means of which we can establish more exactly than was formerly possible the relations subsisting between the slope of the aqueduct, the dimensions of the transverse section, and the volume of water which it ought to convey. He found that the resistance is proportional to the surface of canal; he supposed that it increased as the square of the velocity of the water. His formula was composed of two terms, the second of which was the square of the velocity which multiplied a constant coefficient, which he determined by experiment. This formula was sufficiently exact for practice. It applied to open canals.

MM. Dubuat, d'Obenheim, and Benezech, military engineers, endeavoured by new experiments to find a more exact formula. But a logarithmic function, introduced by a kind of hazard, gives it an air of empiricism which ought naturally to lead mathematicians to new researches.

Coulomb had given a coefficient common to two terms of his formula, one of which depends upon the velocity, and the other is proportional to the square. There results from it a very simple expression of uniform motion, which accords with experiment almost as exactly as the formula of Dubuat. It has the advantage likewise to depend upon considerations intimately connected with sound philosophy, as well as with the mathematical theory of fluids.

M. Girard has observed that this supposition of identity between the coefficients, which may hold with certain fluids in particular circumstances, is not generally admissible; and he supports his conclusion by the Researches of Prony, published in 1804, and from which it results that in fact these coefficients ought to be different.

M. de Prony, from a combination of a great number of experiments given in his work, has deduced the value of the two coefficients; and consequently a formula of the uniform motion of water in pipes, and in open canals, sufficient to resolve with all the accuracy requisite in práctice the fundamental questions of hydraulics.

But this formula only gives the mean velocity, which is greater than the stratum of fluid contiguous to the surface of the pipe; a velocity which ought alone to enter into the expression of the retarding force. Hence it follows that the coefficients deduced from experiments hitherto published, have a smaller value than is suitable to the phenomena of motion that takes place at the surface of the pipe.

M. Girard endeavours, in the first place, to determine this velocity. And in order to obtain it, he observes that the velocity of the centre of the water in a pipe differs the less from the lateral motion, the smaller the diameter of the pipe is. He remarks, besides, that the theory of the line or motion of fluids, the formulas of which were first given by Euler in 1770, being applicable to the special case of motion in very small tubes, the observations made on such tubes ought to correspond so much the better with the theoretic formulas.

These considerations led to the hope that new experiments made with very small tubes would serve to determine the two coefficients; this hope was not realised; but the experiments to which it gave rise present, nevertheless, a collection of important facts. The tubes required to be accurately calibred, and it was necessary that the lengths could be altered at pleasure. M. Girard got two sets of copper tubes made, which were drawn through mandrils for making steel wire.

The first series is composed of tubes of 2·96 millimetres in diameter (0·1165 inch). Each tube about two decimetres (7·87

inches) in length, has a brass cap at its end, one of which terminates in a male, the other in a female screw, so that they may be fitted together, so as to form a tube of any length from 20 to 222 centimetres.

The second series is composed of tubes of 1·83 millimetres (0·072 inch) constructed like the first, so as to be screwed into each other.

These tubes may be placed horizontally in a cylinder of tin plate of 25 centimetres in diameter, and five decimetres in height. This cylinder is intended to serve as a reservoir. Externally it has a vertical copper rule pierced with small holes at the distance of five centimetres from each other, and having the threads of a female screw cut in them in order to receive the ends of the tubes. While the water of this reservoir flows out through the tube under experiment, it receives fresh water by means of a stop cock in a leaden cistern standing above the whole apparatus.

The uniformity of the flow being thus secured, the produce is received in a graduated copper vessel, placed horizontally, and it is seen to be quite filled when the water which it contains moistens the whole of a plate of glass which covers almost the whole of its surface. Finally, the time necessary to fill the vessel, according to the different lengths of the tube, is measured by means of a pendulum swinging seconds.

Care is taken at each observation to note the temperature of the water. The influence of this temperature on the phenomena renders this precaution indispensable.

The experiments of M. Girard are more than 1,200 in number. they were begun in the month of December 1813, and continued till May 1815.

The velocity of the water being known by experiment, two observations would be sufficient to determine the two coefficients. But to diminish the small errors, we may combine all the observations, so as to obtain with advantage the two equations really necessary.

M. Girard has arranged all his results in 34 tables, according to the different circumstances of the experiment. These tables show the effect of the temperature on the velocity of the flow; a new property which essentially characterizes the linear motion of fluids.

In this place the Commissioners state ten different phenomena, of which we shall only notice the last. It is that the temperature which acts so great a part in the uniform flow of liquids through capillary tubes hardly produces a sensible effect in pipes of the ordinary dimensions beyond the limits of capillary action.

It appears from the data of these observations that the motion of fluids in capillary tubes is not merely exposed to the influence of temperature, but likewise that the produce of this flow varies according to the nature of the solid substance of which the tubes are composed, and of the fluids subjected to experiment.

The conclusion of the Commissioners is, that this undertaking deserves the praise and approbation of the Class. It renders the

last part promised by the author very desirable. Meanwhile mathematicians will see with pleasure the first three parts published separately in the collection of Memoires par les savans Etrangers.

Since the date of this memoir, M. Girard has become a member of the Class.

*New Steam Engine, fitted for raising Water to very great Heights.* By M. Gengembre, Inspector General of the Mint. Commissioners, MM. Prony and Ampere.

The author of this memoir proposes, 1. To maintain a constant velocity in the column of water raised. 2. To prevent the reaction of this column on the valves. 3. To suppress in the mechanism as much as possible of those masses which lose their velocity, and receive a new one at each stroke. 4. To employ the action of vapour directly, in order to avoid the decompositions of force, which occasion the loss of a considerable part of the action in most machines.

The means employed to satisfy the first condition are not entirely new. They have been already employed by MM. Cecile and Martin in the last machine tried at Marly to raise the water by a single jet to the height of the aqueduct. Among the other methods which really belong to M. Gengembre, the most important has for its object to prevent the column of water from retrograding while the valve is shut. For this it is necessary that the piston remain stationary till the valve shut of itself, by falling, in consequence of its weight, through that portion of water which is rendered immoveable by the fixedness of the piston. To accomplish it, the author has made a very happy use of the other piston, which is then alone capable of acting, because it is alone in motion. And this improvement has appeared to the Commissioners deserving the particular attention of the Class.

M. Gengembre accomplishes the third and fourth objects by placing on a common beam the vapour piston and water piston. This method is very simple; but it completely answers the intentions of the author, and tends to diminish the price of the instrument by simplifying its mechanism.

The conclusion of the Commissioners is, that this machine merits the approbation of the Class, and that it ought to be considered as an additional title to the esteem of philosophers which the author has already acquired.

*New Determination of the Orbit of the Planet Vesta.* By M. Daussy. Commissioners, MM. Bouvard and Burckhardt, reporter.

M. Daussy, in a preceding memoir, for which he obtained the medal of Lalande, had employed all the oppositions of this planet that he could procure. He afterwards observed himself the opposition of 1814 at the *Ecole Militaire*, and calculated that of 1807 from the first observations of M. Olbers. All these oppositions united to the number of six constitute the base of the new determination. They comprehend nearly two revolutions of Vesta, and they are favourably disposed for the object of inquiry. The whole entitles

us to conclude that the eliptical elements ought to be very near the truth, provided the perturbations be exactly and completely calculated. M. Burckhardt had made on these perturbations a calculation which he compared with that of M. Daussy. He only found very slight differences in some terms of little importance, which M. Daussy has examined anew with particular care. Hence there is no error in these points to be dreaded. But in these perturbations are there no other terms but those that have been used? This M. Burckhardt has carefully examined; and the conclusion which he has drawn is, that this inquiry is as complete as astronomy requires. The Commissioners have found in this new memoir of M. Daussy fresh proofs of his skill and zeal for astronomy; and have requested it to be inserted in the next volume of the Memoirs presented by philosophers not members of the Institute.

*Orbit of the Comet of* 1807. By MM. de Lindenau, Nicolai, Bessel, and Nicollet.

After the planet discovered by M. Olbers, we must speak of the comet observed by that skilful astronomer at the beginning of the month of March. This comet was small, and gave little light. It was seen only by astronomers. Those of Paris, disappointed by the weather, could make but a small number of observations; from which, however, M. Nicollet deduced a parabolic orbit. Foreign astronomers, sooner aware of the existence of the comet, and less distracted by circumstances, were able to follow the comet longer, and with more assiduity. From these more numerous and longer continued observations, they have deduced an eliptic orbit; and it is remarkable that the greater axis of this elipse is less than that of the planet Uranus, and less even than that of the comet of 1759, the period of which is 75 or 76 years. We may flatter ourselves that it will appear in about 73 years. This at least results from the calculations of several distinguished astronomers, who have each separately come to this singular result, with an agreement very uncommon in a problem which can never be resolved in a way, I shall not say, certain; but even moderately probable after a single appearance. That this agreement may be perceived, we shall exhibit the two eliptical orbits.

| | According to MM. Lindenau and Nicolai. Time at Sceberg. | According to Bessel. Time at Paris. |
|---|---|---|
| Time of perihelion, 1815, April | 26,03857 | 26,00374 |
| Longitude of perihelion | 149° 3′ 28·13″ | 149° 3′ 29·1″ |
| Longitude of the node | 83 28 52·3 | 83 28 46·14 |
| Inclination | 44 29 56 | 44 29 53·7 |
| Eccentricity | 0·93029·345 | 0·93112771 |
| Distance of perihelion | 1·2126878 | |
| Semiaxis | 17·39704 | 17·60964 |
| Revolution | | |
| Direct motion | 72·584 years | 78·89682 |

*Experiments on the Dilatation of Solids and Liquids, and elastic Fluids, at high Temperatures.* By MM. Dulong and Petit. Commissioners, MM. Gay-Lussac and Biot.

The object of these experiments is to determine the dilatation of mercury and solid bodies compared with that of air at high temperatures. The authors first compared the dilatation of air with that of mercury in glass. The apparatus employed was similar to one formerly used by Gay-Lussac for the same object, below the boiling point of water. The commissioners explain the modifications which it underwent in the hands of Dulong and Petit, and the causes of them. We cannot enter into these details, nor into those of the experiments repeated in two different ways, which agreed in showing that the dilatation of mercury in glass increases when compared with that of air, as there was reason to presume would be the case from experiments on other liquids. The difference is insensible as far as 100° (centigrade), a result which M. Gay-Lussac had already established, and which is of consequence in the calculation of astronomical refractions. Above this term the mercurial thermometer rises more than the air thermometer; and when the former marks 300°, the latter marks $8\frac{1}{2}$ less.

This result only gives the apparent dilatation of mercury in glass. To know the absolute dilatation, the authors employed a process analogous to one contrived by Borda to know the dilatation of platinum rods for the operations of the meridian. Their experiments made with this apparatus at different temperatures, rising gradually to 300°, led them to this unexpected consequence, that in high temperatures the dilatation of metals follows a more rapid law than that of the mercurial thermometer; and, *a fortiori*, more rapid than that of air; so that when an air thermometer marks 300° on the scale, a mercurial one will mark 310°, and a metallic thermometer 320°. This result, though not easily foreseen, is not, however, contrary to analogy; for it does not imply that the dilatation of metals, compared with that of air, increases more rapidly than the absolute dilatation of mercury, which indeed would be very unlikely; but more rapidly than the apparent dilatation of mercury in glass, which is the excess of the real dilatation of that liquid above that of the vessel in which it is contained. But since the observation of the metallic thermometer gives to metals an increasing dilatation with respect to air, it is probable, it is even certain, according to the experiments of MM. Dulong and Petit, that glass likewise possesses this quality. Hence the progressive increase of its volume ought to make that of the mercury less sensible, and may balance it sufficiently to render its progress slower than that of the metals considered alone. This is what the authors carefully remark.

Supposing these notions accurate, the dilatation of mercury in metals (in iron, for example) ought to appear increasing, the liquid dilating more than the metal. The authors have verified this

by weighing the quantities of mercury that could be contained in a vessel of iron at different temperatures, rising higher and higher, Between 0 and 100° they found the absolute dilatation of mercury, corrected from that of the iron, exactly as assigned by Lavoisier and Laplace, by analogous experiments made in a glass matrass. But at higher temperatures the mercury dilated at a much more rapid rate; for it flowed from the vessel in quantities much more considerable than would have been the case if the iron and glass had preserved proportional dilatations.

Supposing, then, the facts well observed, and the numerical reductions exactly made, we cannot doubt that mercury, glass, and the most infusable metals, follow increasing rates of dilatation when compared to that of the air thermometer, when they are exposed to higher temperatures than that of boiling water; and that these differences (which could not have been supposed) are very sensible even below 300°.

This is an important result; for which we are indebted to the authors of the memoir. Being no longer able, then, to regard any of these bodies, except perhaps air, as following the law of dilatations proportional to the increase of temperature, it becomes necessary to measure the absolute dilatations of this fluid at high temperatures, and to show their relation to the quantity of heat required. After this we shall know the dilatations of all other bodies by comparing them with that of air. It is then, and only then, that we shall be able to measure the quantities of heat by the thermometer, whether of air, mercury, or metal, and that we shall be able to determine the true laws of the cooling and heating of bodies at all temperatures. Of this the authors are sensible, and they prepare to continue their experiments under this point of view. It is of importance to encourage them; for these researches are at present indispensable for the advancement of our knowledge in the theory of heat.

*Compound Lock.* By M. Peyrard.

The report which M. Molard read on this occasion is a complete history of this kind of lock from the first invention down to our times. It is accompanied with engravings, which render it more easily understood. The Class has decided upon publishing it in the memoirs. This reason, and the impossibility of giving the drawings here, prevent us from making any extract. We shall merely state the conclusion, which regards M. Peyrard:—

"The means employed by M. Peyrard to render every attempt at picking inefficacious, are so combined as to produce this effect in the most certain and complete way. In this point of view we think that this improvement deserves the approbation of the Class."

(*To be continued.*)

# Article X.

## SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

### I. *Corrections of the Paper inserted in the last Number of the Annals, p. 279.* By Mr. Horner.

(To Dr. Thomson.)

DEAR SIR, *Bath, Oct. 3, 1816.*

Having partly prepared my last communication at a time when the preceding volumes of the *Annals* were in the binder's hands, and completed the references afterwards in a perfunctory manner, I did not perceive, till I received your last number, a material error in my reference to that for *May*, 1816. It certainly was not my intention to give my suffrage to the criticism of Dr. Tiarks, but of the correspondent who subscribes N. R. D., and who has with perfect correctness remarked that Mr. Lockhart's fourth cube root of 64 is a disguised cube root of 8. If Mr. L. had written $-(2-6\sqrt{-3})$, instead of the awkward expression $+(-2+6\sqrt{-3})$, he could hardly have failed to anticipate N. R. D.'s observations.

The words "alternately positive and negative," at p. 280, l. 18, should have been "alternately negative and positive." This erratum is probably due to the transcriber. But I notice several in the impression which an attentive reader will readily see to be typographical. Two of them have fallen so unluckily as considerably to affect the perspicuity of the ultimate results of the concluding section. Permit me, therefore, to repeat those results here, and to subjoin a few additional inferences.

The correct values of

$$\sqrt[3]{\frac{c}{2} \pm \sqrt{\frac{c^2}{4} - \frac{b^3}{27}}}$$

$$\left.\begin{aligned} \text{are } \frac{R}{2} \pm \sqrt{\frac{R^2}{4} - \frac{b}{3}} &= A,\ A' \\ -\frac{r}{2} \pm \sqrt{\frac{r^2}{4} - \frac{b}{3}} &= B,\ B' \\ -\frac{\varrho}{2} \mp \sqrt{\frac{\varrho^2}{4} - \frac{b}{3}} &= C,\ C' \end{aligned}\right\} (1)$$

$$\left.\begin{aligned} \text{Or, } \tfrac{1}{2} R \pm \tfrac{1}{6}(r - \varrho)\sqrt{-3} &= A,\ A' \\ -\tfrac{1}{2} r \pm \tfrac{1}{6}(R + \varrho)\sqrt{-3} &= B,\ B' \\ -\tfrac{1}{2} \varrho \mp \tfrac{1}{6}(R + r)\sqrt{-3} &= C,\ C' \end{aligned}\right\} (2)$$

in both the cases of cubic equations; except that the values of C, C′, (as I should have said in my last, instead of B, B′,) in equations (1), become dubious when applied to the reducible case.

To a careless observer, these equations, as far as regards the disputed signs, may appear to confirm Mr. L.'s opinions; equations (1) may in fact be supposed *primâ facie* to be identical with his. But it will be obvious, on reverting to p. 284 for the values of $r$ and $\varrho$, from which equations (2) were derived, that the roots which I call $r$ and $\varrho$ are equivalent to the roots $t$ and $v$ of your former Correspondents in the irreducible case, and to $v$ and $t$ in the reducible. In the latter case, therefore, my results are equally incongruous with those of either Mr. L. or Mr. Atkinson. The last-mentioned gentleman has reasoned accurately respecting the root $t$, but has assumed the case for $v$ without investigation.

At p. 283, l. 13, *for* interrupted, *read* interpreted.

Anxious to be the first to correct my own oversight, I add nothing more at present, lest I should exceed the limits of convenient admission into your *next*.

I am, very respectfully, yours,
W. G. Horner.

## II. *On the Curvature of the Circle.*

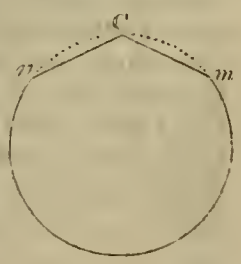
(To Dr. Thomson.)

SIR,

I am glad to observe that the proposition which you obliged me by inserting in the *Annals* for June, respecting the curvature of a circle, has excited some attention. Mathematicians in general seem to have had no clear and definite notions respecting the principle. To suit one purpose, they assume the circle to be of perfectly uniform curvature: to suit another, they assume it to be only a polygon, or figure made up of a number of sides. These two ideas certainly ought not to be used indiscriminately. It is, therefore, necessary to determine which of them is correct.

The principle I laid down has not been objected to, though the demonstration of it has been pronounced unsatisfactory. The following may, perhaps, be considered more conclusive.

If the two right lines, $n$ C, and $m$ C, meet in the point C, they must evidently form an angle; and, according to the controverted principle, this angle constitutes a portion of the circumference of the circle; but this angle is capable of increase or diminution; and therefore all circles of the same diameter have not the same curvature, which is obviously erroneous. And consequently that if all circles be of the same curvature, they must have no angles, and therefore no right lines, which is according to the original proposition.




If the above demonstration be deemed insufficient, I should like to see what proof can be adduced in support of the opposite principle.

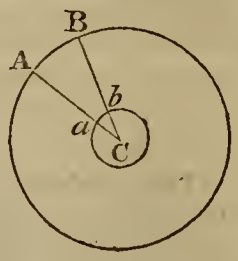
Yours respectfully,
U.

## III. *Another Communication on the same Subject.*

(To Dr. Thomson.)

SIR,

In one of your former numbers there was a proposition stating that no part of the circumference of a circle is a straight line. In your last number there was an attempt to answer this proposition by pretending to assume the same mode of reasoning upon a square as the gentleman who advanced the proposition applied to the circle. Had the mode of reasoning of the former corresponded precisely with that of the latter, the objection would have militated against the proposition; but there is this difference between these gentlemen's reasoning; the former selects, to suit his own purpose, that part of the square which makes an angle; but the latter allows any part of the circumference of the circle to be taken, which completely destroys the force of the answers.




The following demonstration contributes to confirm the truth of the proposition which was laid down by your first correspondent upon this subject:—

Suppose *a b* to be a straight line; then extend the lines C *a* and C *b* to A and B, and describe the large circle. But, according to mathematical principles, A B is to the circumference of the larger as *a b* is to the circumference of the smaller; but the large part, A B, is a curve, and therefore the indefinitely small part, *a b*, is also a curve. Q. E. D.

I am, Sir, your obedient servant,

W. E.

## IV. *Spots on the Sun.*

(To Dr. Thomson.)

MY DEAR SIR, *Bushey Heath, Sept.* 21, 1816.

The spots on the sun having excited so much attention, I take occasion to mention a singular fact that was communicated to me some years past by my friend the late Mr. Wm. Bayly; he told me that, as he was viewing the spots on the sun with a telescope, he observed one of them suddenly split and separate in two. Mr. Bayly was a man of strict veracity; and had accompanied Captain Cook twice round the world as astronomer; and afterwards was Head Master of the Royal Naval Academy in Portsmouth dock-yard. I remain, my dear Sir,

Very sincerely yours,

MARK BEAUFOY.

## V. *Comparative Heights of the Surface of the Caspian and Black Sea.*

In consequence of the observations of Mayer, Pallas, &c. it ha long been the general opinion that the surface of the Caspian Sea

is considerably lower than that of the Black. Dr. Young, in his Lectures on Natural Philosophy, vol. ii. p. 367, states the difference, but without giving any authority, as 306 feet. Some years ago Messrs. Engelhardt and Perrot undertook a journey to the countries that divide these two seas, partly to examine the Caucasus, but chiefly with a view to subject the relative heights of the surface of these two seas to a barometrical measurement. The result of their labours was published last year at Berlin, in two octavo volumes, under the title of Reise in die Krym und den Kaukasus. I saw this book some months ago in Sir Joseph Banks's library; but it happened to be lent out before I had an opportunity of perusing it, and I have not seen it since. The account, therefore, of their measurement, which I am about to give, is derived from an extract from that book published by Gay-Lussac and Arago in the first number of the Annales de Chimie et Physique, p. 55.

The distance between the two seas is 813 werstes. Now as 104·3 werstes constitute a degree of the meridian, this distance may be stated at 539$\frac{1}{2}$ English miles. They levelled this distance no less than three times, by means of a barometer. The height of the barometer at every station was always taken four times, at an interval of 15 minutes each, and every precaution was attended to that could ensure accuracy in the observations. They began at the mouth of the river Kuban, which falls into the Black Sea, and proceeded to the mouth of the Terek, which falls into the Caspian nearly under the same parallel. The number of stations was 51, so that the mean distance between each was nearly 12·6 miles. (For the distance between the two seas by the road they took was 990 werstes.

The first measurement was begun on the 13th of July, and terminated in October. It was made under very favourable circumstances, and gave 105 metres, or 344·5 English feet, for the elevation of the surface of the Black Sea above that of the Caspian. The second measurement was made from the Caspian to the Black Sea between the 10th and the 14th of October. But the weather was very unfavourable; for they encountered a great deal of wind, rain, and snow, which usually, when they occur, render barometrical measurements too low. They found the difference between the surface of the two seas, by this second measurement, 92 metres, or 301·8 English feet. M. Perrot had the patience to make a third measurement; but he was so long detained on the way, that there was no corresponding observation on the shore of the Black Sea, M. Engelhardt having made his last the evening before M. Perrot reached the Caspian. This third measurement would give 99 metres, or 324·8 English feet. But this measurement, for the reasons stated, cannot be relied on. The mean of the first two measurements gives us 98·5 metres, or 323·17 English feet.

It was the opinion of Pallas that the level of the Caspian Sea had formerly been much higher than at present. This opinion is confirmed by Perrot and Engelhardt, who place the ancient height 234

metres, or 767·7 English feet, above the present level; so that the quantity of water lost must be immense. They conceive that this did not happen by evaporation; for, according to Gmelin, the Caspian Sea does not contain one-fourth of the quantity of salt which exists in the Atlantic. They conceive that it has made its escape by means of subterranean channels, which occasionally open. But the possibility of such an escape, at least into the Black Sea, seems problematical. The surface of the Black Sea being so much higher than that of the Caspian, if any such channels had existed, the water ought to have moved the contrary way, and increased, instead of diminished, the size of the Caspian.

### VI. *Comparative Heights of the Surface of the Red Sea and of the Mediterranean.*

When the French were in Egypt they levelled, under the direction of M. le Pere, the distance between the Red Sea and the Mediterranean, and found the surface of the Mediterranean 8·12 metres, or 26·64 English feet, below the surface of the Red Sea at low water; and 9·9 metres, or 32·47 English feet, below the surface of the same sea at low water.

### VII. *Comparative Heights of St. George's Channel and the German Ocean.*

The difference between the height of the surface of the sea on the west and east coast of Great Britain is 50 feet; that is to say, the surface of St. George's Channel is 50 feet higher than the surface of the German Ocean, calculating the difference by the canals proceeding east and west from Birmingham. The descent of these canals is known by the number and height of the locks in each. But I do not believe that the height of these locks is given with so much precision as to allow us to found any calculations upon it.

### VIII. *Method of hardening Steel by Arsenic.* By Mr. Gill.

(To Dr. Thomson.)

SIR, *No. 83, St. James's-street, Oct. 4, 1816.*

Seeing in your last number a notice from Mr. Gregor on the late, much to be lamented, Mr. Smithson Tennant's discovery of a small quantity of arsenic in the Indian cast steel, or wootz, I am happy in being able to add my testimony of such a union being sometimes made in this country, where very great hardness and strength in steel articles are required; although the process is but in very few hands, and by no means so well known as it ought to be. Thus some locksmiths can make slit-saws, which will readily saw through a case hardened key; and I have heard of some celebrated makers of awls, which, slender as they are, will yet penetrate through a shilling, without bending or breaking; and I know that this extraordinary hardness and density is given to them by quenching them, when heated to a due degree, in a solution of arsenic in animal

oils; but I shall reserve the communication of the exact process for a work which I have long had it in contemplation to publish on the treatment of iron and steel in general; and particularly according to the superior processes employed by my late father, myself, and others, which I have made it a study to obtain.

With regard to wootz, I know that an old and celebrated maker of sextants, and other mathematical instruments, has found that a dividing knife made of wootz, hardened in a particular manner, stands better than any he ever made before; and I have had a desk knife, made by my own process, of wootz re-cast in this country, in constant use for three years and eight months, without its edge being in the least degree injured, or ever requiring to be set.

I trust that these facts will now induce the manufacturers of cast steel in this country to make experiments with a view to its improvement, by the union of arsenic with it during its fusion; from which results of the utmost importance may probably be obtained, and this country no longer remain under the stigma of being excelled by the simple Hindoo in one of its staple articles.

I sincerely regret, with yourself and Mr. Gregor, that so celebrated a chemist as Mr. Smithson Tennant should be cut off, without benefiting the world by the communication of the valuable mass of knowledge which he must undoubtedly have acquired in the course of his long experience. It is a case of too frequent recurrence that skilful and scientific men set too little value upon their discoveries; and, content with employing them for their own benefit, think not of the great labour it cost them in attaining them, and leave the world but little better for their experience. It shall, however, be my care, that a similar fate shall not befal my late father's improvements; as it is only by the diffusion of knowledge that the arts can be brought to perfection. I cannot here avoid paying that tribute of justice to the Society for the Encouragement of Arts, Manufactures, and Commerce, which its eminent services to the public so fully merit; and which, under the auspices of its Royal and scientific President, and the very great augmentation of its members, bids fair to attain that national importance which, from its great utility, it is entitled to. It is, indeed, with the exception of the Board of Longitude, the only institution in this country where mechanical skill and ingenuity can hope for a reward; and I am truly proud in having had it in my power to forward its beneficial objects as a member, and a Chairman to its Committee of Mechanics for many years.

I am, with much respect,

Sir, your most obedient servant,

THOMAS GILL.

## IX. *On Mr. Ryan's Mode of Ventilating Coal-Mines.*

I have been requested, by a letter from a friend in Newcastle, to publish the following document in the *Annals of Philosophy*:—

*Newcastle, Oct.* 5, 1816.

We whose names are undersigned, being persons who composed the meeting held at the Assembly Rooms, Newcastle, Sept. 9, 1815, observing that Mr. Ryan has affixed the resolution of that meeting, with our signatures, to his "Letter to the late Secretary of the Society of Arts," on his method of ventilating coal-mines, feel ourselves called upon to declare, that no part of that method was then explained to us, but simply an application of the inverted siphon to the clearing away of collections of carbureted hydrogen gas which may have accumulated in cavities in the higher levels of a coal-mine; which he illustrated by the common experiment of a bended glass tube immersed in two fluids of different densities.

In consequence, however, of this general explanation of Mr. Ryan's *principle*, the particular application of which, at the same time, he expressly declined communicating, we thought it right to give our testimony of general approbation of it, "as consistent with the principles of philosophy;" and in consequence of his assurances of its successful application in certain coal-mines in Staffordshire, "recommended the propriety of its adoption to be taken into consideration by persons interested in the coal-mines."

On the merit or demerit of the plan now pointed out by Mr. Ryan, we do not feel ourselves called to give an opinion; our only object is to state, that no communication of it was made to us at the meeting above alluded to.

| | |
|---|---|
| R. W. Grey. | Christ. Benson. |
| T. H. Bigge. | Wm. Clarke. |
| William Turner. | Wm. Armstrong. |
| Nat. J. Winch. | Rob. Wm. Branaling. |
| John Carr. | |
| John Hodgson. | |

## X. *Answer to Mr. Donovan's Defence in the last Number of the Annals, p.* 315.

(To Dr. Thomson.)

SIR,

I perceive that Mr. Donovan, in his reply to my observations, has not ventured to assert directly that his Essay obtained the prize, but merely endeavoured to show that the reasoning which he attributes to me is inconclusive. This he seems to have done, partly to mislead the public from the real state of the case, and partly to exhibit his acquaintance with the terms of logic. I stated as a *fact* which I knew *certainly*, and not, as he insinuates, by any "inference from the definition of the academic prize, or the abstraction of 10*l.*" that he obtained the 40*l.* which were voted merely as a gratuity; and I now tell him, if he is ignorant of it, that the Committee appointed to examine his Essay (which constitutes about two-thirds of the present publication, and the only tolerable part of it) declared it not entitled to the prize. He must, therefore, excuse me if I

prefer the testimony of the distinguished members of that Committee to his inference. Mr. D.'s direct proof shows that, in his logical studies, he overlooked that part which cautions us against using an argument which may be retorted. My Essay, he says, is a prize essay, because two others, which obtained 25*l.* and 30*l.*, appear in the Academy's Transactions, and are there denominated prize essays. To this I answer that, if his appeared there, the argument might be admitted; but as the Academy did not think it worthy of this honour, it cannot be compared to those which were published without hesitation; and this circumstance I consider decisive on the point. If the sum constituted the prize, we are to suppose that it is proportional to the merit. Mr. D.'s Essay must, therefore, have been superior in the ratio of 40 : 25 to one which was published; and yet, though the Transactions do not seem overburthened with scientific communications, they positively reject it. Does not this show that the Essay was considered worthless, and that the money was voted on some other account than its merit? If it is necessary to explain why they gave any thing to it, I should ascribe it partly to their wish to encourage a laborious individual, and partly to the feeling of compassion caused by a complaint which he made in his manuscript of the heavy expense which he incurred by his experiments, which he said was such as nearly to equal the total sum offered. His subsequent conduct, however, will effectually prevent any repetition of such ill-judged liberality.

My fears for the character of the Academy arose, not from any idea that they were pledged for the assertions which Mr. D. makes in his Essay: that is obviated by their prefatory advertisement; but it could not secure them from the charge of want of judgment, if they bestowed a prize on a production, half of which was merely a compilation from different journals, and the other, though not so absurd as the hypothesis which he has since substituted in its place, yet is sufficiently so to astonish those who had any knowledge of the subject. With Mr. D.'s Essay I have now done. It is unnecessary for an individual, whose name, even if known, would have no weight, to volunteer any further in a cause which the Academy itself has taken up, particularly as he has apologised to them for his conduct, in a letter which is inserted in their journals. In the course of the discussion, my original statement was read before the Council of the Academy, and was not blamed or controverted; so that, if he denies its truth, he does it without the pretence of ignorance; and it is useless to dispute with a man who shuts his eyes against conviction. As to the other part of his letter, namely, the process for obtaining silver, if I am wrong, I err with many; if I am impolite, so are most of the chemical writers who mention the subject; for their statements are all at variance with his. It therefore does not, I hope, argue very profound ignorance to question the accuracy of a chemist, who has scarcely been heard of, on the authorities which I shall mention. In Thomson's System, vol. iii.

p. 148, are these words: "Silver is precipitated from this salt by most metals, especially by mercury and copper, *which at the same time combine with it.* In Aikin's Chemical Dictionary, it is said that all the silver cannot be precipitated by copper. Cramer's Docimasia, a good authority on this subject, states, that silver precipitated by copper retains between $\frac{1}{32}$ and $\frac{1}{64}$ part of the precipitant which he proposes to remove by fusion with a little nitre, as likely a method as the action of ammonia, which might possibly form the fulminating ammoniuret. Mr. D. will perhaps reply, that ammonia has no action on metallic silver, neither has dilute muriatic acid, as is generally supposed; yet I find that it acts on silver thrown down by proto-sulphate of iron. At all events, the expense of ammonia would be more than the value of the silver lost in the reduction of luna cornea. Fourcroy proposes, in his Systeme, to remove the adhering copper by cupellation; but it would be tedious to name the others who could be brought forward to support me. Berthollet obviously did not attempt to ascertain the quantity of copper, but was satisfied with its presence. From an experiment which I made long ago, and which can easily be tried, I imagine that there is a considerable affinity between the two metals. I exposed to heat, *per se*, the precipitate by carbonate of soda from a solution of standard silver, expecting that as this metal is reducible without the addition of charcoal, I should obtain it free from its alloy. But I found that the button contained copper. Now unless we suppose that carbonic oxide found its way through the luting of the crucible, the latter metal must have been reduced by the affinity of the silver. For these reasons I question the process of Mr. D., and think I can account for his mistake. The first portions which fall down are, as I have often observed, and as Cramer also mentions, much purer than the latter; and it is possible that he experimented on some of the former. However this may be, I protest against the right of any man exclaiming against an examination of his experiments even by *conjecture.* Whoever applies the scale of equivalents to Kirwan's table of salts, will distrust Mr. D.; and it is amusing to hear a man talking of politeness who denies, without any ceremony, the assertions of Davy and Wollaston.

*Dublin, Oct.* 8, 1816. M. B.

## XI. *Analogy between the Kidneys and Testicles.* By Dr. Cross.

(To Dr. Thomson.)

SIR, *Glasgow, Sept.* 9, 1816.

From the original propinquity of the kidneys to the testicles; from the venal and spermatic artery so often coming out of the aorta in one trunk; from the venal plexus of nerves sending downwards to the testicles one or more fillets; and from the intimate connexion in structure between the urinary and the genital apparatus; may we not legitimately presume that there is also between them a connexion in function? And, since the genital is a primary

or fundamental function in the animal economy, that the urinary secretion is subservient to the business of generation? Is not this conjecture supported by the trim of the generative function being so proportional to the urinary secretion; by those substances which excite urinary secretion stirring up at the same time the generative appetites and powers; by morbidity of urinary secretion being always accompanied by morbidity of the generative function; witness the concomitant want of urea and of sexual desire in diabetes: and I had almost subjoined, is not the above conjecture supported by the long strings of rules—the result, without doubt, of more or less experience—which have been so confidently laid down by old medical authors for ascertaining the procreative powers from the qualities of the urine?

I remain, Sir, most respectfully,

Your most obedient servant,

JOHN CROSS.

XII. *State of the Wheat in the County of Edinburgh.* In a letter from the Rev. Dr. Grierson.

(To Dr. Thomson.)

MY DEAR SIR, *Manse of Cockpen, Sept.* 20, 1816.

We have an alarming season. A scarcity we look upon here as quite certain, if not something worse than a scarcity. In this parish two or three small patches of barley have been cut, but no wheat nor oats. Indeed, by far the greater part of these two latter have scarcely yet begun to colour. My friend and neighbour, Mr. Witherspoon (one of our great farmers), and I, made an experiment yesterday to estimate as nearly as we could the quality of the wheat on his farm. He blindfolded himself, and then went into one of his wheat-fields that had been sown after beans, and plucked six ears of wheat. He did the same thing in a field after fallow; and also in one after potatoes. We found in the six ears after beans 163 sound grains, and only 24 diseased or blighted; in the six ears after fallow, 102 grains sound, and 54 unsound or blighted; and, shocking to state, in the six after potatoes, we found only 30 grains having any thing in them at all, and even these far from marketable grain. To that in the field after beans the sound to the unsound is as 163 to 24; after fallow, as 102 to 54; and after potatoes as 30 to 150!

The general result is, the diseased grains in the whole 18 heads are 198, and the sound ones 493, or as 1 to $2\frac{1}{2}$ nearly.

I am, with great esteem and regard,

My dear Sir, yours most faithfully,

JAMES GRIERSON.

XIII. *Congelation of Oil by Dilute Nitric Acid.*

*Oct.* 9, 1816.

W. H. will be particularly obliged by either Dr. Thomson, or

one of his chemical readers, explaining the process of congealing animal oil by diluted nitric acid.

I do not recollect having tried the experiment with animal oil, though I am not aware of any reason why it should not succeed with animal as well as vegetable oil. I have made it very often with olive oil, without ever failing. My method was this:—Put into a wine-glass nitric acid diluted with about 10 parts of water, and cover the surface of the liquid with a thin coat of olive oil. Put the glass in a window, and allow it to remain exposed to the light for three or four days. If at the expiration of this time the oil has not begun to assume a tallowy appearance, add a little more nitric acid, and replace the glass. If this second addition of nitric acid be not sufficient, a third portion may be added. But it is only when the acid is very weak that such additions are necessary.—T.

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## Article XI.

### *New Patents.*

George Dodgson, of St. Paul's, Shadwell, pump and engine manufacturer; for a method of simplifying and improving the construction of extinguishing engines, and forcing pumps. May 27, 1816.

Isaac Hadley Reddell, of Orange-court, Leicester-square, engineer; for certain improvements in or on the means of lighting the interior of offices, theatres, buildings, houses, or any places where light may be required. May 27, 1816.

Robert Kemp, jun. of Cork, smith and brass founder; for an improvement or improvements in the art of making or manufacturing cocks and keys. May 27, 1816.

James Heathcoate, of Loughborough, lace manufacturer; for certain improvements upon a machine or machinery already in use for making hosiery or frame work knitted, commonly called a stocking frame. May 30, 1816.

John Ransome, of Ipswich, ironmonger; for certain improvements on ploughs. June 1, 1816.

William Shand, of Villiers-street, Strand, artificial limb maker; for certain improvements in the construction of artificial legs and feet, made of leather and wood, acting by a lever and spiral spring. June 1, 1816.

John Foulerton, of Upper Bedford-place, Russel-square, Esq.; for various improvements in beacon buoys, which improvements are applicable to other useful purposes. June 11, 1816.

Edward Light, of Foley-place, Marylebone, professor of music; for certain improvements on the instrument known by the name of the harp lute. June 18, 1816.

## Article XII.

# METEOROLOGICAL TABLE.

| 1816. | Wind. | Barometer. Max. | Min. | Med. | Thermometer. Max. | Min. | Med. | Hygr. at 9 a. m. | Rain. | |
|---|---|---|---|---|---|---|---|---|---|---|
| 9th Mo. | | | | | | | | | | |
| Sept. 14 | S W | 29·99 | 29·95 | 29·970 | 67 | 48 | 57·5 | 56 | | ☾ |
| 15 | S | 29·96 | 29·93 | 29·945 | 72 | 46 | 59·0 | 61 | | |
| 16 | S | 29·95 | 29·87 | 29·910 | 74 | 44 | 59·0 | 70 | | |
| 17 | E | 29·85 | 29·78 | 29·815 | 72 | 48 | 60·0 | 72 | | |
| 18 | N E | 30·02 | 29·78 | 29·900 | 70 | 47 | 58·5 | 59 | — | |
| 19 | N E | 30·02 | 29·98 | 30·000 | 60 | 43 | 51·5 | 53 | | |
| 20 | E | 29·98 | 29·64 | 29·810 | 62 | 40 | 51·0 | 62 | | |
| 21 | S E | 29·60 | 29·57 | 29·585 | 60 | 50 | 55·0 | 63 | | ● |
| 22 | N E | 29·74 | 29·60 | 29·670 | 66 | 48 | 57·0 | 66 | | |
| 23 | N | 29·73 | 29·64 | 29·685 | 63 | 51 | 57·0 | 72 | | |
| 24 | S | 29·97 | 29·73 | 29·850 | 61 | 51 | 56·0 | 59 | 0·13 | |
| 25 | S E | 30·09 | 29·97 | 30·030 | 67 | 42 | 54·5 | 72 | | |
| 26 | N | 30·12 | 30·06 | 30·090 | 64 | 48 | 56·0 | 56 | — | |
| 27 | Var. | 30·06 | 29·96 | 30·010 | 63 | 47 | 55·0 | 60 | — | |
| 28 | S | 29·96 | 29·70 | 29·830 | 63 | 43 | 53·0 | 80 | — | ☽ |
| 29 | S W | 29·65 | 29·32 | 29·485 | 60 | 39 | 49·5 | 65 | 0·43 | |
| 30 | S W | 29·65 | 29·42 | 29·535 | 60 | 47 | 53·5 | 89 | — | |
| 10th Mo. | | | | | | | | | | |
| Oct. 1 | S W | 29·58 | 29·55 | 29·565 | 63 | 46 | 54·5 | 78 | ·66 | |
| 2 | W | 29·84 | 29·41 | 29·625 | 62 | 45 | 53·5 | 65 | ·16 | |
| 3 | Var. | 29·84 | 29·80 | 29·820 | 58 | 49 | 53·5 | 85 | ·16 | |
| 4 | S W | 29·82 | 29·80 | 29·810 | 66 | 53 | 59·5 | 77 | — | |
| 5 | W | 29·82 | 29·78 | 29·800 | 66 | 52 | 59·0 | 71 | | |
| 6 | S E | 29·78 | 29·74 | 29·760 | 68 | 56 | 64·0 | 85 | 0·54 | ○ |
| 7 | E | 29·87 | 29·74 | 29·805 | 59 | 55 | 57·0 | 80 | 0·27 | |
| 8 | E | 29·95 | 29·87 | 29·910 | 66 | 54 | 60·0 | 65 | | |
| 9 | E | 29·98 | 29·95 | 29·965 | 63 | 55 | 59·0 | 64 | — | |
| 10 | N W | 29·95 | 29·92 | 29·945 | 65 | 51 | 58·0 | 69 | | |
| 11 | N E | 30·06 | 29·92 | 29·990 | 60 | 44 | 57·0 | 73 | — | |
| 12 | | 30·06 | 30·04 | 30·050 | 57 | 47 | 52·0 | 65 | — | |
| 13 | | 30·04 | 30·03 | 30·035 | 55 | 39 | 47·0 | 88 | | |
| | | 30·12 | 29·32 | 29·840 | 74 | 39 | 55·9 | 69 | 2·35 | |

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.* — 14. The sky overcast with numerous beds of *Cirrocumulus*, which at sun-set changed to *Cirrostratus*, and became red. 15. a. m. An electrical smell: much dew: there appears to have been a *Stratus* in the night: serene day. 16. Much dew: large plumose *Cirri*: very fine. 17. Much dew: misty: fine day: in the evening a solitary *Cumulus* cloud in the W. spired up to inosculate with a *Cirrostratus* above it, a *Stratus* at the same time appearing in the meadows nearer to us: several discharges of lightning in the N. W. followed these appearances: the barometer, which had fallen a little, now rising. 18. Overcast day: a little rain perceptible in the evening. 19. Fair, with a grey sky, and a few distinct *Cirrostrati* beneath. 20. Grey sky, a. m.: then sunshine, with a breeze. 21. Cloudy, a. m.: some dripping at mid-day: fair evening. 22. Cloudy, in different modifications. 24. Showers: breeze at S. E. evening: misty air. 25. Misty to S.: fair, with *Cumulus* beneath *Cirrocumulus*: a *Stratus* at night. 26. Various modifications of cloud: some rain in the night. 27. *Cumulus* beneath *Cirrocumulus*. 28. Cloudy morning, the wind increasing from the westward: rain, mid-day: fair evening. 29. Wet morning: much wind at S. till evening: stormy night. 30. Fair, with *Cirrostratus*: much wind in the night.

*Tenth Month.*—1. Wet morning: much wind: lunar halo at night. 2. A plentiful dew: cloudy afterwards, with much wind: drizzling rain at intervals. 3. Cloudy, a. m.: wind N.: drizzling: in the night easterly, with misty air. 4. Overcast: small rain. 5. Fair. 6. Misty morning: much dew: *Cirrocumulus* in the superior *Stratum*, as for some days past at intervals: rain, p. m. and night. 7. At 20 minutes before one this morning a loud explosion of electricity, which kept the ground in a sensible tremor for several seconds: it was followed by thunder in long peals, and vivid lightning to the south and east for above an hour: also by much wind and rain: the day was cloudy and drizzling after. 8. Fair: mostly cloudy: *Stratus*. 9. Cloudy: breeze at E.: large *Cirri*: a few drops of rain: a well-formed mushroom was brought me, which measured 12 inches over the crown, and weighed 20 oz. 10. *Cirrocumulus* above *Cumulus*: calm. 11. Fair, save a few drops. 12. Cloudy. 13. Cloudy: breeze.

## RESULTS.

Winds Variable.

| | | |
|---|---|---|
| Barometer: | Greatest height | 30·12 inches. |
| | Least | 29·32 |
| | Mean of the period | 29·840 |
| Thermometer: | Greatest height | 74° |
| | Least | 39 |
| | Mean of the period | 55·9 |

Mean of the Hygrometer at 9 a.m. ..... 69°

Rain..... 2·35 inches.

TOTTENHAM, *Tenth Month*, 14, 1816. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

*DECEMBER*, 1816.

## Article I.

*Biographical Account of Dr. A. F. Gehlen, Member of the Royal Academy of Sciences of Bavaria.**

ADOLPH FERDINAND GEHLEN was born in the town of Butow, in Prussian Pomerania, on Sept. 25, 1775. His father possessed a considerable apothecary's shop, which at present is occupied by his brother, and where he commenced his first chemical studies. His father was likewise a landholder, and gave up the management of the laboratory to his son from a very early period. This made him speedily master of the processes, and was the foundation of the progress in the science which he afterwards made. Indeed, if we consider the shortness of his life, and the great advances in science which he had made, we must admit that his industry was uncommon, and that his views were principally directed towards the good of society.

After having acquired the elements of the learned languages in the school belonging to the place of his birth, he removed to Konigsberg, in Prussia, and there continued the study of pharmacy under that celebrated chemist the learned apothecary Hagen. Here he attended for three years the different classes of that eminent university, and to his chemical studies he united those of natural history and of languages. So great, indeed, was his progress in these, that he carried on a scientific correspondence in seven living languages with the most celebrated men in the different countries of Europe. The small-pox had considerably injured his powers of hearing; and though his invincible industry enabled him to over-

* Translated from the beginning of vol. xv. of Schweigger's Journal.

come the obstacles which this defect threw in his way, we cannot but acknowledge that his deafness rendered the great knowledge which he acquired so much the more wonderful.

After having obtained the degree of Doctor of Physic, he betook himself to Berlin, and, connecting himself with Klaproth, one of the first scientific men of his time, he continued to make still further progress in his chemical studies. Though his writings had already given considerable celebrity to his name, his extraordinary worth was known only to those who, from personal intercourse, were witnesses of the precision of his researches, and to those who were in the habit of corresponding with him. But so general was the perception of the utility of this correspondence, that he was soon raised to the first rank among German chemists.

While employed at Halle in teaching with reputation the theory and practice of chemistry in the Institute established by the Privy Counsellor Reil, the proposal was made him to go to Munich as a Member of the Royal Academy of Sciences. The Society acquired in him one of its most useful members. It is true that, in consequence of the unsettled state of the times, it was not possible for him to obtain a proper laboratory; but he made the greatest sacrifices to accomplish his objects, and his whole house was devoted to the purposes of science. The contributions which he made to the publications of the Academy, important as they are, enable us to make but a very incomplete estimate of his indefatigable scientific exertions. The experiments and observations which, by the royal regulations, fell under the charge of the Society during his time, belonged mostly to the department of science which he represented. The great alacrity with which he undertook them procured him not only the universal respect of his colleagues, but likewise repeated expressions of satisfaction from our most benevolent King.

He resigned a situation which he had filled for three years with considerable profit in the University of Breslau, because he had removed to Bavaria, and taken up his residence in Munich; and hopes were held out to him that a laboratory would be speedily built, and properly accommodated for his most useful investigations.

On account of his health, which was always weak, and which had been injured by his indefatigable exertions, he repaired for two successive years to the mineral wells at Baden, near Vienna, and was at the same time invited by chemists to repeat his important experiments on a large scale at the Imperial Glass Works. The Imperial Government was sensible of the importance of his labours; and the King of Bavaria testified his sense of his services, not barely by the most honourable outward marks, but by giving orders to begin the building of his laboratory, so necessary for enabling him to continue his exertions without interruption, at a time when the kingdom was in a state of distress from the unparalleled exertions which had recently been made.

Rejoicing in the near accomplishment of the wish which he, and all who were capable of estimating the importance of the science,

had so long formed, he found himself actuated by new vigour, and resolved speedily to undertake several scientific and practical objects which he had long had in view. His purpose was, after another visit to the baths, which were considered as beneficial to him, to consecrate his new abode, as a mark of the high estimation in which he held the Bavarian Government, for the advantage of science, and of the kingdom in which he lived, to exert himself with greater activity than ever, and to devote the whole of his life to the important pursuits of his own science.

He had been employed for some weeks in an important set of experiments on the alloys of the metals with arsenic; and this occasioned the unfortunate accident which, in consequence of his preparing and breathing only a comparatively small quantity of poisonous gas, put an end to his life on July 15, 1815, at mid-day, after suffering dreadfully for 19 days. The hopes entertained of restoring his health by the skill and friendship of his medical attendants were unfortunately disappointed. The news of his death was received by every person with the most deep-felt regret, which manifested itself publicly, and was most honourable to him.

For besides the Institute with which he was associated, many of the inhabitants of Bavaria became gradually witnesses of his useful exertions. The Agricultural Society, whose important objects he promoted to the utmost of his power, possessed in him, ever since its institution, one of its most valuable members. The Society of Apothecaries in the kingdom, which already has done so much, and which promises so much good, reckoned him among its founders and most zealous promoters. And how many learned men, artists, and trades-people, are there who applied to him as to a consummate master in the preparation of medicines, of colours, and other similar things, and who have to thank him for his useful assiduity!

How much honesty, how many hopes, how much virtue, lie in his tomb! A thoroughly upright character, which appeared in his noble countenance, by whom truth was prized above every thing—the highest sincerity in his manner of living and in his science—manly courage in opposing trick and deceit—strict sobriety—the fear of God, and benevolence towards man—the utmost disinterestedness of conduct—the most zealous attachment to his science, together with the most active attention to every thing that could improve the situation of his countrymen—these are the case which encloses his honoured image—the qualities which made him the object of esteem during his life, and of regret since his death.

His far distant brothers and relations, for whom he felt the sincerest attachment, and whom he intended to visit once more, and his numerous scientific friends in Germany, and other countries, who now read with regret in the public papers the news of his early death, may find some consolation when they are informed that in his newly chosen country he found many persons who became every year more and more convinced of his great worth, and who took every

method of making him sensible of their esteem; and that the regret of the benevolent Sovereign of Bavaria followed him to his early grave. He is perhaps the only person who, though solely devoted to the investigation of nature, yet found himself as deeply and sincerely regretted as if he had died amid the circle of his own relations. Honour to his memory.

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## *Appendix.*

The preceding panegyric was read over the grave of Gehlen when he was interred, at Munich, on July 18, 1815. It takes no notice of his writings and chemical discoveries. Perhaps it may be gratifying to the English reader if I subjoin a list of such of them as I have had an opportunity of perusing.—T.

In the year 1803, when he was 28 years of age, he became the editor of a chemical journal, published monthly at Berlin, and entitled Neues Allgemeines Journal der Chemie. He continued to publish this journal till 1806, when he changed its title to Journal für die Chemie und Physik, probably finding that chemistry alone was too narrow a field for a monthly journal. This new series continued till the end of the year 1810, when it was stopped, and Professor Schweigger, at Nurnberg, began another journal in its place. The first series of Gehlen's journal contains six volumes; the second, nine. It would be impossible to notice every thing written by the editor of such a journal, in a series of 15 volumes; but I shall give a list of the papers to which he affixed his name:—

1. On the Preservation of Hops for brewing. The hops were put into a copper still with water, and a sufficient quantity of the liquid was distilled off. A brownish red oil was obtained, which possessed the peculiar flavour of hops. The hops were now repeatedly boiled in water, and, the water being evaporated to dryness, an extract was obtained. The oil, being triturated with sugar, was mixed with the extract. This extract was found to answer the purpose of hops when added to beer. Vol. i. p. 665.

2. On preparing Spirits from Potatoes. Ibid. p. 667.

3. On native Alumina from Halle. Ibid. p. 671.

4. Remarks on Ethers, particularly on the Muriatic Ether of Basse. Vol. ii. p. 206.—This is a curious paper, and contains many facts afterwards published by Thenard as new discoveries.

5. On Fluoric Ether. Ibid. p. 351.

6. On the Quantity of Chromium in different Minerals. Ibid. p. 687.

7. On the Changes of Colour which Metalline Muriates dissolved in Ether undergo when exposed to the Sun's Rays. Ibid. p. 566.

8. Supplement to Thomson's Paper on the Oxides of Lead. Vol. iv. p. 112.

9. Description of an Improvement in the Pneumatic Apparatus, in order to prevent Absorption. Vol. v. p. 124.

10. Some Remarks on Palladium. Ibid. p. 234.

11. On Acetic Ether, Basse's Muriatic Ether, and the Relation of the Acidity of Vinegar to its Specific Gravity. Ibid. p. 689.

12. On Crucibles. Vol. vi. p. 111.—He proposes to make crucibles of steatite, and describes some trials of such crucibles.

13. On the Action of Muriatic Acid Gas on Oil of Turpentine, and the Camphor made by that Process. Ibid. p. 458.

14. Improvement in the Apparatus for extricating Gas by Solution. Ibid. p. 505.

15. On the Fusibility of Caustic Barytes. By Bucholz and Gehlen. Journal für de Chemie, Physik, und Mineralogie, vol. iv. p. 258.

16. Experiments on Artificial Iron Pyrites and Magnetic Pyrites. By Bucholz and Gehlen. Ibid. p. 291.

17. Observations and Proposals respecting the Preparation of Sulphuric Acid from Sulphur. Ibid. p. 489.

18. Observations on the Oil obtained when Spirits from Grain are burnt. Schweigger's Journal, vol. i. p. 277.

19. Some Remarks on the Change of Felspar into Porcelain Earth. Ibid. p. 447.

20. Contributions towards a scientific Foundation to the Art of Glass Making. Ibid. vol. ii. p. 88.

21. Analysis of some Minerals, with Remarks upon the Mode of Analysing Stones. Ibid. vol. iii. p. 171.

22. Experiments establishing the peculiar Nature of Formic Acid. Ibid. vol. iv. p. 1.

23. On Starch Sugar. Ibid. vol. v. p. 32.

24. On Salzburg Vitriol. Ibid. p. 333.

25. On obtaining Indigo from Woad. Ibid. vol. vi. p. 1.

26. Alteration produced on Sugar of Milk by Sulphuric Acid. Ibid. p. 115.

27. On the Gilding of Steel the moist Way. Ibid. p. 117.

28. On the Strontian in Arragonite. Ibid. vol. x. p. 135.

29. On the Electro-Chemical System. Ibid. vol. xii. p. 403.

## Article II.

*Observations on the Fire-Damp of Coal-Mines, with a Plan for Lighting Mines so as to guard against its Explosion.* By John Murray, M.D. F.R.S.E. &c. *

(With a Plate. *See No. for November.*)

[It may be proper, from circumstances, to mention, that this paper is printed in the text exactly as it was read. I have added a few notes (read before the Society at a subsequent meeting), explanatory of the plan, or connected with the subject.]

Explosions in mines from the kindling of the inflammable gas, called fire-damp by the miners, have always occasionally occurred. Of late they have become more frequent in some of the coal-mines in this country, particularly those in the districts of the Tyne and the Wear, in the North of England, and have been attended with such fatal consequences as to have forcibly called public attention to the subject. In an explosion in one mine, about two years ago, 92 persons were killed; in another, which occurred soon after, 32 lost their lives; in one which happened within these few months, 57 persons were destroyed; and recently it has been affirmed that several hundred lives are lost annually from this cause. From the state of the mines, particularly in the accumulation of wastes, the collection of water, and the increasing depth of the workings, there is reason to fear, too, that such accidents will become more frequent. Humanity loudly calls, therefore, on every effort being made to obviate the calamity; and even as a national concern, the immense loss of property in the mines, and the probability which has been suggested that the working of them must ere long be abandoned, give to the subject the highest claims to consideration.

I have to submit to the Society the account of a method which has occurred to me of lighting mines, not liable, like the common method, to the risk of kindling the fire gas, and which I trust may go far to obviate these unhappy occurrences. †

The inflammable gas which is disengaged in coal-mines, it is well known, is carbureted hydrogen. In some situations it is much more abundant than in others. It has been supposed to be produced

* From the Transactions of the Royal Society of Edinburgh.

† The production of fire-damp is much less considerable in the Scotch collieries than in those of the west or the north of England. It would be important to discover the cause of this, but it is not very obvious. Probably it arises from the smaller scale on which they are wrought. In some of them, however, it does occur, though in quantities not so considerable but that it is usually carried off by the common mode of ventilation, or by firing it as it begins to accumulate. In the mines in Ayrshire it is the practice to fire it daily. Within these few years, explosions from it have in different cases been productive of fatal accidents; some of them, especially in the mines in West Lothian and Stirlingshire, to a considerable extent.

from the decomposition of water by coal, and, in particular, from the waste coal in the old workings, exposed to the action of humidity. It is possible that much of it may be from this source; and the fact that it is most abundant in deep mines, where such wastes accumulate, is favourable to the opinion. This is not always, however, its origin. Much of it is disengaged from the solid coal as it is worked, and the surface of the wall of coal often continues to yield it from pores or fissures for weeks or months. It often, too, rushes suddenly, with great velocity, and in large quantities, from rents in the incumbent strata, or from vacuities within the mass of coal, in which it is pent up, apparently in a state of compression. The greatness of the mass of coal confining the gas more effectually, or favouring the compression with which it is retained, may be the cause why it is more abundant in deep mines. Still, from whatever source it may be extricated, if the fact be correctly stated, that it has always an intermixture of carbonic acid, it is probable that it is in all cases derived from the decomposition of water by coal.*

* No question can be more important in relation to the subject of the fire-damp of mines than that with regard to the causes of its production. The facts stated in the text prove that it is not entirely from the old wastes that the gas is discharged, though they may afford a large quantity of it. Its evolution might be considered as a circumstance in part connected with the original formation; the gas might be supposed to have been formed with the coal, to be confined by pressure in its mass, or its interstices, and to be liberated as the pressure is removed by the working. The density of the mass of coal, however, can scarcely be supposed to be such as to have confined the gas from its first consolidation, and it must, therefore, rather be regarded as a new and continued production. There is no operation from which, under this point of view, it can be derived with so much probability as from the slow decomposition of water permeating the coal; and the connexion of the production of carbonic acid with the carbureted hydrogen seems to prove that this is its origin. That water transuding slowly through a mass of coal, and existing in it in some measure under pressure, will be decomposed, is, from the consideration of the general agency of water on carbonaceous substances, extremely probable. The evolution of the same gas from marshy situations, there is every reason to believe, depends on the decomposition of water by carbonaceous matter; and the occurrence, not unfreqnent, of large masses of small coal accumulated at the mouths of the pits, and exposed to humidity, taking fire spontaneously after a certain time, can scarcely be ascribed to any other cause than to such a decomposition, and may therefore be regarded as a proof of it. There are circumstances, too, connected with the production of fire-damp which seem to prove that this is its origin. Thus it does not occur in all coal-mines; in some it is abundant, in others it is almost unknown; and this seems to be considerably dependant on the state of humidity in the coal. In the collieries in this country, for example, fire-damp scarcely ever occurs in those of Mid Lothian; while in those of West Lothian, of Stirlingshire, Fife, and Ayrshire, it is not an unfrequent occurrence; sometimes to such an extent as to have been productive of considerable explosions, and in some of these mines its evolution is nearly constant, so that it is a regular practice to remove it by firing it. I have been able to discover no cause for this peculiarity, but the comparative state of dryness and humidity. It is not owing entirely to the depth, for this differs little. In some of the mines of Mid Lothian the depth is 57 or 60 fathoms. In the Grange Colliery, in West Lothian, where an explosion happened some time ago, the depth is about 50 fathoms; and in the Ayrshire mines, the first bed of coal is at a depth of 30 fathoms; the second at a depth of 26 below this; not deeper, therefore, than that of the Mid Lothian collieries, where the gas does not occur; and, further, from the upper bed of the Ayrshire coal, fire-damp is given out as abundantly as from the lower. But the collieries of Mid Lothian are perfectly

It seems to be altogether impracticable to prevent the production of this gas. To decompose or neutralize it in the mine by any chemical agency, as has been suggested, seems to be equally so. Only two resources apparently remain: first, to employ means of discharging it, so as to prevent any accumulation of it in large quantities; and, secondly, to guard against its inflammation in working the mine, when, from circumstances occasionally unavoidable, it does accumulate, until it can be discharged.

The circumstance of explosions from the firing of this gas not occurring in many mines, situated even in districts where, from the nature of the coal, the depth of the workings, or other causes, such occurrences are frequent, seems to prove that there is no necessary accumulation of fire-damp in the mine—that the accumulation does not so much take place from unavoidable deficiency of ventilation, as from accidental obstructions to it in particular situations, or occasional eruptions of the gas from cavities in the coal or its accompanying strata, against which scarcely any system can effectually guard—that, therefore, the ventilation may be rendered effective, and, by prudent and careful management, may be conducted so as to carry off the quantities evolved. In the Felling Colliery, near Newcastle, in which the explosion that destroyed nearly 100 per-

dry; the coal being what are called *edge seams*, that is, in strata vertical, or highly inclined, a disposition which allows the water to pass off more readily. In Ayrshire again, at Borrowstounness, and at Valleyfield, where there is the generation of fire-damp, I am informed there is much water, which seems even to percolate the coal. This is particularly the case in Ayrshire, the water dropping from the wall of coal, and a current, or *blower* as it is called, of fire-damp sometimes escaping with the water. The still greater production of fire-damp in the English mines is probably owing to the much larger scale on which they are wrought, and to the deep and extensive workings being favourable to the collection of water. It accordingly appears, from the accidents which have repeatedly happened from water bursting into mines, that it is accumulated in old pits and excavations in immense quantities, and that it transudes through the mass of coal. The last accident which occurred, that at the Heaton Colliery, in which 75 individuals were destroyed from the bursting of water into the mine, is a melancholy proof of this. These causes, too, particularly the depth of the workings, favour the accumulation of the gas. This in some measure accounts for the accidents from explosion having become more frequent in these mines, notwithstanding the improvements in their ventilation, and gives some ground for the fear that its accumulation may still increase. The more numerous and extensive the excavations become, it is justly remarked, in a pamphlet published by the Literary and Philosophical Society of Newcastle, the greater will be the difficulty of guarding against surrounding wastes filled with water, or carbureted hydrogen, or carbonic acid gas; and when, at a future period, it shall be found necessary to work the lower seams in this coal-field, the operations of the miner must be carried on under immense accumulations of water. If these views be just, the propriety of impressing on the coal proprietors the necessity of conducting the workings on a better system than has hitherto been followed, will be obvious; and from the apparent indifference of many of them on this subject, the propriety of legislative interference to regulate the economy of the mines, which has been repeatedly suggested, will scarcely be questioned.

It is a curious circumstance that, in those mines in which the fire-damp does not occur, the production of choak-damp, or carbonic acid gas, is not unfrequent. Thus it often occurs in the Mid Lothian collieries, and sometimes at no great depth.

sons, about three years ago, took place, the mine, it is stated, was considered as a model of perfection in the purity of its air, from the system of ventilation; and in an account of a second explosion in this mine, in which 23 were killed, it is mentioned, "that so powerful was the stream of fresh air in all the working parts of the mine, that the candles could with difficulty be kept from going out," and "that the persons employed in it declared that they never wrought in a pit so wholesome and pleasant." In another mine, in which, in the same year, an explosion took place, in which 32 men were killed, the general arrangements were so perfect, that it was considered, by every one acquainted with the state of it, to be altogether free from danger. These facts seem to show that there is no want in the power of ventilation; and, indeed, it has been stated, on high practical authority, that in this respect no great improvement can be expected.* If these statements are correct, what is principally to be looked for, independent of employing the best method of ventilation, and of a more strict attention to the state of the mine, in preventing any partial obstruction to its operation, is some mode of security against the inflammation of the gas, either as it is discharged from the fissures of the coal in working, or when it does accumulate partially, from causes frequent, though occasional, in their occurrence.

So far as can be learnt from the circumstances of those explosions which have occurred in the Newcastle and Sunderland mines, the principal causes giving rise to the accumulation of the inflammable gas have been some neglect with regard to the means of ventilation —such as failing to keep up the fire sufficiently at the mouth of the air shaft, or obstructions in the passages or in the old wastes. The latter appear to be the most common cause; parts of the roof fall in, in the old excavations, or, by a yielding at the bottom of the pillars and walls, the sides of the passages gradually approach, forming what the miners call a *creep*; sometimes, too, the stoppings and trap-doors which direct the current of air through the passages, are neglected, the ventilation is either partially stopped or is impeded, the fire-damp gradually accumulates, and, mingling with the air, forms a mixture which is capable of exploding. There is reason to believe, too, that in some cases the accident has arisen from the sudden discharge of the gas from fissures in the strata, or from the opening of a cavity in the mass of coal in which it had been confined; it often takes fire at the candles of the miners, when discharged in this way; is sometimes discharged in large quantity with the greatest violence; and if intermingled with a rapid current of atmospheric air, the inflammation may increase in rapidity to an explosion.

It is obvious that attention to these circumstances is of the first importance; and, so far as improvements in the system of the mines in that respect are practicable, their propriety cannot be

* Report by Mr. Buddle to the Society for Preventing Accidents in Coal-Mines.

questioned. As it appears, however, that there are some causes which can scarcely be effectually obviated; and as the utmost attention which can reasonably be expected seems, under the circumstances of the mines, and the constant generation of the enormous quantities of gas which they yield, to be insufficient for perfect security, the importance becomes evident of some mode of lighting being devised, which should guard against the firing from the large discharge, or occasional accumulation of the inflammable air, while at the same time the danger should be indicated, so that the necessary means to remove it might be employed.

No difficult or complicated method can be expected to succeed. Any method, to be successful, must be simple, easy of execution and of use, and not too expensive. That which I have now to explain will be found, I trust, possessed of these advantages.

The facts on which it is in a great measure founded are, that the inflammable gas accumulates in the roof of the mine; that it is fired, in the usual mode of lighting, before the mixture of it with the atmospheric air fills the mine, or that part of it in which the accumulation is taking place; and that it cannot fill it while the mine is worked, as the respiration of the workmen would be previously affected. The miner works with his candle or lamp at a certain elevation, occasionally moving with it; and thus when the fire-damp has accumulated so far as to fill a considerable part of the roof, the accidental approach of the lamp, or some concussion throwing the gas downwards, so as to bring it into contact with the flame, sets it on fire. In one of the explosions, for example, within these two years, that of the Hall Pit, near Sunderland, in which 32 men were killed, the explosion was supposed to have been occasioned by the fall of a stone from the roof, which carried the inflammable air with it, so as to bring it into contact with the pitmen's candles; and this circumstance of a flake or mass falling from the roof, and throwing the inflammable air before it to the candles, has been often assigned as a cause of these explosions. It is a proof of what is indeed sufficiently established—the accumulation of the inflammable gas in the roof of the mine.

The method, therefore, which I would propose, is to bring the supply of air to sustain the combustion of the lamp from the floor of the mine. This may be easily done by burning the lamp within a glass case, having a small aperture at the top to admit of the escape of the heated air and smoke, and having attached to the under part of it a tube reaching to the floor of the mine to convey the air. Plate LVIII., Fig. 8, represents this in a fixed lamp.

One principal difficulty in contriving any safe mode of lighting coal-mines, must be that of having moveable lights; for these the workmen will often find it necessary, and in general will be desirous to employ. This may be easily attained by connecting with the bottom of the case or lantern a flexible tube, air tight, or nearly so, which may be done by a tube of prepared leather varnished; this tube being of such a length as to reach nigh to the floor. The

lamp can thus be held in the hand, or attached to any occasional support. Fig. 7 represents a lamp of this kind.

No danger, or scarcely any, I conceive, can arise in the use of this apparatus. If the size of the upper aperture be duly adjusted, no air can enter by it to the lamp, for the current of heated air will prevent this. And this air can never be heated so high as to kindle any mixture of carbureted hydrogen.

No inflammable air can enter from the bottom so as to be capable of kindling, for the reason already assigned, that from its levity it rises to the roof of the mine, and the mixture of it with atmospheric air, which is explosive, is always accumulated there. Nor can this increase so as to extend to the floor of the mine, and the miner remain present, as previous to this the effect of this mixed air, received by respiration, would be felt, and give warning of the danger.*

The flexible tubes of the moveable lamps may be easily prepared, and preserved air tight; and there being so ready a supply of air from below, if there were any minute fissure in the sides of the tube, no air would enter by it, or the quantity would be so small, and so much diluted by intermixture, that there could be no risk. In the fixed lamps, having an iron or copper tube conveying the air, there could be no risk of this kind; and if it were necessary,

* It has been said that the inflammable air sometimes issues from the floor of the mine; and this has been stated as a sufficient objection to the method I have proposed. The fact is, that it seldom comes from the floor, but usually from the sides of the wall of coal; and in general even the discharge is rather from nigh the roof than from beneath, as must indeed be the case in the escape of an elastic fluid from an imperfectly solid mass. But even if it did *issue* from the floor much more frequently than it does, it does not *remain* there, but rises to the roof, where the accumulation of it, and the mixture of it with the atmospheric air which renders it explosive, uniformly take place, as all the facts connected with the state of the air in the mines prove: nor can any accumulation of it take place, which shall reach the floor of the mine, but by its filling the space from the roof downwards, mixed more or less with the atmospheric air. All that is necessary, therefore, is to guard against the chance, extremely small in itself, of the open end of the tube being in the direction of a stream of the gas, if at any time it should issue from the floor; and this is easily done by the methods stated in the descriptions, of the tube being turned up at its extremity, or of its being closed for the height of two or three inches, with apertures above this height, to admit the air. Any small quantity which might be brought by the current of air entering the tube must be unimportant; and any danger from this source must require such a combination of circumstances as may well be expected never to occur—that of the tube being in the direction of the current of gas—of the mixture of it with atmospheric air being in that limited proportion when it reaches the flame of the lamp in which it explodes, and of the whole air at the floor of the mine being also in that state in which it will explode; and all this independent of the circumstances that, by any such mixed air passing into the lantern, the flame of the lamp will be extinguished instead of explosion happening, and that explosion, even if it did occur, would not be conveyed along this length of tube.

The same arrangement, with regard to the tube, obviates another possible inconvenience—that of the entrance of carbonic acid gas, which, from its greater specific gravity, may sometimes occupy the floor of the mine. It seems scarcely ever to be accumulated to this extent in mines in which fire-damp is generated; and if it were, its entrance into the lantern would be productive of no other accident than that of extinguishing the flame. But even this is easily obviated, by admitting the air at any height from the floor which may be found requisite.

similar metallic tubes, with moveable circular joints, could be adapted to the moveable lamps.

There is another circumstance which will give security to these lamps, should it ever happen, from any unforeseen cause, that a mixture of inflammable air were introduced—the rarefaction of the air within the lamp, and especially near the flame. It is well known that mixtures of inflammable air with atmospheric air, or even with oxygen, cannot be inflamed if the elastic fluid be in a certain degree of rarity. The experiments of Grotthus with regard to this are important. They prove that the combustibility of the inflammable gases is so much dependant on their density, that if a mixture of any of them with oxygen gas be rarefied to a certain extent, either by the air-pump, or by elevation of temperature, it could not be kindled by the electric spark which kindled the same mixture easily in its denser state. Hence, as he justly remarked, bodies may be inflammable under pressure, the inflammability of which is weak, or not apparent in a rarefied atmosphere; and in mixtures of different inflammable gases with atmospheric air, there will be a certain degree of density within which only the mixture can be inflamed. The inflammability of any mixture of carbureted hydrogen with atmospheric air is limited to certain proportions, and in all of them is inconsiderable. Dr. Thomson states, what is a proof of this, that he had never been able to cause any mixture of it with atmospheric air to explode, it merely burnt rapidly; its exploding in the mine must, therefore, probably be owing to the large mass of it inflamed, and to the state of condensation in which it exists. Another circumstance, which shows that even in the mine its power of kindling so as to explode is not more than what just renders it possible, is, that it is not kindled by the ignited sparks from the collision of steel and flint, a machine producing these being used to give light in working or exploring the mine, when much danger is dreaded, and having very seldom caused explosion. Thus coming barely within the verge of the power of exploding, and owing it to these circumstances of quantity and condensation, there is every probability that, if kindled in small quantities, it would not explode, and that, presented to an ignited body, much diluted, and in a rarefied state, it would not even inflame. Hence the chance of its inflaming in a lamp such as that described is inconsiderable, were it even admitted to the flame; and the certainty of its not exploding might almost be depended on. The security from this might even be carried so far, by adapting properly the size of the upper aperture, so as to produce the greatest degree of rarefaction in the air, that if a mixture of the fire-damp with atmospheric air were introduced, it might, instead of inflaming, become incapable of supporting the combustion, or at least might so far weaken the flame as to give indication of the danger.

Lastly, if even, from some singular cause, an explosion did happen within the case of the lamp, it appears to me very doubtful if it would be propagated further. It must be extremely feeble.

The flame or ignition could not be communicated by the upper aperture to the air without, partly from its smallness, and partly from the upper part of the lamp being previously occupied by air not capable of kindling. Nor could it be easily communicated downwards through the tube, especially in a flexible tube, the sides of which would first yield, and then collapse; and though conveyed downwards, there would be little or no probability of an air occupying the floor of the mine capable of being inflamed. The risk of such a communication, if it were thought there were any, might probably also be diminished by conveying the air rather by several small tubes than by one larger. Other contrivances sufficiently simple, if they were supposed necessary, might be employed. A cup containing water, for example, or a saline solution, might, in situations peculiarly hazardous, be suspended within the case, over the orifice of the air tube, which, if any explosion were to happen, would by the agitation throw out the fluid, and extinguish the flame.

If, notwithstanding all these means of safety, there should in any particular case be any dread of danger from the admission of inflammable air with the common air, this might be completely obviated by an additional arrangement. The air to supply the lamps might be brought by a cast-iron pipe from any part of the mine where the danger did not exist, or, what would give entire security, from the bottom of the shaft, where the air must be pure. A pipe or pipes of this kind running through the principal passages, small upright tubes might arise from it at convenient distances to the fixed lamps. A similar mode might be extended even to the moveable lamps; the flexible tube attached to the lamp might be of such a length as to reach to a part of the mine where the air was known to be pure; or such flexible tubes might be adapted to branches fitted with stop-cocks, and communicating with the main trunk. Thus a system of perfect security, I conceive, would be attained. Independent of the other circumstances diminishing any hazard, there are here only two modes of communication with the external air, from neither of which can any danger arise—by the upper aperture and the lower tube. The former can admit no air to the lamp within, and the latter must convey merely pure air. Both, therefore, must be perfectly safe.

I have stated these diversities of method merely to show how far the plan may be carried, where particular situations require it, and absolute security attained; but it is very probable that in general they will be unnecessary, and that the simple mode first explained, of a tube connected with the lamp supplying air from the floor, will be sufficient.

This method, in its simplest form, affords another security, that of avoiding the igniting of any stream or blast of inflammable air, as it issues from the coal. This it often does suddenly, and with great violence. A current of this air being kindled in the common mode of lighting the mines, by the approach of a candle or lamp, is a frequent cause of explosions, the stream of flame extending to the

mixture of inflammable air and common air in the roof of the mine, and causing it to explode. The inclosing of the lamp in the case of course prevents any accident of this nature.

By these arrangements, adapted more or less to circumstances and situations, a system of lighting mines may be established, I trust, perfectly safe, with any common care. And the extreme simplicity of the plan, facility of execution, and economy, are recommendations in its favour.

It is scarcely necessary to enter on the details of the modes of construction of the apparatus, as these are both obvious, and admit of considerable diversity. I have given what appears to be the best figure of the glass case for a fixed lamp, sufficiently wide to prevent it from being broken by the heat, and not too much so, so as to lessen the current and rarefaction of the air. The aperture, too, is adjusted to the same purpose. If a lamp with oil be employed, it can either be suspended from the top, or fixed on a socket from beneath; if a candle be used, it must have a socket, which it may be requisite should be a sliding one, to adjust it to the due height as it burns down. The glass cases may be protected from external injuries by a wire netting. Fixed lamps will in general be less exposed to risk than moveable lights, and by employing them in sufficient number, few of the latter may be required. In all the passages of the mine the former may be employed; and in all cases where fixed lamps with a metallic tube reaching nigh to the floor can be introduced, the method is more simple, and is attended with less trouble, and requires less attention than any other that can be used.

Some precautions may be necessary in kindling the candles or lamps. In the moveable apparatus this may be done at the bottom of the shaft, or any other part of the mine where the air must be pure. The fixed lamps may be kindled in a similar manner, being supplied with a flexible tube at the bottom, to be removed when they are transferred to the fixed tube, and, if necessary, with the additional precaution of stop-cocks to each. Various arrangements with regard to these will readily occur.*

* To the observations in the text, on the construction of the lamp, a few details may be added.

The principal circumstance requiring adjustment is that of the size of the aperture by which the heated air escapes. If it be not sufficiently wide, the flame is faint, and on the movement of the lamp becomes unsteady, and is liable to be extinguished. If it be too wide, there may be some risk of a current of air entering the lantern by it, especially if the under tube be not sufficiently wide, by which the whole security from the method would be lost. The due size is most easily found, by affixing to the aperture at the top a conical tube, and cutting this down in successive trials, until the diameter is attained at which the flame is steady and bright. It is not easy to give a precise dimension, as the flame is dependant on the breadth and height of the wick, the purity of the oil, and the state of the air; but I find that in a lantern of the size of the moveable one mentioned in the text —five inches in height by three in width, with a flat cotton wick three-tenths of an inch broad, and burning so as to consume about two ounces of oil in six hours, the diameter of the aperture being a very little less than half an inch, admitted of the flame being steady and bright when the combustion was fully established. In the mine it may be required to be a little larger. When the lamp is kindled, if re-

I have said that the accumulation of the carbureted hydrogen in the air of the mine may be discovered by its effect on respiration.

mains for a minute or two more faint, and if moved hastily in this state is liable to be extinguished. One aperture is preferable to two or three smaller apertures, as there is less risk of any counter-current; and to guard also against this in any movement of the lamp, it is proper to have the opening in the form of a short tube. The heat of the air issuing from an aperture of this size, with a flame such as has been described, I found to be 370°, the bulb of the thermometer being in the current exactly at the orifice; when introduced entirely within the aperture, and immediately above the flame, it rose to 465°. Air of this temperature, it is obvious, cannot inflame fire-damp, or any mixture of it.

No very accurate adjustment is required with regard to the size of the tube conveying the air into the lantern. It is sufficient to have it wide enough. The state of the flame is then regulated entirely by the size of the upper aperture, and any slight excess of width in the tube beneath is of no importance. I find that with a lantern and lamp of the above size, a tube three-fourths of an inch in diameter interior measure, and 3½ feet in length, answers very well. This circumstance, of no accurate adjustment being necessary, gives an advantage to this method in actual use. Where the safety of the lamp depends on such an adjustment, it is difficult to construct it in such a manner that it shall burn with a bright flame, and steadily, so as not to be liable to be extinguished by movement, or by the least failure either in the current of air, or in its purity. The bringing the air from the floor renders any such adjustment unnecessary, and allows, therefore, of a more bright and steady flame being produced with entire safety. And by the lamp being thus always supplied with the purest part of the air, it will continue to burn where any safety lamp on a different principle must be extinguished, and of course will enable the miner to work in situations where no other will be of any use.

In fixed lamps, the length of the tube must be regulated by the height of the passages. The thickness of the two beds of coal at Newcastle is about six feet each. But it is unequal, and in some places is not more than four feet. On the other hand, the occasional falling of the roof forms dome-like cavities above this height. It is in these that the inflammable air chiefly accumulates, and in the lower passages or workings, where they are open, the draught of air must in general prevent it from being collected. Hence the limits to the elevation of the lamp, and of course to the operation of the principle on which the method is founded, are less than they at first appear; though even the height of three, or 3½ feet, from the floor, while it is probably best adapted to the necessary illumination, will give the requisite security.

A lamp with oil is more convenient than a candle, as requiring no adjustment with regard to the wick; and by the common contrivance of a plate with a screw on the aperture, the oil is prevented from being spilt, on any occasional inclination of the lamp. The usual time of a miner's work is six hours; the lamp, of the size just now mentioned, with fresh oil and wick, burns seven hours. The miner, therefore, may take it with him newly trimmed, and the lantern need never be opened in the mine, by which any risk from the communication of its flame to the surrounding air may be avoided. If it were necessary that it should burn for a longer period, it might, without any inconvenience, be made of a larger size; and the wick might be made so as to admit of being raised by a contrivance similar to that of Argand's. All the joinings of the case or lanthorn, it is obvious, ought to be as close as possible.

By employing a metallic lantern, with a lens of very thick glass in front, the risk from breaking, which is incurred when a glass case is used, is avoided. This construction has other advantages. It affords a great deal of light in the most favourable manner; the illumination being directed with less loss on the space where it is required. Where the situation admits of it, a lantern of this kind can be made to project light in a straight line to a great extent, and illumination may be thrown into a passage, or along the side of a wall which is to be worked, by its being placed at one extremity. This may not only have the advantage of economy, but of greater safety, so as to render this method proper to be employed on this account alone, independent of any other consideration; for the lamp being placed at some distance from the miner, the risk is avoided from the concussion arising from his working, from any fall from the roof, or from the sudden discharge

Its deleterious agency is well known. At the same time, as in the greater number of situations, its addition to the air must be gradual; it will not exert its full deleterious power, but produce only such effects as will give warning of its presence.

Even before it acts this far, it will be apparent by its smell. Hydrogen in a humid state has a sensible odour. The fire-damp in mines is known by its smell; the miner, in judging of its presence, always advancing with considerable confidence when no smell is to be perceived; and this criterion must become still more evident, when the person exposed to it is guarded from its early explosion, and it is thus allowed to accumulate to a greater extent. It is also sometimes apparent to the eye, by the vapour which is diffused through it, forming a kind of mist, floating under the roof of the mine, and fluctuating with every movement of the air.

There is one other circumstance which has been employed as a criterion, though, in the usual mode of applying it, it seems to be a very hazardous one—what is called the *candle top.* This is a yellowish-coloured diffusion of the light round the flame of the candle, rising higher, and assuming a greenish-blue colour when the quantity is considerable; and when it is still larger, giving rise to a rapid succession of luminous points or flashes. The miner, when judging from this of the presence of fire-damp, advances cautiously in the mine, observing the appearance on his candle, by raising it slowly from a certain height from the floor—a circumstance, I may remark, which shows very well the tendency to the accumulation of the inflammable gas in the roof, and the comparative purity of the air below. He thus advances as far as he can with safety, and it is singular to what length the miners sometimes proceed with this trial. This peculiar appearance seems to arise partly from the extinguishing effect of the reduced air on the flame of the candle; for a similar effect is produced in immersing a lighted candle slowly in any gas unfit to sustain combustion; and as it extends so high, it must also partly arise from the imperfect ascension of the inflammable gas. If any inflammable air were to enter the lamp I have described, this appearance, or something similar to it, would take place, even sooner, from the rarefaction of the air, and probably the flame would be entirely extinguished before any explosion occurred. It would, therefore, give an equally sure indi-

of the inflammable gas from any opening in the coal. Illumination may also be thus obtained, in situations where, from imperfect ventilation, it is difficult to support the combustion of a lamp, such as the close extremities of passages or of new workings. These are the very places, too, which are more peculiarly liable to the accumulation of the inflammable air; and in both respects, therefore, the advantage is obvious of a mode by which, where the direction of the working admits of it (which it will almost always do to a certain extent), light may be thrown from a distant spot, where the same difficulty and the same hazard do not exist. Lastly, in ascertaining the state of the air in passages where danger is suspected, or in exploring them after the accident of an explosion, the same method will give greater safety. Where a more diffused light is wanted, this is easily attained by the surface of the lens being more or less scratched.

cation with much more safety; though it is also probable that no air so far impregnated with carbureted hydrogen could ever enter from the floor of the mine while the atmosphere above was such that it could be breathed.

When the danger is suspected to exist, the state of the air in any part of the mine may be accurately examined with great facility and entire safety. With a moveable lamp and tube, such as has been described, or, if it were thought to give more safety in extreme cases, with light obtained by the collision of steel and flint, in the *steel-mill*, as it is called, a person could advance to any spot, empty a bottle of water there at any particular height from the floor, cork the bottle, and immerse the mouth of it inverted in water. A portion of the air would thus be withdrawn, and at the bottom of the shaft, or at the mouth of the pit, it might be tried whether it were explosive or not. Even if it did not kindle, the degree of the approach to danger from the intermixture of a certain portion of the inflammable air might be ascertained. That a mixture of carbureted hydrogen and atmospheric air should be capable of being inflamed, they must be in certain proportions to each other. Not less, as Dr. Thomson has remarked, of the carbureted hydrogen than $\frac{1}{12}$ of the volume of common air must be present; and when it exceeds $\frac{1}{6}$th of the volume of the air, it ceases to explode. All mixtures in the proportions between these will explode, but beyond these extremes will not. If the air withdrawn from the mine for examination does not explode, it may be discovered how near it is to the first proportion at which this will happen, by adding to different portions of it, certain proportions either of hydrogen or atmospheric air until an explosive mixture is formed. If the addition of a small proportion of hydrogen, for example, were sufficient for this purpose, this would indicate the near approach to danger, by showing that a very little further intermixture of fire-damp would render the air in that part of the mine explosive. An excess of hydrogen, if it were present, would always be hazardous; for although it might not form, properly speaking, an explosive mixture, still it would be inflammable. These experiments are so simple, that the more intelligent miners or superintendents might be easily taught to perform them. One of the most frequent causes of the unfortunate accidents that have occurred seems to have been the want of any proper method to ascertain the extent of danger. In many of them the accumulation of the fire-damp was suspected; it was in trying to ascertain this that the explosion happened; and it is astonishing to observe, in some cases, the extreme imprudence with which this was done, by approaching with a common candle. In mines peculiarly liable to such accidents, it might be well to have a regular system of making such trials at stated periods. And this is more necessary when it is considered that all methods of lighting that may be proposed are, strictly speaking, only calculated to lessen the danger from accidental firing of the gas; and that, in one point of view, they are a source

of hazard, as giving the idea of greater security, and being liable, therefore, to lead to less strict attention to ventilation.

When the accumulation of gas to a dangerous extent is ascertained, it may be drawn off by various methods. A communication may be formed with a part of the mine in a state of thorough ventilation, and the rapidity of the current of air might be increased, or the foul air might be pumped out by a steam-engine, or by an exhausting machine, such as that proposed by Mr. Taylor,* brought to act on any particular part.†

It is not necessary, however, to enter on the details of the system, which, with regard to several of the contrivances, are indeed sufficiently obvious, and which might further be varied by local circumstances, and be improved by a knowledge of these, and by experience. My object has been merely to state the general method, and explain its principles, with any collateral observations which appeared to me to be of importance.

* Thomson's Annals, vol. iii.; or Philosophical Magazine, vol. xxxviii.

† When the workings of a mine are carried to a considerable distance from the course of the current of air, without a corresponding shaft being made, the ventilation becomes very imperfect. In this case it is stated in a very candid communication by Mr. Scott (Edinburgh Journal, Dec. 1815,) that the lamps burn with more brightness near the floor than near the roof of the mine: the lamp, therefore, with the tube, will be adapted to such situations, or at least the circumstance of the air being brought from the floor will counterbalance any obstacle from its being inclosed, and will allow it to burn as well as an exposed lamp would. In the Scotch coal-mines in general, the system of ventilation appears to be even less perfect than it is in the mines in the north of England, probably from the same necessity not having existed, for rendering it equally perfect, as the production of fire-damp is so much less abundant. The circulation of the air in them is often so languid, that a method of lighting by a close lamp would perhaps be attended with difficulty, at least if it were necessary to place the lamp in such situations, which, however, the contrivance of employing a lantern with a lens in some measure would obviate. The English mines present the combination of a more perfect ventilation with the constant production of enormous quantities of fire-damp. The object, therefore, is to guard against explosions from the accidental accumulation of the gas, while, at the same time, the general plan of ventilation admits of this being more easily carried into effect. A singular method, which shows both the imperfect ventilation, and the moderate extent within which the gas is generated in the Scotch mines, has been practised—that of firing the quantity accumulated at a stated period—in some mines daily. A miner enters the mine with a lamp inclosed in a lantern; he advances as far as is proper, holding it as low as possible, and lying down on the ground, or sometimes even in a trench dug in the ground; he removes the lamp from the lantern, and raising it, or advancing it towards the closed extremity of the working, where the gas is collected, fires it. Mr. Scott, in the communication referred to, proposes a plan of firing the gas as it collects, by a lamp of a particular construction, suspended nigh the roof. This would be safer than the old method, by which the workmen are sometimes injured, and which, whenever the production of the gas becomes considerable, is evidently impracticable. But the exhausting machine noticed in the text may always be applied with sufficient effect, and where the gas is slowly collected must be preferable to any other method where a current of air cannot be completely established. In the Hurlet mine, near Paisley, it has been employed with entire success, and on so small a scale that it is worked with a hand pump. The cylinder exhausting the air is 23 inches in diameter, and it makes a 13 inch stroke 13 times per minute; it discharges, therefore, in that time, 40 cubic feet of air. It is worked by a boy, and only as it is required. Tubes of tinned iron are connected with it, which are prolonged as the excavation extends.

It is further obvious that the mode of producing illumination by coal gas may be connected with this method, at least so far as regards the fixed lamps. The gas might be prepared by a furnace, at a part of the mine where there would be least hazard from carrying on the operation—at the bottom of the air shaft, for example, where a strong current of air moves outwards, and the gas might be conveyed through a main trunk to pipes terminating by a small aperture within the glass case. Its combustion, as it issued from the aperture, might be sustained with safety by the current of common air, supplied in the mode already described. A very brilliant illumination might thus be obtained; and from the peculiar advantages of situation, this might be done so economically that it might render moveable lamps, to which the method could not well be applied, unnecessary. It might even become a source of advantage, by getting rid of much of the waste coal, and converting it into coke. It is possible, however, that the attention required in the process, and its interfering with the operations of the mine, might, independent even of any supposition of hazard, render the propriety of its introduction doubtful.

The general method, I may add, will prove equally effectual in obviating the danger from *choak-damp*, carbonic acid gas, the other evil which miners dread, and which often also occurs in caverns, subterranean passages, and other situations. From its specific gravity being so much greater than that of atmospheric air, it is known to remain nearly at the floor, and to extend very gradually upwards. A person, therefore, may advance with safety, where it is present, by the precaution of holding in the hand a candle within a glass case, having a tube attached to it supplying air from near the floor. The supply of air to support the combustion being thus from beneath, the presence of carbonic acid gas to any hazardous extent would soon be discovered, by the flame of the lamp becoming fainter, and being at length extinguished, while the respiration of the individual would not be affected: and by raising the open end of the tube to different heights from the ground, the extent to which the atmosphere of carbonic acid reached would be ascertained. By establishing in mines in which the choak-damp is liable to occur, a system of lighting similar to that which has been described, the danger from it would be effectually obviated.

## *Description of the Plate.*

Fig. 8 represents the fixed lamp.

A, the glass case within which the candle or lamp is placed in a socket, with the aperture at the top, of a sufficient size to admit of the escape of the smoke and heated air.

B, the tube of tinned iron, or of copper, which enters beneath the socket, conveying air from the floor to support the flame. To

show the length, it is represented in two parts, and at the under extremity it is turned up to the height of three inches, to attain the advantages explained.

Fig. 7 represents the moveable lamp.

A, is a lantern of tinned iron, five inches in height, and three inches in width, with a glass lens, *a*, three inches in diameter, projecting half an inch in front. The lamp, *b*, is introduced at an opening behind, which is closed with a cover secured by a wire, passing into a small groove or tube, as represented at *c*; *d* is the aperture in the small dome at the top, by which the smoke and air escape; *e* is a small projecting plate to disperse the current of hot air; *f*, the handle, which rises from the double back.

B is the tube of leather, with a spiral wire within, to prevent its compression, which conveys the air to support the flame; three fourths of an inch in diameter, and from three to four feet long, and represented in two parts, to show its length. It is adapted by a screw to the short projecting tube, *g*, at the bottom of the lantern; and the lamp within resting on a plate at the height of half an inch from the bottom, the air enters beneath this, and rises by its sides. To the under end a tin tube, *h*, is adapted, closed at the end, with apertures in the sides, at the height of about three inches, to admit the air; this having the same advantages as the turning up of the tube in the former figure, and in a moveable lamp being more convenient.

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## Article III.

### *Mr. Longmire's Answer to John George Children, Esq. on the Wire-gauze Lamp.*

(To Dr. Thomson.)

SIR, *Whitehaven, Oct.* 17, 1816.

In your last number I met with an answer to my remarks on Sir H. Davy's lamp, by John George Children, Esq.

Mr. Children, it seems, is a man who comes to *rapid conclusions*; and, having imagined that you had prevented the publication of his paper, he gets all at once ill affected towards you, and absolutely indignant at me, if we are to judge from his paper (in the Phil. Mag. for the last month) on J. H. H. Holme, Esq.'s remarks on Davy's lamp. Mr. Children subscribes himself "*sincerely* yours;" but, after some delay in the publication of his paper, he goes over at once to the Editor of the Phil. Magazine.

Mr. Children, Sir, begins by telling us that, as you did not entertain, like him, a high opinion of Sir H. Davy's lamp, he was not surprised to see my remarks in your journal. Mr. C., in this instance, does not give you much credit for your impartiality as an Editor. And, according to his way of thinking, you ought now,

as your opinion of the lamp is changed, to prevent the publication of all papers hostile to it, should they even come from your professed correspondents.

Mr. Children says he will hardly be accused of making personal attacks. I must confess *I was much surprised* to observe so little invective in his paper in the *Annals*, and was pleased to meet with, in him, so *singular* an exception to that *great* and *general* want of *urbanity* exhibited by Davy and his intimate friends on this occasion; but when I had seen the Phil. Mag. for the last month, I found Mr. C. was not so well entitled to be considered such an exception.

In the paper to which Mr. Children alludes, I doubted the *absolute* security of Sir Humphry's wire-gauze lamp. Davy has lately furnished me with materials of defence; which, though I have plenty in reserve, I consider quite sufficient to convince the public that I was right in my assertions. Sir Humphry tells us, in the Newcastle Courant, that "in one instance he found a lamp without a second top;" and such are his fears as to the consequences, that he considers it a "gross and unpardonable instance of carelessness in the maker, who, if any accident had happened, would have been guilty of homicide." Does not Davy in this instance confirm what I predicted, when I said, "If we suppose the lamps to be *always* constructed properly, *which might admit of a doubt*;" and does he not give me a proper justification for saying that the lamp would not always be completely constructed.

Sir Humphry, in the same paper, informs us "that the wire-gauze in several lamps in the collieries, which had been in use six months, and cleaned by careful workmen without being removed, was as good as new; whereas the gauze in some, that had been used for a much shorter time, *and taken out of the lamp*, (how comes this, Mr. C.?) and cleaned roughly, was *injured at the bottom* (a general complaint!), and if not actually *unsafe, becoming so.*" This quotation is a proof that I had sufficient grounds to doubt the perfect efficacy of the lamp *in practice.* Still, had Sir Humphry introduced the lamps to the notice of miners with *due caution*, I should not have annoyed him and Mr. Children with my remarks; but as Davy and his followers chose to stile them perfectly safe lamps, and to make a world of unnecessary parade about them, I thought it a duty which I owed to my brother miners to put them on their guard against such quack-like proceedings. Every month's experience adds to my conviction as to the necessity of the measure; but if the sequel had turned out contrary to my expectations, I should not have blamed myself, as I had the good of the miners at heart.

Now the public shall be the judge between Mr. Children and me. If they think my paper on Davy's wire-gauze lamp "a weak attempt to injure" this lamp, they will "let it perish in its own insignificance," (as Mr. C. once intended to do;) but if they do not find it such an attempt, but one to put the mining world on their guard against the deceptious language of a person with a high

name, they will remember it and its author with some respect as long as the lamp is known, either as an useful instrument to miners, or as a *curiosity*; and not give any thanks to Mr. Children for bringing forward unworthy remarks against a person who was rendering miners an important service. I will stand by the verdict of the public!

Mr. Children says, "Had Sir Humphry ever asserted its absolute perfection (meaning construction), he would have merited Mr. Longmire's attack." I did not, nor do not, say, that Davy has ever asserted the perfection of construction according to the *letter*, but he certainly has according to the spirit of the meaning; for when he says, as he often does, that his lamp is perfectly safe, its complete construction is a very obvious inference, as is its perfection in practice. That Davy has not given over publishing high notions of the lamp's security, may be seen from the following paragraph: he says, "The experiments above detailed are the first that I have made on *currents* of fire-damp;" (indeed!) "They prove what I had inferred from its other properties" (I doubt it); "and they offer simple means of rendering wire-gauze lamps *perfectly safe* against *all* circumstances," (mark you the absolute "ALL!") "however *extraordinary* and *unexpected*," (wonderful!) "and of placing their security above the *possibility* of *doubt* or *cavil*." (No more comfort to poor "pseudo-philosophers!") This is very strong and very conclusive language; and if we had perfect reliance on Davy, *in his boasting moments*, it would be decisive. At any rate, according to Mr. Children's view of the subject, it adds considerably to my means of justification, if I had been in want of it. The paragraph from which this quotation is taken contains in itself the essence of all Sir Humphry's previous boastings, and ludicrous display of infallibility; and of his threats and stigmas so profusely dispersed on every side of him.

Davy began by considering thin, uncovered, and unprotected glass, aided sometimes by moist pipeclay, horn, asbest, &c. as perfectly safe barriers between coal-miners and destruction. The glass lamp is a fair specimen of his *original* notions of *absolute security*; but after Stevenson's perforated metallic lamp became known, these notions altered; and then wire-gauze was considered safer than the perfectly safe glass lamps. Single wire-gauze lamps reigned in Davy's head for some time, in perfect safety, as the means of absolute security to colliers; but they have given way to double wire-gauze lamps, which may be called the doubly-secure perfectly safe lamps. Should this change go on, "step after step," before he gets to the desired, ALWAYS BOASTED OF, and *real*, absolute security, it will be difficult to determine of what kind of materials the lamp must be made; but probably he must have new materials, and also a new mode of construction. Mr. Children argues that the imperfection of materials should not be urged against Sir Humphry. This would be proper did Davy lessen his pretensions to suit contingencies; but so long as his ideas soar as high as those in the para-

graph last quoted, he cannot but take upon himself not only the risk of accidents by imperfection of materials, but also from the inadvertencies of colliers. It is not likely that Davy will cease to talk of absolute security so long as such tempting things as the civic crown, national distinctions, &c. are held out to him by his—complimenters: for nothing but that security will entitle him to these great rewards. Besides, if he ceases to talk of giving perfect safety, he would let himself down to the level of ordinary men; and be no better than the very colliers, to whom he is holding out a universal preventive of accidents by fire-damp; for these colliers have all along had a system of ventilation, which, to reason as Mr. Children does, might be called perfectly safe (notwithstanding what has lately happened), as they can obtain for a time an *uninterrupted* current to sweep away all the fire-damp without injury to others. This is quite sufficient in the new way of security; and the crushing down of stops, and accidentally opening of doors, the usual sources of accident in ventilation, are only equivalent to the neglect of using lamps with single tops, or the error of injuring them by rough usage. Now as Sir Humphry cannot admit that his lamps are subject to the same contingencies as those of other men, we find that he is forced to admit their *want of safety* in both construction and practice, while he would make us believe he has placed "their security above the possibility of doubt or cavil."

Mr. Children endeavours, Sir, to show in his second paragraph that any enlargement of the apertures by the wearing of the wires will not produce accident. Let us now, Mr. Editor, take this question into consideration. Mr. Children argues from Mr. J. Murray's experiments. Mr. Murray extends the distance of safety between the wires to $\frac{1}{8}$ of an inch; Mr. C. therefore concludes that a wire may be removed from between every two wires of those which are placed at $\frac{1}{20}$ of an inch apart, before the flame will pass; and he "humbly suggests" that the lamps must have a little more than "*begun to wear*," before danger can ensue from this cause. I am very willing to let Mr. Murray's scientific attainments stand on the footing that Mr. Children has introduced them; but I cannot help saying that the error which Mr. C. has detected does not add strength to his own recommendation; and if he meant that Mr. M.'s evidence should appear with all its force, he ought to have checked, *in this instance*, his wish to expose the *small errors* of his contemporaries. But Mr. Children, I see, will lay hold of trifling advantages at the expense of *every* person—except Sir H. Davy.

I reasoned from the experiments of Sir Humphry, who, at the time I wrote, had limited the size of an aperture to $\frac{1}{20}$ of an inch on the side, and considered any larger size to be dangerous. If Sir H. Davy grounded his assertions on accurate experiments, my reasoning retains its full force; but if the contrary can be shown, then it is of no avail. I certainly join issue with Mr. Children, in this instance, with some degree of diffidence; because I have very lately read over the most of Sir Humphry's experiments on this

subject with much attention, and I met with mistakes which I did not expect to see in the works of one who is said "to be so skilful an experimenter, and one so deeply informed in chemistry, both theoretical and practical." Thus Davy, in the advertisement to his pamphlet, informs us that "wire-gauze of 3000 apertures in a square inch was sufficiently fine to prevent explosions, used in a cylinder, but did not bear the proof of a concentrated explosion from a close glass vessel." But in his late communications he says, "I have never been able to pass the flame of coal gas, or any other *carbonaceous gas*, through wire-gauze having more than 1600 apertures to the square inch, *by any means*." Here there is either a great mistake in the experiments, or a great deficiency of memory. Sir Humphry says, in his experiments, that red-hot iron bars did not ignite fire-damp; but now he is afraid to trust small wires; yet if bars will not set the gas on fire, there cannot be any danger from wires. Lastly, Sir Humphry tells us that if fire-damp be mixed with air in the proportion of 1 to 14, the mixture will be fired by a candle; but Mr. Buddle's practice is much at variance with this result. Mr. Buddle finds that from 525 to 700 hogsheads of air "will dilute and render harmless" from 170 to 230 hogsheads of fire-damp. According to this statement, a candle will fire a mixture of five parts of air and one of fire-damp, instead of 14 of air and one of fire-damp, as Sir Humphry says. Thus there is as great a difference between Sir Humphry and Mr. Buddle as between Sir Humphry and Mr. Murray. What am I to do in this dilemma! None of these *high authorities* are to be dissented from with impunity! If I prefer Sir Humphry's experiments, I shall displease Mr. Children; who says, "few will feel inclined to treat lightly Mr. Murray's opinion on scientific subjects, especially in the department of chemistry." If I decide against Mr. Buddle, I shall also incur Mr. Children's displeasure, for he thinks very highly of Mr. Buddle's practical knowledge. And if I find fault with Sir H. Davy, Mr. C. will say I am actuated by "obstinate prejudice, or a worse feeling." I find myself involved in uncertainty, and filled with doubt; for as soon as I begin to contradict—danger may ensue! But I must run all hazards. As Sir Humphry has lately informed us that a slight motion will pass the flame of coal gas through apertures under 400 to the square inch, we may suppose, till decisive experiments confirm or contradict it, that the flame of fire-damp will be forced through similar openings, as the passage of the flame of coal gas seems to be made by mechanical force. Now as uncertainty commences in 400 apertures to a square inch, any one of which cannot be more than $\frac{1}{35}$ of an inch on the side, I think danger will ensue when the apertures have got above $\frac{1}{20}$ of an inch on the side, or about 250 in a square inch. But Davy and Murray must settle this affair between them.

With regard to the difference between Sir H. Davy and Mr. Buddle, we might release Sir Humphry from the suspicion of error, were we to reason as some have done; for Mr. Buddle belongs to

that "most obscure and humble class of people," the colliers, as they are called by Sir Humphry's friend, the Edinburgh reviewer; which colliers, this gentleman says, "are the furthest removed from the influence of science." Hence Mr. Buddle must have come to practical results in the rule-of-thumb way, and cannot be supposed to be very correct. Should any person start the objection to this argument, that Mr. Buddle is an "*enlightened*" miner, and cannot err, I answer that, when he made this great mistake, he was not *possessed* with the *new light*.

So much, Mr. Editor, for the apertures.

Mr. Children, Sir, commences his third paragraph with a quotation from my remarks, which he calls very obscure, and says it can *scarcely* be called English. As there is no intermediate style between the English and not English expressions, I think Mr. C., while he is finding fault with me, expresses himself "very obscurely." Mr. C. may, if he thinks proper, point out the supposed defect in this sentence, and his doing so will enable me to avoid a similar error. In the mean time I will show him that this quotation, if it be considered as bad as he supposes it, is countenanced by many imperfectly constructed sentences, which are to be found in most of the papers in favour of the wire-gauze lamp. I will point out a few such sentences to Mr. Children's notice. I observed, with some degree of surprise, a grammatical error in the latter part of the concluding sentence of a paper in the *Annals of Philosophy* for October last, by a John George Children, Esq. a F.R.S. "*calling itself*" an answer to Mr. Longmire's objections to Sir H. Davy's lamp. Part of the sentence is as follows: "unless obstinate prejudice, *or* a worse feeling, *render* them insensible to the impressions of truth." Here two substantives have a *disjunctive* conjunction between them, and require a verb in the singular number, but Mr. Children has given them one in the plural number.

I will now show the public other sentences in Mr. Children's paper that are inaccurately constructed. "But it is not in the principle or construction of the lamp, but in the materials of which it is *not* of necessity made, &c." The "*not*" is a superfluous word, and gives this part of the sentence a ridiculous cast. "The compass, and the ship *that steers* by it." We all know that ships are steered by the compass; but we never heard before that a ship steers by it. "But perhaps it is not in the mere screwing on the bottom of the lamp that Mr. Longmire means (though certainly all his words express), and this sentence is intended to be taken, &c." What all my words express, Mr. Children has failed to inform us in this sentence. "But I have done; and willingly meet Mr. Longmire on this point; the same, indeed, to which in our conversation we agree to refer it—the experience of a few years; when I have no hesitation, &c." It is easy to find out Mr. Children's meaning in the former part of this sentence, but the latter part conveys no definite idea. I could fill two or three pages with inaccurate sentences made by Mr. Children, Sir Humphry Davy, the Editor of the

Phil. Mag., and other writers in favour of the wire-gauze lamp; but I will content myself with noticing one of Sir Humphry's.

This sentence is to be found in the Newcastle Courant, among Davy's late communications to that newspaper. It has not, however, been copied into the Phil. Mag., probably because even the Editor of that work was ashamed of it. Sir Humphry says, after stating that he had been anxious to answer every objection to his lamp, "But I do not think it proper to name the individuals by whom they (the objections) have been made; for I would *willingly* consign to *forgetfulness* those who do not deserve to be remembered." Sir H. Davy intended to write a sentence that would exterminate opposition, but spoiled its aptness, and prevented its effect, by inserting *forgetfulness* instead of *oblivion*. It would be well for Davy if he could even consign his opponents to *forgetfulness*, as they would then *forget* to make disagreeable remarks on his lamps; but as to consigning them to oblivion, it is quite out of the question; for there are amongst philosophers, men as capable of judging on this subject as Sir H. Davy or his friends, who do not think very highly of the lamp; and there are many amongst miners who restrict more than I its utility. The writings of some of the first class may perhaps be read when Sir H. and his works are no more; and of the second class, some who have not written will be long remembered by posterity for the mining operations which they will leave behind them. Consign such persons to oblivion, indeed! He must rip to the hearts of thousands to get at the good impressions of the proscribed persons; for where the heart has treasured up good remembrances, the tongue will often disclose them. Consign persons to oblivion, indeed! None but tyrants or insane persons ever think of such a thing.

Would Sir H. Davy be the *Grand Turk* of science? And has he arranged the media by which he would render into mutes, and strangle the efforts of, all who will not echo his opinions, or write in his praise? Sir H. Davy may succeed in such intentions when the King of Great Britain shall tremble at the Pope's Bull; or when Sir H. has brought his own wire-gauze lamp to perfection—but no sooner need he expect to succeed.

The first two quotations from Sir H. Davy's papers are, of themselves, sufficient answers to Mr. Children's principal objections; who, after admitting that absolute security is a want that miners will never get over, has detailed out his remarks to very little purpose. As it would be giving them a consequence to which they are not entitled, to enter into a formal refutation of all of them, I will content myself with adverting to a few, and will leave Mr. Children the option of recalling me to any one of them that he thinks deserving of further notice, and I will presently refute it.

I have not, Mr. Editor, seen any thing in Mr. Buddle's letters which amounts to "a pretty sufficient answer" to my remarks, that few, if any, practical miners will hazard their credit on the unrestricted assertion that the lamp will yield the boasted security.

Mr. Buddle may have written letters which I have not seen; but I believe he would not so far commit himself as to maintain the perfect security of the lamp. I have, indeed, observed that Mr. Buddle was one in an underground party at play with the wire-gauze lamp, and the object of the game was, *not to injure* the lamp by throwing stones and coals upon it, and by striking it with picks.

Now, really if Mr. Buddle will lend himself to—stop a little! I am wrong in this instance. "Prejudice, or a worse feeling," has got the better of my judgment. They attempted to break the lamp, but could not. "Sophistry is now put to the blush," and disappears on the approach of Sir H. Davy's resplendent light! I shall become the third "*enlightened*" miner. Yes! the discovery that a sharp-pointed pick will not perforate the wire-gauze lamp, is a discovery which is worthy of the name of Davy, and worthy of his name only. "This is exactly such a case as we should choose to place before Bacon, were he to revisit the earth, in order to give him, *in a small compass*, an idea of the advancement which philosophy has made since the time when he pointed out to her the road she ought to pursue."

I stated in my remarks that the collier would have to travel in the dark, if he could, as soon as his light was extinguished. Mr. Children says, I told him in a former sentence that the collier could travel in the dark. This is a mistake. I, however, do not assert that a miner cannot travel in the dark in the strictest sense of the word, because I know to the contrary; there being some places in which he may travel, and others where he cannot travel, in the dark. In both situations he has to meet with difficulties which will often make him vexed at the constructors of the wire-gauze lamp; as no man likes to be running his head against pillar corners, falling over pieces of coal or stone, cutting his head against sharp projecting parts of the roof stone, and perhaps bringing it down upon him, and breaking his legs or arms, or killing himself at once (though it appears from Sir H. and Mr. Buddle's experiments that "large pieces of falling stone and coal" cannot injure the miraculous wire-gauze lamp). If Mr. C. were left *in the dark* in a safe part of some mines, and be forced to follow the miners a few hundred yards through a circuitous road, it would be such a piece of experience to him as would prevent him from extolling lamps whose many deficiencies are mostly greater than that defect, which is a preventive to their being lighted in safety where they go out. Dr. Clanny, I believe, has succeeded in relighting his lamp with safety in any situation; and I am sure any lamp may be so lighted; and why the *Newton of chemistry* should be so deficient in chemical skill and ingenuity on this occasion, I am at a loss to say; but probably Mr. Children can give us a reason.

Mr. Children says "the screw is a sad stumbling block to Mr. Longmire." It never was a stumbling block to me; but it is one over which Sir H. will some day get a fall, if he does not remove it. Had I constructed a lamp with a screw in such a situation, and

fastened on the top so badly as to permit it to be taken off to clean, or even to be rivetted on, I should have trembled for my character as a mechanic. No living child ever came forth in a weaker state than Sir H.'s lamp, when ushered into notice, and its construction will be longer in arriving at Mr. C.'s "common sense perfection," than a child is in reaching maturity. As soon as I saw the lamp I suggested to several persons that the part which contains the wick and oil ought to be put into the cylinder at the top, and the means of suspension spring from the bottom of the lamp.

Were Sir H. to adopt this way, it would be one other step towards perfection. I will here remark that Sir H. Davy says he has had a cylinder perforated with small apertures, but it was not so cheap as the wire-gauze. Sir H., then, cannot find any other advantage which his mode of construction has over Stephenson's, or else he would have named it; for the paragraph alluded to was inserted to do injury to Stephenson's lamp. Now I will tell Sir H. that I expect to be able to show him and the world a better lamp on Stephenson's mode of construction (*which is original*) than he (Sir H.) has yet made with wire-gauze.

Mr. Children says he is willing to meet me on my averment that the obstacles which the lamp has to meet with in the mine have not been taken into the question so much as they ought. I here express my willingness to meet him on this or any other subject that is agitated by me, as I consider him a *very eligible correspondent*; and since he has voluntarily brought himself into my notice, I will some time or other let him hear from me on that subject, to which he devotes the largest portion of his time. When I made my remarks on the wire-gauze lamps, the double wire-gauze lamp was not thought of. This circumstance is of itself an answer so much to the purpose, that I dare say Mr. Children will not meet me again on this point; for if the obstacles which the lamp had to meet with in the mine had been previously taken into consideration as much as they ought, any additional security would not have been required. At the time I wrote, Sir H. had not made experiments on currents of fire-damp; which in the mine are obstacles that, of course, could not before have been much considered. This circumstance, and the result of Davy's experiments, are two other decisive answers to Mr. Children.

Mr. Children, Sir, says in his second letter, "I find I was mistaken in supposing the lamps were made solely of copper or brass gauze." By this time perhaps he will have found himself wrong in many other particulars. He, however, makes as much out of a mistake as any man can do. It appears he had not the chemical sagacity to discover "the fact" that so oxidable a metal as iron was "so *completely adapted*" to the purpose, or he would have argued the question on that footing in the first letter, and not evaded it by saying that brass and copper, and not iron, were used; the contrary to which he must have, or ought to have, known, before he could write so forwardly. But now he finds, when others tell him, that

iron is "*completely adapted*" for the lamps, and he contrives to make it a circumstance of additional testimony of the safety of the instrument, and a means of removing the remaining doubts of the most sceptical minds—except only "such as are insensible to the impressions of truth."

Had Mr. Children examined as many coal-mines as I have, and seen the fire-damp in its various modifications, he would not have concluded so hastily that the oxidable iron is so "*completely adapted*" for the lamps, though he might know, were he well acquainted with the properties of metals, that it is better adapted to that purpose than either copper or brass. Mr. Children concludes his second letter in a way that betrays a considerable want of candour. Two persons may hold different opinions on the same subject, either or neither of which are right, without either person being conscious of acting from prejudice arising from ignorance, obstinacy, or any other motive of worse origin.

Now I declare that I do not care the value of a peppercorn for the indirect stigmas which Mr. Children and his associates throw out against me, as one that differs from him and Sir H. on the lamp-making business. But I am not so intolerant as to suppose, nor shall I humble myself, and disgust the public, by asserting that Mr. Children, because he differs from me in opinion, is "insensible to the impressions of truth." No, Mr. Editor, such a narrow-minded conclusion I expect always to avoid.

I am, Sir, your very humble servant,

JOHN B. LONGMIRE.

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## ARTICLE IV.

### *On Safety Lamps for Coal-Mines, in Answer to the Letters of Mr. Children and Mr. Knight.* By Mr. Holmes.

(To Dr. Thomson.)

DEAR SIR,

I SHALL not attempt to describe the feelings of pity and surprise produced upon my mind on reading the letters of Mr. Children and a Mr. Knight, which appeared in the last number of the Phil. Mag., as they will be readily understood by every disinterested and honourable man; nor can I suppress my astonishment that a Journal which ought to be conducted upon liberal and scientific principles, should become the vehicle of so much personality as is displayed in Mr. Children's letter.

It is fortunate that there is another scientific journal which is conducted upon honourable and independent principles, and that its pages are open to all, and influenced by none. Through this medium I make my reply.

Mr. Children may imagine—and, according to the policy he has adopted, he appears desirous of leading others to imagine—that my prejudices against the wire-gauze lamps are proof against conviction. In this I beg to say Mr. Children is mistaken, though I do assert that these lamps have to surmount the objections which I have made before they afford either a secure or a serviceable light.

In my experiments it was not my intention to discover whether the wire-gauze lamps were suitable for ordinary purposes, for I never disputed but that they might be equally safe with a common lantern, if not exposed to the numerous accidental circumstances which have uniformly produced explosions; but my object (knowing these accidental circumstances) was to ascertain whether they would be safe for *all the requisite uses of a coal-mine*. I ascertained that they would not, and it has required all the abilities of the distinguished inventor to contend with difficulties he did not at first calculate upon. These difficulties I have progressively pointed out, and I shall be happy if he ultimately succeeds in overcoming them.

It is necessary, however, to prove whether or not my experiments and objections have been well founded. In doing this, I shall not leave the public to depend upon my assertions. The experiments quoted by Mr. Children were instituted in *June*, and *previous to that period the public were not in possession* of any observations from Sir Humphry Davy relative to the principal points touched upon in the paper from whence Mr. Children quotes. In *July* Sir Humphry Davy published his "Additional Practical Observations," which I am pretty well aware resulted from his experiments at the Royal Institution after my experiments were made known. Immediately on their being printed, I transmitted that gentleman one of my papers upon safety lamps. Previous to that time lamps with 576 *apertures* to the square inch were recommended for use, as will appear from Sir Humphry Davy's paper to the Royal Society published in the Philosophical Transactions, 1816, wherein he says, "Iron wire-gauze of $\frac{1}{50}$ and of 24 apertures to the inch, or of 576 to the square inch, appeared safe *under all circumstances* in explosive mixtures of *coal gas*. Sir Humphry Davy will now, I have no doubt, admit that these experiments were not consistent with the trials a lamp must undergo in a coal-mine, at least I conclude so from his Additional Practical Observations, wherein, exclusive of strongly recommending the use of a *double wire-gauze cylinder*, he says, "In adopting from 30 to 26 apertures to the inch (from 900 to 676 to the square inch), and wires of from $\frac{1}{50}$ to $\frac{1}{40}$ of an inch in thickness, *even* single lamps are secure in all atmospheres of fire-damp. Now it is rather singular that fire-damp requires 100 more apertures to the inch than coal gas, which is by far the more ready of the two to pass explosion through the wires. The fact is, I proved them insecure. Sir H. Davy afterwards found them so; and then I am told that my objections are prejudiced and ill-natured. What can be more palpable than that they were blazoned about in the first

stage of their insecurity as completely *perfect*; a merit they have not yet attained, nor ever will attain.

In regard to Sir Humphry Davy having known the effect of coal dust, pyrites, &c. floating in the atmosphere of a mine, before me, and consequently that he knew the effects of currents of fire-damp, I can only say that this does not appear probable, from what I have before stated, or from the postscript to his paper in the Phil. Mag. for Oct. 1816, p. 201, wherein he says, "The experiments above detailed on the blower are the *first* I have made upon *currents of fire-damp*; and in the same postscript he recommends gauze of a *still finer texture* than before: and that coal dust will fly off in sparks, I refer Mr. Children to Mr. Buddle's letter in the Journal of Science and the Arts, No. II. p. 103.

Mr. Children, after describing his apparatus for experimenting upon the lamps, says, "In a *few seconds* about *two inches* of the wire-gauze became red-hot; and by continuing the blast from each bladder, it rose *almost* to whiteness, the heat being greatest at the side opposite the jet of atmospheric air—*at this point the exterior gas exploded.*" Sir H. Davy, in his paper in the Phil. Mag. for Oct. p. 198, says, "The wire-gauze is impermeable to the flame of all currents of fire-damp as long as it is not heated above redness; but if the iron wire be made to burn as at a strong welding heat, of course it can be no longer safe, and though *perhaps* such a circumstance can never happen in a colliery, yet it ought to be known, and guarded against; and if a workman having a *single lamp* should *accidentally* meet a blower acting on a current of fresh air, he ought, on finding his lamp becoming hot, to take it out of the point of mixture, or secure it from the current." I will refer Mr. Children again to the experiments stated in the same paper, before I. G. Lambton, Esq. when the flame of *fire-damp* passed single cylinders. Sir H. Davy says much about the current on this occasion; but I presume neither he nor Mr. Children will say that a stronger current of air is not frequently to be found in almost any old and dangerous mine. It might not be in the colliery Sir H. Davy descended or went into at Wallsend (or Kenton), which is one of the safest in this district; but he probably might have found them had he gone, as Dr. Clanny and myself did, with his insulated lamp, unheedingly through a labyrinth of foul workings.

But I will put it in another position, and suppose that no danger can exist unless the wire be brought to a red or white heat. This will occur in almost every case when the lamp, by any accident, is left for a *few seconds* inclined in an angle of about 45°, or when a strong current of air blows upon the flame for about *half a minute*. Now I will leave it to any of your readers, who understand the principles of mining, and know the carelessness of miners, to ascertain how many accidental circumstances might in the course of a year occur to place a lamp in the above situation in an explosive atmosphere; so that Sir H. Davy has fallen into Scylla in avoiding

Charybdis; for the large wires permit explosion to pass, and the small wires reduce the security between contingencies and danger; and I can assert that none of these little, though positive accidents, will affect either the steam lamp, or the original insulated lamp invented by Dr. Clanny.

Mr. Children says, "Nothing but a current of air directing the flame with great force to one point can heat the gauze sufficiently for flame to traverse it; and should such a current be met with in the mine, it must inevitably extinguish the light." This I deny, particularly if the current be formed by a blower of gas diluted with atmospheric air, which, by acting for a *few seconds* upon one point, would pass the explosion; and Sir H. Davy, in his paper in the Phil. Mag. for Oct. p. 197, says, "If a blower or strong current of fire-damp is to be approached, *double gauze lamps*, or lamps in which the circulation of air intercepted by slips of metal or glass, should be used; *or if the single lamp be employed, it should be put into a common horn or glass lantern.*" Sir Humphry Davy forgets that blowers frequently break out without any previous indication of such a phenomenon. Indeed, were I desirous of finding out an *insurmountable* objection to gauze lamps, it would be furnished by the possibility of a strong current of air having generally the power to extinguish a light, which under all circumstances is very feeble.

In the course of Sir H. Davy's trials to make an insulated lamp (the details of which, notwithstanding Mr. Children's supposition to the contrary, I am *fully acquainted with*), he accidentally discovered that explosive mixtures of gases could not readily be passed through narrow tubes or perforations. His hypothesis upon this point is not yet quite satisfactory; but I shall be glad if the long train of modifications ultimately produce such a lamp as may answer all the expectations of himself and his friends. I admit that constant use is the greatest proof of their security. But coal-mines do not explode every day, or every year, even with naked lights. I contend that their great merit is yet theoretical, and may be overthrown by a few moments of carelessness or fortuitous circumstance.

I regret that Mr. Children should have drawn me into further controversy, when I was desirous of waiting the issue of time and experience. I have never objected to the double cylinder lamps; and hope my fears of the sufficiency of their light will not be substantiated; but in concluding, I must remind Mr. Children that when an invention is one thing in *June*, another in *July*, another in *August*, and requiring *double* precaution in September, the objections made to it in the first stage cannot be construed applicable to its progressive changes, neither are they invalidated by the better success of subsequent improvements. In giving Mr. Children credit for his high feelings towards Sir H. Davy, I beg to remark, that I glory equally in the friendship of Dr. Clanny; and have more reason to feel myself proud in defending his cause in regard to the lamp, than Mr. Children can have in defending that of Sir

H. Davy. Dr. Clanny has been injured by party spirit and popular opposition; but the time will come when his genius and philanthropy will be acknowledged by society.

I have in the course of my writings upon this subject, and particularly in the paper from whence Mr. Children has made some quotations, mentioned Mr. Stephenson as entitled to reward, before Sir H. Davy. That I am not solitary in this opinion, will be evident to Mr. C., by reference to a letter of R. W. Brandling, Esq. to the Secretary of the General Meeting of the Coal Trade at Newcastle, in the Tyne Mercury, Oct. 15, 1816.

A few words to Mr. Knight; for which he may thank the amity of my contempt. Mr. Knight is anxious to know how I got access to the Gas Works. The answer is simple. *I quietly walked in at the gateway.* But I must inform this champion of science that, had I known the rules of his establishment, I should certainly have requested permission in the first instance from a proprietor; but, ignorant of these, I proceeded in open daylight to the works, where I inquired for a proprietor. The man whom I asked went into the office, and returned with a person whom, from his appearance, I was bound to consider a gentleman; and, without further inquiry, to admit that he was a proprietor, or a person qualified to act: at all events he gave me leave to try my experiments, and called a number of people round to witness them. All this was done in the presence of several perfect strangers (who obligingly gave me their names). I shall, therefore, leave the liberality of Mr. Knight's statement to the judgment of the enlightened readers of the *Annals*; and though he seems very anxious to disclaim any participation in my experiments, I do not see the necessity of his precaution, as his name was never mentioned; and if I may judge from this exterior index of his principles, it would not have done me much credit if it had.

To conclude: Mr. Children, in the course of his philosophical and dispassionate paper, digresses into the following expressions, which appear new in science, viz. *insinuations, ignorance, envy, idiot, madman, caviller, spleen.* It must be a bad cause which stands in need of such subterfuges as Mr. Knight's, or of the personal and scurrilous expressions of Mr. Children; and I appeal to any liberal man whether such an antagonist in the fervour of his friendship, deserves another word from,

Dear Sir, yours very truly,

*Bishopwearmouth, Oct.* 13, 1816. J. H. H. Holmes.

## Article V.

### *A Comparison of the Temperatures at Tottenham and Plymouth.* By James Fox, jun. Esq.

(To Dr. Thomson.)

SIR, *Plymouth, Aug.* 5, 1816.

Under the head Meteorology in the *Annals* for January last, you have stated it (from the average results of temperature) to be warmer during the summer months of 1814 in the west of England than in the more eastern part of the kingdom. I am apprehensive that those who read that statement, without troubling themselves to examine *in detail* the journal kept at Tottenham, and that which is registered at Plymouth, will not arrive at sufficient information.

For although it be true that the *average* temperature has been greater at Plymouth than at Tottenham, yet as both average results were made of *extremes*, it may not be amiss to inquire further, and ascertain to what *changes* the inhabitants of each place are daily exposed. Such an inquiry would decidedly prove that in the west of England a more equal temperature is preserved than in other parts of the kingdom. In the summer months we have less heat by day, and less cold by night; and in winter are warmer both by night and day than at Tottenham. These are circumstances of much importance to invalids, and it is on their account chiefly I trouble you with these observations. Indeed, to ascertain the daily *range* of the thermometer (by means of a self-registering one), for the information of the medical profession, was the principal object I had in view, when I first proposed keeping a journal of the weather; that by putting them in possession of facts, regularly recorded, they might the better ascertain the superiority of the temperature of the west of England, than by merely common received opinion, without such aid.

And here I cannot refrain from expressing a wish that all who publish their meteorological observations would adopt *one* method of keeping their registers. Comparison with each other would be much facilitated by it.

If the changes of the barometer and thermometer were to be traced on a scale of any given and approved dimensions, would it not be the most pleasing manner of noting them? The daily maximum and minimum of temperature would thereby become obvious without the fatigue of searching over "closely packed columns of well-arranged figures." The eye would be at once struck with any change, and would as quickly see on what part of the day, month, or year, it had taken place. Five minutes' comparison of two such scales filled up at different places, would give a much more satisfactory idea of the temperature and changes of each place than the closest examination of a common journal; and if the state of the wind by the initial letters, and other observations in

an approved short hand, were written at the bottom of each daily column, it would render it a complete register in the most compact form. The variations of the barometer would instantly be traced to a change of wind or temperature; and by observing the alterations of weather which accompany, or quickly follow, such changes, we shall be enabled to form a tolerably correct opinion of weather, allowance being made for the extreme uncertainty of our climate.

I inclose a statement extracted from Mr. Howard's register and my own, containing the monthly average of the *line of heat*, also that of *cold*, together with the *greatest range* of the thermometer during each month; by which the changes of temperature are shown to be much greater near the metropolis than in the west.

I am, &c.

JAMES FOX, jun.

P.S. Will not the hygrometer contrived by Mr. Wilson be affected by change of temperature, as well as by the alteration of the atmosphere from dry to moist, &c.?

*A Comparison of the Temperature of Tottenham and Plymouth for Two Years.*

1814.

| | Maximum Monthly Average at Tottenham. | Maximum ditto at Plymouth. | Minimum Monthly Average at Tottenham. | Minimum ditto at Plymouth. | Greatest Range of Thermometer at Tottenham. | Ditto at Plymouth. | Less Range at Plymouth |
|---|---|---|---|---|---|---|---|
| Jan... | 32·54° | 36·45° | 20·87° | 27·29° | 33° | 34° | —° |
| Feb... | 39·10 | 43·95 | 27·25 | 32·64 | 32 | 32 | 0 |
| March | 43·74 | 46·90 | 31·90 | 33·83 | 39 | 32 | 7 |
| April. | 61·48 | 58·40 | 40·17 | 44·28 | 42 | 38 | 4 |
| May.. | 61·03 | 62·64 | 40·09 | 45·09 | 39 | 34 | 5 |
| June.. | 65·07 | 64·40 | 46·75 | 50·76 | 49 | 32 | 17 |
| July.. | 75·19 | 70·54 | 54·32 | 56·03 | 49 | 29 | 20 |
| Aug... | 71·86 | 69·12 | 52·13 | 54·45 | 43 | 30 | 13 |
| Sept. . | 65·95 | 66·50 | 44·54 | 51·89 | 42 | 33 | 9 |
| Oct. .. | 56·06 | 56·35 | 37·67 | 43·29 | 43 | 31 | 12 |
| Nov... | 46·20 | 48·33 | 33·50 | 38·32 | 35 | 30 | 5 |
| Dec... | 45·38 | 47·61 | 35·03 | 39·43 | 31 | 26 | 5 |

1815.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Jan... | 37·16 | 39·09 | 27·70 | 29·00 | 28 | 27 | 1 |
| Feb... | 50·64 | 50·44 | 38·28 | 41·27 | 32 | 26 | 6 |
| March | 55·53 | 52·09 | 39·36 | 42·16 | 44 | 32 | 12 |
| April. | 58·30 | 57·50 | 38·83 | 40·87 | 42 | 39 | 3 |
| May.. | 69·90 | 64·52 | 47·54 | 48·24 | 46 | 33 | 13 |
| June.. | 72·58 | 68·70 | 47·79 | 51·63 | 42 | 33 | 9 |
| July.. | 71·77 | 69·87 | 50·62 | 53·97 | 38 | 31 | 7 |
| Aug.. | 72·28 | 69·94 | 51·81 | 53·88 | 36 | 30 | 6 |
| Sept.. | 67·55 | 66·83 | 43·10 | 51·54 | 48 | 34 | 14 |
| Oct... | 59·58 | 59·03 | 41·55 | 46·29 | 33 | 29 | 4 |
| Nov.. | 45·07 | 46·39 | 31·82 | 30·16 | 39 | 32 | 5 |
| Dec... | 42·10 | 43·24 | 30·48 | 32·89 | 32 | 32 | 0 |

## Article VI.

*Register of the Weather in Plymouth for the first Six Months of* 1816. By James Fox, jun. Esq.

(With a Plate.)

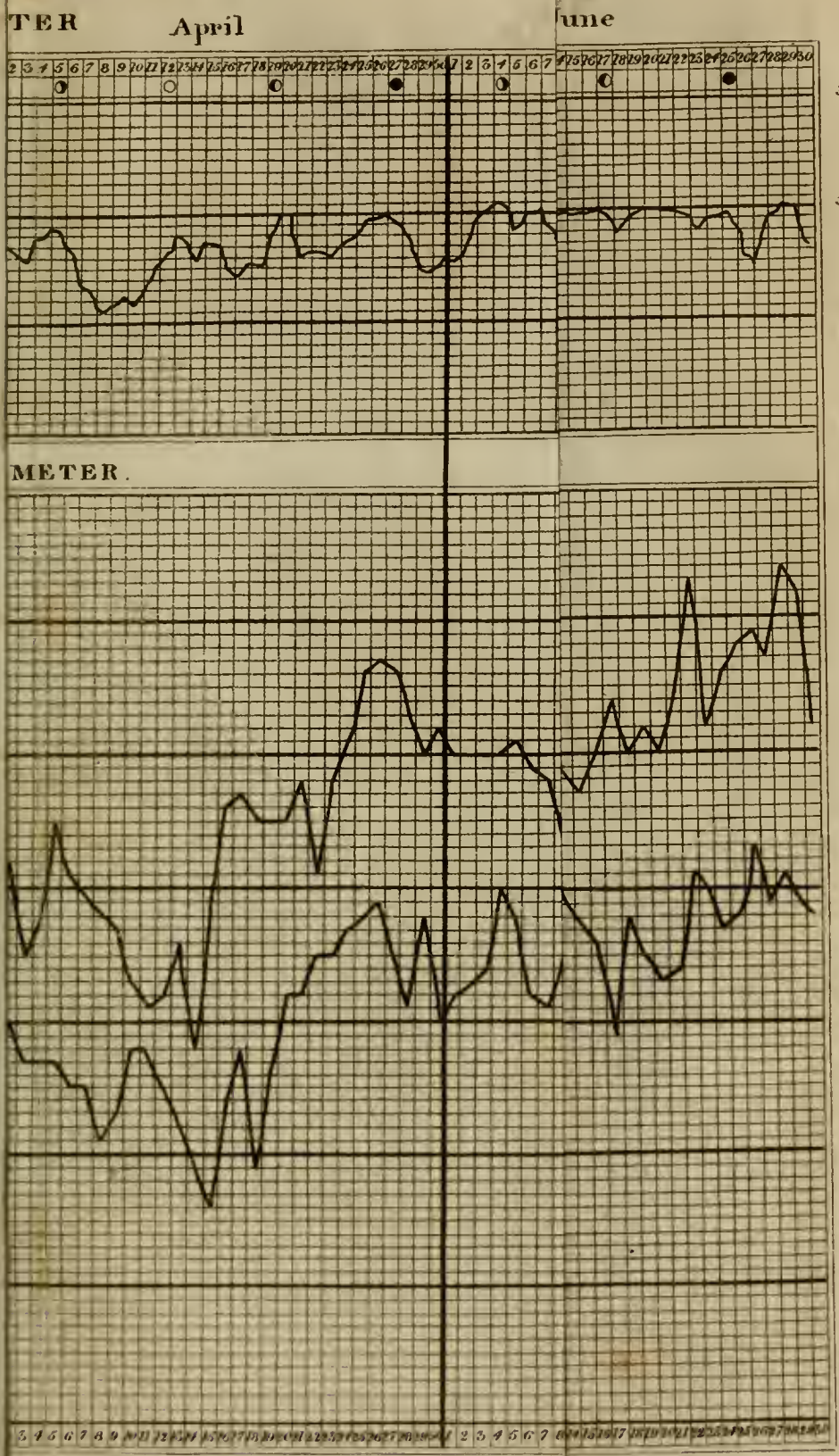
### *JANUARY.*

| Date. | Wind. | Rain. | Observations. |
|---|---|---|---|
| 1816. | | | |
| Jan. 1 | E | | Fog, morn; fair day. |
| 2 | E N E | | Fair; cloudy at night. |
| 3 | N W | 0·10 | Showers early, morn; fair day. |
| 4 | W N W | | Misty. |
| 5 | W N W to S W | | Ditto. |
| 6 | W to W N W | | Ditto, morn; cloudy and high wind, aftern. |
| 7 | W N W | | Cloudy. |
| 8 | W | 0·30 | Misty, morn; high wind and rain, aftern. |
| 9 | Var. | 0·20 | Ditto, ditto; showers. |
| 10 | W N W | | High wind; cloudy and fair. |
| 11 | W | 0·38 | A gale of wind, and heavy showers of hail and rain. |
| 12 | S W | 0·51 | High wind; cloudy and fair, morn; heavy showers, aftern. |
| 13 | W and S S W | | Ditto; cloudy and fair. |
| 14 | N W | 0·10 | Showers, morn: cloudy and fair day. |
| 15 | S S E to W N W | 0·61 | Heavy rain. |
| 16 | W to S | | Cloudy and fair; heavy rain and a severe gale at night. |
| 17 | S to W | 0·50 | Heavy gale, with rain, morn; showers of sleet, aftern. |
| 18 | W | | High wind: showers, morn; cloudy aftern. |
| 19 | N W | 0·78 | Heavy rain. |
| 20 | N E to S | 0·47 | Ditto; stormy at night. |
| 21 | E N E to W N W | | Showers, morn; cloudy and fair eve. |
| 22 | N W to W | | Cloudy. |
| 23 | N E | 0·27 | Cloudy morn; showers, aftern.; cloudy and fair eve. |
| 24 | E to E N E | | Showers. |
| 25 | N E | | Cloudy. |
| 26 | N W | | Ditto. |
| 27 | N E | | Ditto. |
| 28 | N E | | Fair. |
| 29 | E to E S E | | Cloudy. |
| 30 | S E | | High wind; fair morn; cloudy and fair aftern. |
| 31 | S E | | Ditto; cloudy morn; ditto, ditto. |
| | | 4·22 | Inches rain. |

| | | | *Wind.* |
|---|---|---|---|
| Barometer: | Greatest height | 30·40 inches | W N W |
| | Lowest | 28·94 | N E |
| | Mean | 29·639 | |
| Thermometer: | Greatest height | 51° | W |
| | Lowest | 28 | E |
| | Mean | 38·112 | |

TER at Plymouth January to Jun

*l above the level of the Sea)*

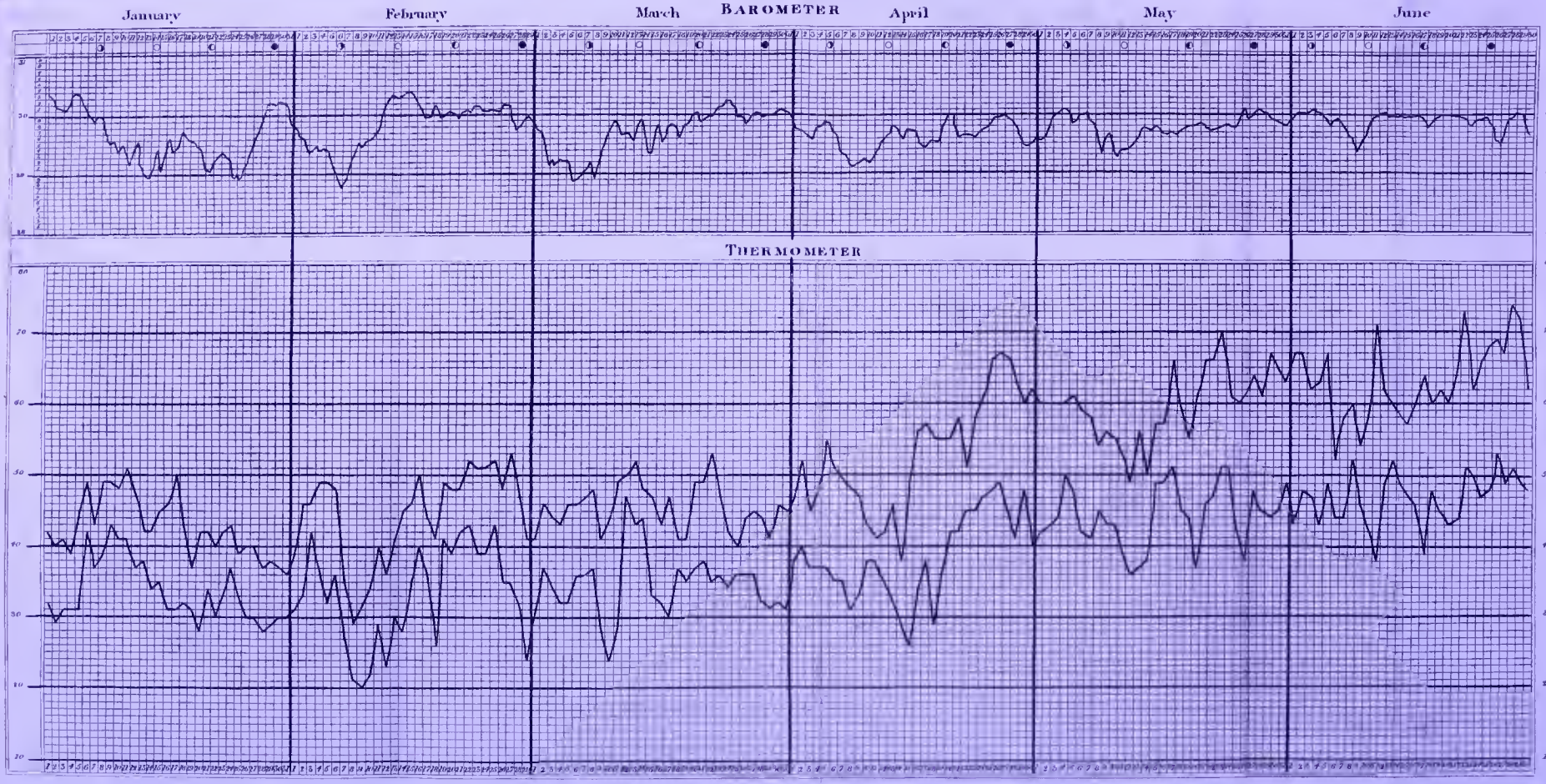



*dock & Jay, Paternoster Row, December 1.1816.*

# Scale of the BAROMETER and THERMOMETER at Plymouth January to June 1816.

(Place of Observation 112 feet above the level of the Sea)






Engraved for Dr Thomson's Annals, for Baldwin, Cradock & Joy Paternoster Row December 1816

## *FEBRUARY.*

| Date. | Wind. | Rain. | Observations. |
|---|---|---|---|
| 1816. | | | |
| Feb. 1 | E N E | | Cloudy and fair morn; cloudy aftern. |
| 2 | E to S | 0·51 | Misty morn; heavy rain, aftern. |
| 3 | S | 0·43 | Heavy showers. |
| 4 | W S W to W N W | | Cloudy and fair day; fair eve. |
| 5 | Var. | 1·46 | Cloudy morn; high wind, and heavy rain, aftern. and night. |
| 6 | Var. | | Heavy rain early, morn; showery day. |
| 7 | N E | | High wind; cloudy; a little snow. |
| 8 | N E | | Fair. |
| 9 | N E | | Ditto; a snow shower. |
| 10 | Var. | | Fair. |
| 11 | N E to N W | | Ditto. |
| 12 | N W to E N E | | Fog, morn.; fair day; cloudy and fair at night. |
| 13 | S to W N W | | Cloudy and fair. |
| 14 | W N W | | Ditto; fog at night. |
| 15 | W | | Thick weather. |
| 16 | W N W | | High wind; cloudy. |
| 17 | N W | | Ditto, ditto. |
| 18 | N W | | Fair morn; cloudy aftern.; misty and high wind at night. |
| 19 | W N W | | Cloudy. |
| 20 | S W | 0·06 | Thick weather. |
| 21 | S | 0·10 | Showers. |
| 22 | S | | Cloudy. |
| 23 | S E | | Ditto and fair morn; fair aftern. |
| 24 | S to W | 0·05 | Cloudy morn; thick aftern.; high wind. |
| 25 | W | 0·04 | High wind; thick weather. |
| 26 | N W | | Fair. |
| 27 | W | 0·05 | Misty morn; cloudy day. |
| 28 | N W to N N E | 0·07 | Showers; fair at night. |
| 29 | Var. | | Fair. |
| | | 2·77 | Inches rain. |

| | | | *Wind.* |
|---|---|---|---|
| Barometer: | Greatest height | 30·39 inches | W |
| | Lowest | 28·78 | Var. |
| | Mean | 29·841 | |
| Thermometer: | Greatest height | 53° | W |
| | Lowest | 20 | N E |
| | Mean | 38·844 | |

## *MARCH.*

| | | | |
|---|---|---|---|
| March 1 | E N E | | Snow showers, morn; cloudy and fair aftern. |
| 2 | Var. | 0·43 | Heavy showers. |
| 3 | S to W | 0·25 | High wind, and ditto. |
| 4 | S to W N W | 0·15 | Showers. |
| 5 | Var. | 0·21 | Cloudy day; rain at night. |
| 6 | S to S W | | High wind, and hail showers; cloudy and fair at night. |
| 7 | S to W | 0·28 | Heavy showers; hail and rain. |
| 8 | E to N W | | Showers. |

| Date. | Wind. | Rain. | Observations. |
|---|---|---|---|
| 1816. | | | |
| March 9 | N N W | 0·02 | Snow showers, morn; hail at night. |
| 10 | N W | | Fair morn; cloudy and fair aftern; misty at night. |
| 11 | S to W | 0·92 | High wind, and heavy rain. |
| 12 | S W | | Ditto, ditto. |
| 13 | W S W | | Cloudy and fair; misty at night. |
| 14 | S to S S E | 0 | Thick weather; heavy rain, and a gale, at night. |
| 15 | S to W N W | 0·55 | A severe gale in the early part of the day; a calm at night. |
| 16 | S E to N W | | Heavy rain, morn; high wind; cloudy and fair aftern. |
| 17 | W N W | 0·13 | Showery day; cloudy and fair at night. |
| 18 | W N W | | Showers, and high wind. |
| 19 | N W | 0·10 | Cloudy and fair morn; showers, aftern. |
| 20 | N W to N E | | Fair day; cloudy at night. |
| 21 | S S E to N W | 0·07 | Misty day; ditto. |
| 22 | N W to E | | Cloudy and fair. |
| 23 | N E | | Ditto. |
| 24 | N E | | High wind; cloudy. |
| 25 | E N E | | Ditto, ditto. |
| 26 | N E | | Ditto; fair. |
| 27 | N E | | Ditto, ditto. |
| 28 | E | | Ditto; cloudy and fair. |
| 29 | E | | Ditto, ditto. |
| 30 | E | | Ditto, ditto. |
| 31 | E | | Ditto; fair. |
| | | 3·55 | Inches rain. |

| | | | *Wind.* |
|---|---|---|---|
| Barometer: | Greatest height | 30·23 inches | N E |
| | Lowest | 28·86 | Var. |
| | Mean | 29·702 | |
| Thermometer: | Greatest height | 53° | N N E |
| | Lowest | 24 | N W |
| | Mean | 40·177 | |

## *APRIL.*

| | | | |
|---|---|---|---|
| April 1 | E | | High wind; cloudy and fair. |
| 2 | E N E | | Ditto. |
| 3 | E | 0·06 | Showers, morn; cloudy aftern. |
| 4 | N E | | Cloudy and fair. |
| 5 | E to N W | | Ditto, ditto. |
| 6 | N W to W | 0·12 | Ditto, ditto; showers at night. |
| 7 | W N W | | High wind, and hail showers. |
| 8 | Var. | 0·07 | Cloudy and fair morn; showers, aftern.; fair at night. |
| 9 | N E | | Ditto, ditto. |
| 10 | N N W to N N E | 0·10 | Showers. |
| 11 | S E to N E | 0·09 | Ditto. |
| 12 | E to N W | 0·04 | Fair morn; showers, aftern. |
| 13 | N W | | Cloudy and fair; a shower. |
| 14 | N W | 0·07 | Snow showers about two inches deep. |
| 15 | N W | | Cloudy and fair. |

| Date. | Wind. | Rain. | Observations. |
|---|---|---|---|
| 1816. | | | |
| Apr. 16 | W to S W | 0·02 | Cloudy morn; showers, aftern. |
| 17 | W to W N W | | Fair ditto; cloudy and fair aftern. |
| 18 | S W to W | | Fair day; cloudy at night. |
| 19 | W to N W | | Cloudy and fair day; fair at ditto. |
| 20 | S E | | Ditto, ditto. |
| 21 | E | | Fair. |
| 22 | E | 0·23 | Showers. |
| 23 | E | | Light ditto. |
| 24 | E S E to W | | Cloudy day; fair at night. |
| 25 | N E | | Fair. |
| 26 | N E | | Ditto. |
| 27 | Var. | | Ditto. |
| 28 | S to S E | | Ditto; cloudy at night. |
| 29 | S E to W N W. | 0·30 | Heavy showers, morn; cloudy aftern. |
| 30 | W N W | | Cloudy. |
| | | 1·10 | Inch rain. |

| | | | *Wind.* |
|---|---|---|---|
| Barometer: | Greatest height | 30·00 inches | S E |
| | Lowest | 29·11 | Var. |
| | Mean | 29·623 | |
| Thermometer: | Greatest height | 67° | N E |
| | Lowest | 26 | N W |
| | Mean | 45·700 | |

## *MAY.*

| | | | |
|---|---|---|---|
| May 1 | S to S S E | | Cloudy morn; cloudy and fair aftern. |
| 2 | W | 0·22 | Heavy showers early, morn; ditto day. |
| 3 | W | 0·20 | Ditto morn; cloudy day; high wind, aftern. |
| 4 | W to S W | | High wind; cloudy and fair day; showers at night. |
| 5 | W | | Ditto, ditto. |
| 6 | W | | Ditto, ditto. |
| 7 | S S E to W | 0·07 | Showers; rather misty. |
| 8 | S to W N W | 0·25 | Heavy showers; cloudy and high wind at night. |
| 9 | W N W | | High wind; cloudy and fair; a large halo round the moon. |
| 10 | W N W | 0·33 | Ditto, and heavy showers early, morn; cloudy day. |
| 11 | W N W | 0·33 | Ditto, and showers. |
| 12 | N N W | | Ditto, ditto. |
| 13 | W N W to N W | | Showers, morn; cloudy and fair day. |
| 14 | S E to E N E | 0·28 | Thick weather. |
| 15 | S to E | | Misty morn; cloudy aftern. |
| 16 | S E to S | 0·50 | Ditto, ditto. |
| 17 | N W | | Fair day; distant lightning at night. |
| 18 | E N E | | Heavy showers early, morn; cloudy aftern.; fair at night. |
| 19 | E N E | | Cloudy and fair morn; fair aftern. |
| 20 | E N E | | High wind; fair. |
| 21 | E N E | | Fair. |
| 22 | E N E | | Cloudy. |

| Date. | Wind. | Rain. | Observations. |
|---|---|---|---|
| 1816. | | | |
| May 23 | E N E | | Fair day; cloudy at night. |
| 24 | E N E to S W | | Cloudy morn; misty aftern. |
| 25 | S to W N W | 0·52 | Misty morn; showers, aftern. |
| 26 | S S W | | High wind; cloudy and fair day; showers at night. |
| 27 | Var. | | Cloudy morn; cloudy and fair aftern. |
| 28 | S | | Cloudy and fair. |
| 29 | S S W to W N W | | Ditto day; fair at night. |
| 30 | S W to W N W | | Ditto. |
| 31 | W N W | | Cloudy. |
| | | 2·70 | Inches rain. |

| | | | *Wind.* |
|---|---|---|---|
| Barometer: | Greatest height | 30·11 inches | S S W |
| | Lowest | 29·27 | W N W |
| | Mean | 29·780 | |
| Thermometer: | Greatest height | 70° | E N E |
| | Lowest | 36 | W N W |
| | Mean | 51·983 | |

## *JUNE.*

| | | | |
|---|---|---|---|
| June 1 | Var. | | Fair. |
| 2 | N W | | Cloudy and ditto. |
| 3 | N W | | Ditto; high wind. |
| 4 | N W | | Ditto; fair at night. |
| 5 | N W | | High wind; cloudy and fair; cloudy at night. |
| 6 | N W | | Ditto, ditto, morn; fair aftern. |
| 7 | N W | | Cloudy and fair. |
| 8 | N W | | High wind; cloudy. |
| 9 | N W | 0·06 | Ditto; showers. |
| 10 | N W | | Cloudy and fair. |
| 11 | Var. | 0·05 | Ditto, ditto, morn; showers, even. |
| 12 | S | | Ditto, ditto, ditto; cloudy ditto. |
| 13 | S to N W | | Cloudy morn; misty aftern. |
| 14 | N N W | | Ditto, ditto. |
| 15 | N W | 0·20 | Showers, morn: misty and high wind, aftern. |
| 16 | Var. | | Ditto, ditto; cloudy and fair, aftern. |
| 17 | Var. | | Cloudy and fair. |
| 18 | Var. | 0·35 | Heavy rain, morn; cloudy and fair aftern. |
| 19 | Var. | | Cloudy and fair ditto; fair aftern. |
| 20 | Var. | | Fair. |
| 21 | S | | Ditto; cloudy at night. |
| 22 | Var. | | Ditto, ditto; and fair at ditto. |
| 23 | W | 0·11 | High wind, and light rain, morn; cloudy and fair aftern. |
| 24 | W N W | | Ditto; cloudy and fair. |
| 25 | S W | | Fair morn; cloudy aftern. |
| 26 | S W | 0·55 | Heavy showers. |
| 27 | N W | | Cloudy and fair. |
| 28 | N W | | Fair. |
| 29 | S W | 0·09 | Cloudy and fair day; light rain at night. |
| 30 | S W | | Ditto, ditto, morn; light rain, aftern. |
| | | 1·41 | Inch rain. |

| | | | *Wind.* |
|---|---|---|---|
| Barometer: | Greatest height | 30·09 inches | N W |
| | Lowest | 29·36 | N W |
| | Mean | 29·885 | |
| Thermometer: | Greatest height | 74° | N W |
| | Lowest | 38 | Var. |
| | Mean | 55·083 | |

*Errata in former Communications.*

Mean Temperature of May, 1814, should have been written .... 53·84°
Ditto of January, 1815 .... 34·04
Ditto of September, 1815 .... 59.20

## Article VII.

*Description of two Cases of Tetanus.* By Dr. Cross.

(To Dr. Thomson.)

SIR, *Glasgow, Sept.* 2, 1816.

Please to give a place in the *Annals of Philosophy* to the two following cases of tetanus :—

J. Murdoch, apprentice to Mr. J. Caldwell, flesher, Main-street, Gorbals, on Oct. 21, 1815, consulted Mr. Peter Miller, surgeon, Hutcheson Town, about a stiffness of the jaws. He was then 13 years of age; four feet nine inches high; stout; well shaped; and of a fair complexion. Mr. Caldwell, in whose neighbourhood the boy had been brought up, took a fancy for him from his childhood, on account of his uncommon alacrity and obliging disposition; so that, although his parents were poor, he has for the most part lived with his present master, and enjoyed generous diet. He could assign no cause for the stiffness of his jaws; but at length on his left leg a sore was discovered, which led back to the 9th inst., when, at the building of a hay-stack, he received from the prong of a hay fork, thrown at him "in fun," a small wound four inches above the left inner ankle, and about a line or two behind the edge of the tibia. Although the wound was pretty deep, yet no blood issued, except a dot "about the size of the head of a pin." The place soon became swelled and red, but not very painful until the 12th, when he made an excursion with some boys to a glen, five miles distant, to gather bramble-berries, got himself drenched with rain, and after his return neglected to change his clothes. As the sore had become painful, and its edges hard, Mrs. Caldwell applied a poultice. Hitherto the matter discharged had been of proper consistence, but now became more and more watery, while the pain became less and less. On the morning of the 18th, he complained of a difficulty in chewing a piece of loaf bread. In the evening he could not open his mouth to the usual extent, and felt considerable difficulty in chewing. On the morning of the

19th, in supping his porridge, he could scarcely get the spoon into his mouth. In the forenoon of the 20th he had wheeled a cow's hide to a tan-work a quarter of a mile distant, and was looking into a tan-hole, when he became giddy, and nearly dropped in. On getting home with his barrow, he had to go to bed, and in a few hours became free of giddiness, but the stiffness of the jaws was slowly gaining ground. On the 21st, in the forenoon, his arm, in throwing a stone at a dog, fell powerless by his side, and the stone dropped from his hand. He does not know whether the arm became relaxed or stiff. Till the evening of the 21st the stiffness was limited to the jaws. Now the tongue also became sore and stiff, as if it had not sufficient room. Having become alarmed at the increase of symptoms, he now applied, as formerly mentioned, to Mr. Miller, who immediately stripped him, and discovered on the left leg the sore slightly inflamed, but devoid of pain, and discharging a watery fluid. ℈iss. of compound jalap was prescribed, and he was desired to return next morning.

22. *Morning.*—Had slept little; physic had operated; stiffness had extended to the back of the neck. The proposal of sending him to the Infirmary alarmed him, so that he hurried home, but almost fell two or three times on the road. In the afternoon, being unable to walk to Mr. Miller, he was visited about two o'clock. The sore was enlarged, and pencilled with caustic: ℈i. of ipecac. and gr. ¼ of tart. antim. were given as an emetic, and operated slightly. What came up, he says, was very sour. He was now put into the warm bath, after which the following mixture was ordered:—℞. tinct. opii, gutt. lxxx. eth. sulph. ʒss. aq. cinnam. ℥i. mix., the half to be taken immediately, and the remainder in half an hour. From this time forward he was closely attended by Mr. Miller, and occasionally by his partner, Mr. M'Lean.

*Eleven at Night.*—A paroxysm had thrown him backwards to the floor. A table spoonful of the following mixture, ℞. tinct. opii, ʒii. sp. eth. nitr. ʒiss. aq. cinnam. ℥iii. mix. was ordered to be swallowed every second hour. Camphorated mercurial ointment was to be rubbed occasionally over his spine, and a dose of cal. jal. and ginger was given.

On the 23d, doses of calomel and opium were ordered. The ointment, the warm bath, and the purgative, were continued; and in the evening an injection was administered; still the symptoms continued to increase.

On the 24th, the cold bath, in place of the warm, was ordered. With a little force, he could be bent to the sitting posture in the bath. The former prescriptions were continued. Still the symptoms were increasing.

On the 25th, he could not be made to sit in the cold bath. A solution of ℥iss. of sulph. magn. in cinnamon water was ordered.

26th.—The notes do not mention whether the physic operated. The cold affusion was ordered. Opiate frictions were set a-going, and a dose of jal. coloc. and ginger was given.

On the 27th I was requested to attend, and saw him, for the first time, at twelve o'clock at night. By this time his jaws were closely and immoveably locked; and his whole body was stiff, and bent backwards into an arch, so that he rested on the occiput and heels. From the first he had enjoyed but little sleep; now it had entirely departed from him. His urine had been all along scanty. The skin was dry, except over the head, face, and neck, which were for the most part bedewed with sweat. His chief pain lay in the pit of the stomach, where there was an evident swelling. He described the pain to proceed from this spot over the body. For the last 28 hours purgatives and injections had failed to procure any alvine discharge. Deglutition had all along been difficult; but now he could scarcely manage to swallow any fluid introduced between his teeth, and never without inducing a paroxysm. The paroxysms were now frequent, and accompanied with difficulty of breathing, sometimes almost amounting to suffocation, and with palpitation of the heart, which could be seen and heard. His breath had the mercurial fœtor. As the medicines were now producing no effect; as the disease seemed to be fast approaching to a fatal termination, for which indeed the poor boy became impatiently desirous, and was calling upon his nurse to put an end to his miserable existence; and as hydrophobia, which is very much a kin to tetanus, has been of late repeatedly cured by blood-letting, we resolved to give the lancet a fair trial. Accordingly we drew from the arm ℥xvi. of blood. Before the blood ceased to flow, he was able to open his jaws to the extent of half an inch. The opportunity was seized of giving a smart dose of purgative medicine, which was ordered to be followed by repeated doses of castor oil. An injection, which was thrown into the rectum by a syringe, was immediately ejected past the syringe with considerable violence against the foot of the bed. The relaxation produced by the bleeding gradually disappeared, and in a few hours the jaws became as locked, and the body as stiff, as ever. The bleeding was repeated in about 10 hours afterwards, and produced a relaxation of the jaws, though not to the former extent. The physic had now begun to operate. A smart dose of calomel was given, and ordered to be followed by a solution of sulph. magn. in cinnamon water. Repeated doses of sp. eth. nitr. were also ordered. In a few hours the effects of the bleeding had almost disappeared. On the evening of the 28th, and on the 29th, the bleeding was repeated, and on both occasions produced considerable relaxation, though not to the first amount. He had now been bled four times, and had lost 5 ℔ of blood. After every bleeding the pulse became exceedingly quick, but by and by subsided to about 110. Brisk purging with sulph. magn. and an occasional dose of cal. was kept up. Under all this depletion, his whole food was a little soup, so that he had now become exceedingly weak. For fear he would die in our hands, we did not venture to take more blood till the 30th, on the morning of which day he seemed a little recruited. The body still formed an arch from occiput to heel; and

the jaws were locked, though not quite so immoveably, as before the bleedings. This difference, however, we were disposed to place to the account of debility. From the regular temporary amelioration of the symptoms after every bleeding; from the inefficacy of every thing else; and from the hopelessness of the case, if left to itself; we resolved, after much hesitation, to make one desperate trial more with the lancet. The vein was opened. A considerable stream of blood issued, and the finger was held to the pulse till it became exceedingly quick and feeble; when the poor boy all at once became exceedingly low, and seemed ready to expire at every paroxysm. Having tied up the arm, we retired hastily to another room, and agreed to let him have as much wine as the friends could get him to swallow, more as a mere cordial, than as a medicine from which any benefit could now be expected, and announced that we did not expect that the patient would survive long. Next morning, however, we found our young patient in a state of intoxication, and much relieved. As there had been all along a scarcity of urine, we substituted gin to-day for the wine, and kept him in a state of half intoxication for ten days. Purgation was at the same time kept up. During this period it was remarked that he ejected his urine to a great distance; on one occasion to the distance of 14 feet. His recovery was very slow and gradual; so that, for many days after the gin was commenced, he was lifted about like a piece of wood. Not long after he began to walk about, his motions were stiff and slow. He has, however, long since regained his former health, and strength, and activity.

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James Campbell Wright, Cowcaddens; aged 49; five feet and seven inches high; thick; muscular; well shaped; strong and agile; the hair on the face red; the hair of the head formerly lightish brown, but, since the late shavings and blisterings, approaching to black; has all along enjoyed excellent health; has occasionally taken a hearty glass with his friends, but upon the whole has been a man of sober and religious habits. On Jan. 27, 1816, at eight o'clock in the morning, in lifting a deal, a little splinter, not so thick as an ordinary pin, made its way into the middle of the fore part of the first phalanx of the right thumb, and was immediately taken out by his comrade. The circumstance was quite forgotten till next day, Sunday, about 12 o'clock mid-day, when the part became painful and swollen. About five o'clock in the afternoon, a small suppuration of about the size of a pin's head was discovered in the spot from which the splinter had been extracted. As soon as the matter, which was of thick consistence, was let out, the pain diminished, but the thumb continued to increase gradually in size, and to become weaker and weaker. By ten o'clock next morning the whole right hand had become so powerless, that he was unable to lift a tool, or even a breakfast knife. By 12 o'clock the hand had become cold, and he was warm-

ing it at the fire, when in an instant he was seized with an excruciating pain in the situation of the carpal bones; the back of the head forthwith swelled, and became a little red. At three o'clock he applied to Mr. Roderick Gray, surgeon, Cowcaddens, who ordered the whole hand to be covered with cloths dipped in vinegar and strong spirits. No relief was obtained. As the pain was increasing, and as Mr. Gray was from home, I was called in the evening. He was screaming aloud, and moving restlessly from place to place in severe agony. There was a slight ruffling of the cuticle, where the speck of suppuration had formed. The chief pain was in the first phalanx of the thumb. I immediately made an incision down to the bone from the borders of the first joint to the tip. A good deal of black blood issued, but no pus could be discerned. The whole hand was immediately immersed in warm water, and afterwards covered with a poultice, and a smart dose of cal. and jal. was ordered. The pain of the thumb was immediately removed, and the pain in the back of the hand much mitigated.

On the morning of the 30th I met Mr. Gray in consultation. We learned that since the incision was made into the thumb, it had been free of pain, but that in about half an hour the pain in the back of the hand gradually increased, and at length produced screaming and restlessness, as before. We applied eight leeches, and ordered them to be followed by warm fomentations, and afterwards by a large poultice. A smart dose of calomel and jalap was given in the evening.

On the 31st the same number of leeches was applied, and purgation kept up. Still the pain and swelling increased.

On Feb. 1, eight leeches more were applied. While they were at work, he felt a pain suddenly ascend from the hand to the back part of the head. The hand now became easier, but the pain in the back part of the head became proportionally violent. The head was shaved, and rubifaciants, and sinapisms, and blisters, were successively applied, with some temporary, but no permanent, benefit.

By the evening of the 4th the neck and jaws had become somewhat stiff and painful, and uncommon anxiety was expressed in the face. Mr. Gray having shortly before had a case of bruise which fell a victim to tetanus, and being struck with the similarity of the symptoms before him with those which had so lately made a deep impression on his mind, was the first to discern the approach of this horrific disease. On examining more attentively, we found that the jaws could not be fully opened, and that the stiffness of the neck was considerable. As, however, calomel had been repeatedly given, and might possibly be the cause of all this pain and stiffness, we were willing to wait till morning. An ounce of castor oil was in the mean time given.

Mr. Gray saw him at seven next morning, and found tetanus advancing with rapid strides. At ten we met. By this time the jaws during the intermissions scarcely admitted the point of the

little finger, and during the paroxysms were closely locked. The stiffness of the neck was greatly increased. The head was fixed immoveably during the intermissions; and during the paroxysms was drawn forward, with the face turned a little towards the left, and with the occiput of course inclined as far towards the right. Often while the attendants were laying him back in bed, and when the head was yet a few inches from the pillow, he was drawn forward with a sudden convulsive jerk to the sitting posture. There was always great difficulty of deglutition; and, during the paroxysms, of respiration. We immediately filled nine large sized tea-cups with blood from his arm. The quantity, taking in what was in the plate and on the floor, must have exceeded four pounds; and the stream was the largest that either of us ever witnessed. The tetanic symptoms immediately abated. A few pil. aloes c. coloc. were given, and gr. iiiss. of opium were left to be taken in an hour.

In the evening the jaws and neck were still a little stiff, and there was considerable pain in the back of the head. We contented ourselves with giving a little physic.

Next morning, at 10 o'clock, finding not only the stiffness of the jaws and neck, but also the pain in the back of the head, greatly increased, we filled five saucers with blood from the temporal artery. The quantity of blood might amount to ℥xxv. During the operation he almost fainted twice. The same dose as yesterday of opium, preceded by a few purgative pills, was ordered. From this time forward we did not witness one formidable symptom of the disease, although for eight weeks he continued to be more or less troubled with convulsive contortions of the head. Purgatives during the day, and opiates at night, were continued till every spasmodic symptom disappeared. To follow out the history of the hand is at present unnecessary. Suffice it to mention that at this moment a probe can be made to rattle amongst the carpal bones, and that he is hesitating about allowing an operation to be performed. In other respects he is at present in perfect health and strength.

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The subserviency and adaptation of the voluntary powers throughout all the animated world to the cravings of the stomach, and the intimate sympathy that is conspicuous at every step of pathological research between the stomach and the voluntary organs, warrant the conjecture that the stomach is the primary seat of this disease. The pain and swelling in the epigastric region, and the appearances after death, almost confirm this opinion. I attended a farmer a few years ago in the parish of Cumbernauld, who laboured under an hysterical disease, which passed through the following succession of symptoms every morning:—He became languid, and unable to speak; the head by and by fell down upon the breast; then, with both fists clenched, he gave the breast a number of most unmerciful blows; then with gaping mouth bellowed out a wild convulsive laugh; then served the head as he had done the chest; then screamed out an-

other wild laugh; after which he often got free belching off flatus from the stomach, and soon recovered. But when the flatus could not at this period escape, his whole body became stiff, and straight as the trunk of a tree, and rolled along the floor; at length, after another laugh, and free belching, the fit terminated. I call it a hysterical disease, in distinction to epileptic, because he could hear and remember what was said to him during the fit. In this case the stools, which were regularly preserved for my inspection, emitted large bubbles of gas. This man, before the fits had entirely left him, underwent, by advice of a strolling doctress, the following notable remedy:—A bannock made of his urine and meat was given to the dog. Soon after this the fits entirely disappeared. The cure was ascribed to the doctress; and the medicines, in spite of every remonstrance, were discontinued. The disease has, it seems, lately returned. From this case, I argue that, if the œsophagus had persevered in refusing exit to the flatus, the fit would have continued, and the man would have died of tetanus, or something like it. Now Larrey says, that "in the examination of the bodies of persons dead of tetanus," he "found the pharynx and œsophagus much contracted, and their internal membrane red, inflamed, and covered with a viscid reddish mucus." The rationale, then, of the treatment in the above cases is, first to reduce the irritability and tone of the parts exterior to the stomach, and then to brace up the stomach, and restore it to the exercise of its functions. Although distention of the stomach is the primary cause of the disease, it is not to be understood that this condition of the stomach remains throughout the disease the solitary and unassisted cause of all the symptoms. This condition of the stomach forbids sleep, so necessary to the animal functions, and lays a restraint upon all the secretions and excretions in the body. Thus the original cause soon spreads from the stomach over all the vital organs. It is by this very spreading and gradually involving other organs in the disease, and at the same time locking them up so that they cannot assist with their critical discharges, that tetanus, as it advances, gathers so much strength, and, when unassisted, proceeds so rapidly to a fatal conclusion. I remain, Sir, most respectfully,

Your obedient servant,

JOHN CROSS.

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## ARTICLE VIII.

### *Remarks on Mr. Watts's Paper on the Length of the Pendulum.*

(To Dr. Thomson.)

SIR,

I trust your ingenious correspondent Mr. Watts will forgive the remark that I think he has been rather hasty in censuring Govern-

ment for not connecting the pendulum experiment with the trigonometrical survey of England, Scotland, and Wales, unless he has received *positive* information that the use of the pendulum is to be neglected altogether by the gentlemen who so ably conduct that interesting work. It behoves us to avoid judgments, and, above all, *censorious* judgments, upon insufficient evidence. The main object of the Board of Ordnance in carrying on this grand undertaking for so many years, has been to produce a series of correct county maps, upon a much larger scale than has yet been attempted. These maps are now in course of publication; and I believe all persons who have had opportunity of examining them have been as much struck with their beauty as with their accuracy. As, however, the work is conducted by men of science, it is not likely that the mere business of producing faithful maps is all to which they will attend. They have already, as is well known to the readers of your *Annals*, done much in relation to the measurement of degrees on the meridian. There is also an eminent geologist, Dr. Maculloch, connected with the work; and at this moment, I believe, going from station to station with Col. Mudge and Capt. Colby, or with whichever of those gentlemen may now be in Scotland. It has, also, been all along understood by persons who have occasional intercourse with those gentlemen, that before the operations are finally terminated, the length of the second's pendulum will be determined at the principal astronomical stations in their progress from south to north, a clock and other apparatus being provided for that purpose; and that the grand experiment in reference to the attraction of mountains will again be repeated at the place which they shall judge to be most suitable, after they have completed the trigonometrical reticulation of Great Britain. It has, I say, been all along understood that these are parts of the general plan; and it is surely only a moderate exercise of common candour, to leave the gentlemen who so ably conduct this important undertaking to work out their own intentions in their own way, and not to take it for granted that they have made up their minds to neglect a class of operations simply because they have not advertised to the world how or when they mean to set about them.

I can scarcely doubt that Mr. Watts will agree with me as to the justice of the preceding observations: nor can I believe that he will be angry with me if I express my apprehensions as to the accuracy of his own results with regard to the length of the second's pendulum at Plymouth. An experiment of so much nicety as this, in which the difference of latitude between 50° and 55° does not occasion a variation in the length of the pendulum exceeding $\frac{18}{1000}$ths of an inch, requires a variety of precautions which do not seem to have occurred to this gentleman.

1. He has neglected to introduce the *weight* of the thread of *pite* into his computation. The French philosophers, in their observations for a like purpose, have not fallen into this source of error,

although the wire to which their ball was attached weighed but little more than the 1000th part of the ball itself.

2. There is a source of error arising from the comparatively gross mode of suspension adopted by Mr. Watts, against which he does not appear to have guarded.

3. The length of the proof pendulum was ascertained *before* it was suspended; instead of which it ought, doubtless, to have been ascertained *while* it was hanging vertically in a state of quiescence: or, still better, by the distance between the point of suspension and a horizontal plate, which by means of a fine micrometer screw had been gradually elevated so as just to touch the bottom of the ball while in gentle motion. Here is another source of inaccuracy.

4. Mr. Watts does not seem to have made any allowance for want of uniform density in his brass ball, by suspending it in different positions, and taking a mean of the results.

5. He has not applied the obvious reduction for the length of the circular arch through which his pendulum oscillated. That arch was not so small as to render this reduction unnecessary.

6. He has neglected the correction requisite on account of the altitude of the place of his experiment above the level of the sea. Perhaps, however, he was so near that level as to render such correction needless; yet that circumstance ought to be specified.

7. Another error is involved in the shortness of the time devoted to his experiment, which was only 480 seconds. Now the French philosophers took, at each station, a medium of a series of about six distinct experiments, each of them occupying about 4390 seconds, or more than nine times the duration of Mr. Watts's. The error in measuring the interval of time would with them be reduced to one-ninth of what it would be in Mr. Watts's experiment (supposing both to be equally accurate in this respect): and this error diffused over six equally careful experiments, would give the probability of correctness *in this respect alone* in a ratio of more than 50 to 1 against Mr. Watts.

8. If he *alone* "counted" the number 353 of vibrations, it is impossible to affirm positively that *that* was the number. The well known method of *coincidences* is the only one that can here be relied on, and even that requires extreme care.

9. Lastly, the error in measuring the length of the string of pite, whatever it may have been, would be diminished in the ratio of 19 to 9, by making the experiment with a two second pendulum, instead of that which was actually employed.

I beg to assure Mr. Watts that these observations do not spring from any invidious motive, but are simply presented for the purpose of suggesting to him a few of the nicer and more delicate particulars which *must* be regarded in the pendulum experiment, before its result can be received with confidence by men of science. I shall be happy to learn that he is induced, after meditating upon these hints, to repeat his experiment: and, in that case, I would beg to refer

him, for a variety of expedients to insure correctness, to which I have not here adverted, to the Astronomie Theorique et Pratique of Delambre, chap. 35, or to the third volume of Astronomie Physique, by Biot; in both which he will find perspicuous descriptions of the French apparatus and operations in relation to this inquiry.

For my own part, however, I am inclined to think that the most accurate way of determining the length of the seconds', or other pendulum, would be to cause uniform cylindrical bars, or prismatic plates, properly suspended, to oscillate in appreciable arcs. There are many peculiarities and advantages (with some disadvantages, I confess) attending the experiment with such pendulums as these; and solicitous as I am to see the experiment made with adequate care and skill, in all possible situations, I should gladly contribute my mite by entering a little into detail. But, looking back to what I have written, I perceive that it will be expedient to defer any further remarks to a future opportunity.

I am, Sir, yours, &c.

*Oct.* 4, 1816. R. M. A.

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## Article IX.

### *On the Horse Leech as a Prognosticator of the Weather.* By Mr. James Stockton.

There is scarcely any subject in which mankind feel themselves so much interested as that of the weather, and the continual changes it undergoes; but of the causes which produce those changes they are, for the most part, ignorant. It is only by diligent attention and patient inquiry that we can be enabled to discover the combinations and conclusions of those meteorological phenomena, which at present appear so unconnected and imperfect. As a means of accelerating and improving our knowledge, in this respect, several instruments are in use, among which the barometer, as an indicator of the fluctuations about to take place in the air, is of peculiar utility. Indeed, with regard to early intelligence, it is deservedly esteemed the best, although it must be admitted that the rules drawn from it are not unfrequently fallible. Excellent, however, as this instrument confessedly is, I am led, from long and careful observation, to believe that much surer indications may be drawn from the brute creation. Their various motions, the uneasiness under which they labour, and the precautions they take previously to any atmospheric variation, not only strikingly discover the acuteness of their feelings, and their extreme susceptibility of the impressions of natural causes, but also convey accurate prognostications of a change in the weather. That this was a circumstance not unknown to the ancients is evident from several of their writings,

and particularly from the Georgics of Virgil, in which it is observed that cows and various other animals are uncommonly affected before rain. Several others have also been remarked for this discriminating faculty; but that to which I have chiefly confined my notice, and that, in fact, which appears, from a long series of regular and diligent observations, best entitled to notice, is the horse leech; and it is the intention of this article to record a few remarks on its peculiarities, as exhibited by one kept in a large phial, covered with a piece of linen rag, three parts full of clear spring water, which is regularly changed thrice a week, and kept in a room at a distance from the fire. In fair and frosty weather it lies motionless, and rolled up in a spiral form at the bottom of the glass; but prior to rain or snow it creeps up to the top, where, if the rain will be heavy, or of some continuance, it remains a considerable time; if trifling, it quickly descends. Should the rain or snow be likely to be accompanied with wind, it darts about with amazing celerity, and seldom ceases until it begins to blow hard. If a storm of thunder and lightning be approaching, it is exceedingly agitated, and expresses its feelings in violent convulsive starts, at the top or bottom of the glass. It is remarkable that, however fine and serene the weather may be, and when not the least indication of a change appears, either from the sky, the barometer, or any other cause whatever; yet if the animal ever quit the water, or move in a desultory manner, so certainly, and I have never once been deceived, will the coincident results occur in 36, 24, or even in 12 hours, though its motions, as I have before stated, chiefly depend on the fall and duration of the wet, and the strength of the wind, as in many cases I have known it give above a week's warning.* However unimportant this subject may appear to some, yet as it manifestly tends to assist us in the investigation of natural phenomena, should any of your readers feel inclined to give it the consideration it merits, any satisfactory result of their inquiries must be highly interesting, and will greatly oblige your humble servant,

*New Malton, Oct.* 3, 1816. I. S.

* Previously to the late uncommon fall of rain in this part of the island, in July, the leech was in continual motion; but about two days before the wet set in, it remained stationary all that time out of the water at the top of the glass, and never once descended until the rain came on. This the animal always does when the rain begins to fall; but when the air clears up, and another shower is at hand, it is again on the alert.

## Article X.

### Analyses of Books.

I. *Entomologie Helvetique, ou Classification des Insectes de la Suisse rangés d'après une nouvelle Methode : avec Descriptions et Figures.—Helvetic Entomology, or a Classification of the Insects of Switzerland according to a new Method, with Descriptions and Figures.*

The author (M. Clairville) published the first volume of this work at Zurich, in 1798. The introduction, which occupies about 20 pages, is taken up with the enumeration of the advantages resulting from the cultivation of entomology, and with descriptions of the head, the eyes, and their situation, the insertion and structure of the antennæ and tarsi, which, the author observes, are exactly the same in every species of the same genus, and therefore their precise form, &c. particularly demands the attention of the student. He next presents us with a tabular view of the classification of insects, which he divides into those

*With Wings.*

| | | | Sections. |
|---|---|---|---|
| With jaws | with | Crustaceous wings | 1. Elythroptera. |
| | | Coriaceous wings | 2. Deratoptera. |
| | | Reticulated wings | 3. Dictyoptera. |
| | | Veined wings | 4. Phleboptera. |
| With a rostrum | wings | With balances | 5. Halterata. |
| | | Dusty | 6. Lepidoptera. |
| | | Mixed | 7. Hemimeroptera. |

*Without Wings.*

| | |
|---|---|
| With a rostrum | 8. Rophoteira. |
| With maxillæ | 9. Pododunera. |

He apologises for having changed the Linnæan nomenclature, which he justly observes is not sufficiently precise; and after a few observations on that subject, divides the elythroptera into those having their elytra with a suture, 1. Longer than half the body; 2. Shorter than half the body: 3. Without a suture, one slightly overlapping the other—and makes some general observations on the insects of that section.

The remainder of the volume is occupied with an arrangement of the rhyncophorous insects (curculio, Linn.), which he divides into 11 genera, and with descriptions of some species, elucidated by 16 copper plates.

The second volume was published in 1806, and contains the

Linnæan genera *cicindela*, *carabus*, and *dytiscus*, or those insects with six palpi, and who feed on other insects. It is infinitely superior to the former volume, and abounds with excellent observations on the numerous genera on which he treats, and contains an enumeration of the Swiss species, illustrated with 32 plates. We are extremely sorry that we cannot admit of a more detailed analysis; but we earnestly recommend the perusal of it to the scientific entomologist.

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II. *A Practical Essay on Chemical Re-agents or Tests: illustrated by a Series of Experiments.* By Frederick Accum, Operative Chemist, Lecturer on Practical Chemistry, on Mineralogy, and on Chemistry applied to the Arts and Manufactures, &c. London, 1816. 12mo.

This little book will be found of considerable utility to the chemical student. It contains an account of 64 tests, the method of preparing them, the purposes to which they may be applied, and the precautions to be observed in drawing deductions from their effects. It contains, likewise, a list of all the different bodies that may be detected by tests with references to the tests which must be employed. Perhaps Mr. Accum might have diminished the number of his tests without any injury to the student. The enumeration of tests which do not answer the purpose rather tends to mislead than to instruct. For example, Kirwan proposes muriate of alumina as a test of the presence of carbonate of magnesia in water. It throws down, he says, a carbonate of alumina. Now I have strong doubts about the existence of a solid carbonate of alumina. At any rate, I have never been able to form it. I may notice here what I presume is only a typographical error. In p. 109 there occurs, "Mr. Gahn, the celebrated German mineralogist." I dare say that Mr. Accum is sufficiently aware that Assessor Gahn, celebrated as a *chemist*, having been the person who first ascertained the nature of earth of bones, and who first reduced manganese to the metallic state, is a Swede, not a German.

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## Article XI.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

The Society met on Thursday, Nov. 7. A paper by Sir Everard Home, Bart. on the Circulating of the Blood in the *Lumbricus Marinus* was read. The author is of opinion that animals form a connected series from man, the most complicated down to the simplest of all the animals, scarcely distinguishable from vegetables

in its structure. He thinks, too, that the distribution of the blood constitutes one of the best means of tracing this series. In each class of animals there is something peculiar in the circulation, which belongs to all the genera of the class. This is the case with the molusca, as well as with the other classes. It was this circumstance that induced the author to endeavour to trace the circulation of the lumbricus marinus. The heart, consisting of only one ventricle, is very small, and situated in the back of the animal. It sends an artery towards the tail. It communicates with a vein which transmits the blood to the 26 branchiæ in which the blood is aerated. From these branchiæ it is transmitted back again to the heart. The teredo navalis, the lumbricus marinus, and the lumbricus terrestris, constitute three members of the series; the circulation becoming gradually simpler in each. In the last the blood-vessels themselves carry on the whole circulation.

On Thursday, Nov. 14, a paper by Dr. Rawlins Johnson was read, on the Mode of Propagation of the Hirudo Vulgaris. This little animal is found in abundance in small rivulets, under stones, in those parts where there is little current. It is in length from an inch to an inch and a half. Its colour is brown; and there is a black line from the head to the tail, running down both the back and the belly. It copulates like the snail, and is of course hermaphrodite. It is oviparous, producing capsules containing ova. The young in them show symptoms of vitality in about three weeks, and they are protruded from the eggs in about three weeks more when exposed to the sun, but in the shade not before five weeks. The hirudines destroy each other's capsules. Dr. Johnson gives a minute account of the way in which the capsules are protruded from the uterus of the animal. The greatest number of capsules protruded by a single hirudo was 12. These, supposing one-third of the eggs only to be productive, would have produced 36 young. He thinks that it may be concluded from analogy that the hirudo medicinalis is also oviparous. This point had been already settled by Bergman, who published a paper on the leech in the Memoirs of the Swedish Academy for 1757. The young animal, when first hatched, is colourless, and it continues in that state for some months. It has the property, as had been already mentioned by Muller, of swimming upon the surface of the water with its belly uppermost.

On Thursday, Nov. 21, a paper by Dr. Wilson Philip was read, on the Efficacy of Galvanism in relieving difficult Breathing. The author thinks that it may be considered as established by his preceding papers that galvanism is of little or no service in diseases of the sensorium; but that it will be found an important remedy in all cases when the disease is occasioned by the diminution of the nervous energy. The dyspnæa induced by cutting the eighth pair of nerves which supply the lungs, being exactly similar to asthma, induced the author to expect that it would be found an important remedy in that disease. The trials which he has made confirm the accuracy of this opinion. In about 30 cases, in which galvanism

has been applied by him, every patient was relieved, and several permanently cured. His method was to apply the negative wire from the galvanic battery to the pit of the stomach, and the positive wire to the nape of the neck. About 16 pair of four-inch copper and zinc plates were as many as could in general be endured by the patient. At first only six or eight were all that the patient in many cases could bear. He increased or diminished the number by slipping one of the wires along the trough, according as the feelings of the patient required an increase or diminution of the energy. From five minutes to a quarter of an hour was the time during which the galvanism was applied. He did not find any advantage from prolonging the application beyond the time when the breathing was relieved. He in various cases deceived his patients by pretending to apply galvanism, when in fact one of the wires was not in communication with the trough; but in no one instance was the patient relieved by this pretended application; while the real application always alleviated the difficulty of breathing. The liquid with which the trough was charged was water mixed with one twentieth of its weight of muriatic acid.

## LINNÆAN SOCIETY.

The Society resumed its meetings on Tuesday, Nov. 5. An account was read of a non-descript animal thrown out of a pump well at Hull. The account was drawn up by Mr. Harrison, and some particulars were added by Mr. Hayworth, who presented the paper to the Society. It was a kind of serpent about a foot long. Its principal head was cut off before it was observed; but it was supposed at first to have had nine heads, and therefore to have resembled the hydra of the ancients. But Mr. Hayworth conceives that these may have been rather connected with the creature's lungs. Unfortunately, the animal was so much injured before it was examined, that even the genus to which it belonged could not with certainty be made out; though Mr. Hayworth supposes that it may probably have belonged to the genus ophis.

At the same meeting Mr. Bicheno's paper on British Junci was continued.

On Tuesday, Nov. 19, a paper by Dr. Arnold was read on the Alcyonite, a fossil found in flint in the counties of Norfolk and Suffolk. These petrifactions, it is well known, have been taken for fruits; but the general opinion at present is, that they are of an animal nature. Dr. Arnold gave a minute description of a variety of specimens, accompanied with figures, but did not venture to determine their species. He is inclined to the opinion that, notwithstanding the various appearances which they assume, they may all belong to two, or perhaps even to one species of animal.

## ROYAL INSTITUTE OF FRANCE.

### *Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year* 1815.

### Mathematical Part.—*By M. le Chevalier Delambre, Perpetual Secretary.*

#### MEMOIRS APPROVED BY THE CLASS.

#### ANALYSIS.

(*Continued from* p. 387.)

*Lighthouse with Parabolic Reflectors.* By M. Lenoir. Commissioners, MM. Charles, de Rossel, and Arago.

Numerous researches have been made at different times on the important question of lighting the sea coasts; but few of the results have been published. This art, so useful to navigation, had made but little progress among the ancients. We find nothing certain, either respecting the nature of their lighthouses, or the vivacity of their light. Since navigation has become more adventurous, the necessity has been felt of diversifying the colours of the lights, and adding to their intensity. For this purpose pit-coal was employed, and wood and spherical or parabolic reflectors were used. The light was concealed, and suffered to appear at regular intervals. But the reflected light, being too feeble in itself, was perceived only in the direction of the axis. The invention of lamps with a double current of air revived the hopes of the artists, who were employed with a very laudable zeal in the construction of reflectors, to which they succeeded in giving a sufficient polish and the form of a paraboloid of revolution. It remained to make a set of experiments on the apparatus thus completed, in order to determine, or even improve, its advantages.

The commission appointed to examine the large and fine reflectors which M. Lenoir had just constructed in his manufactory, laid hold of the opportunity to subject to decisive experiments the delicate question respecting the diameter requisite to be given to the flame placed in the focus of the paraboloid.

The first experiments had been made on Oct. 1, 1813, in the garden of the depot of the Marine, with two parabolic reverberators, perfectly similar, of 0·81 metre of opening, and 0·325 metre in depth, the one having in the focus a wick of 16 lines, and the other one of 12 lines. Each of these reverberators was directed with the greatest care upon a white surface distant about 50 metres. A small opaque body placed at a short distance from the screen furnished two shadows, the comparative intensities of which served to determine that of the two cylindrical pencils of light which the mirrors sent to the screen. The comparison of these

two shadows left no doubt of the advantages of the smaller wick, which gave a light at once more lively and more white than the larger one.

This experiment being repeated on Oct. 8, with very slight differences, gave the same result.

The commission met again on Oct. 15, to compare the effects of two wicks of 27 and 20 millimetres. The place of these wicks was changed, to be certain that the difference of light did not depend upon the more or less complete polish of one or the other reflector.

On Oct. 22 the experiment was made with three reflectors at once, the two just mentioned, and a third of 13 millimetres in diameter. The comparison of the shadows showed that the smallest had the advantage, both in the whiteness and intensity of light, over that of 20 millimetres, while this in its turn had the advantage over that of 27 millimetres.

It was of importance to repeat these experiments at a much greater distance, to render more sensible the effects of the divergence produced, either by the eccentricity of the wick, or by defects in the form or polish, which it is impossible entirely to avoid. It was also necessary to see whether the advantages of the small wick would continue at greater distances.

The three reflectors were transported to the foot of the tower of Montlhery, and directed to Montmartre, which is at the distance of more than 28,000 metres. They were so disposed that they could be all three seen at once in the field of the same telescope.

The first experiment took place on Nov. 15, and showed that at a distance, as well as when near, the smaller wick gave out most light in the direction of the axis of the paraboloid.

The same evening observations were made on the divergence of the light, and its intensity at different angular distances from the axis. It was ascertained that the changes of direction had not been made with all the requisite precision. Some modifications were immediately applied to the supports of the mirrors, to enable the angular motion of the axis of each reflector to be measured with precision. After these changes the definitive experiment was made, of which the following are the principal results :—

The two reverberators of 13 and 27, when seen by the naked eye, appeared only one, but extremely vivid. In the telescope they were separate, and that of 13 was sensibly more lively than the other. A single reverberator was still seen perfectly by the naked eye.

The two reverberators were equally vivid when a third part of the surface of the first was concealed. The reverberator with a great wick had the advantage when the half of the other was covered. From this it seems to follow that the mirror with the small wick is about one-third more powerful than the other.

A motion of 2° was given to both reverberators. The fires appeared weakened, but remained visible to the naked eye. That of 13 preserved its advantage.

With a deviation of 3° the diminution of light increased. The fires could with difficulty be seen by the naked eye. The small wick had constantly the advantage.

At 5° they were no longer visible to the naked eye. But some traces of them could be observed by the glass. The one best seen was that with the small wick. The reverberators were brought to their original position. Deviations of 2° and 3°, the contrary way, were attempted. These new proofs fully confirmed the results of the former ones.

The lamp of 27 millimetres consumes in two hours 245 grammes of oil. The lamp with the small wick, which always gave the best light, burnt at the same time only 122 grammes, or one half less than the other.

The conclusions of the report are—

1. That a single reverberator at the distance of seven leagues, or 28,000 metres, appears at least as bright to the eye as a star of the first magnitude, when the observer is placed in the continuation of the axis.

2. That at 3° from that direction the light of the reflector has lost all its brilliancy, and can scarcely be seen without a glass.

3. That instead of increasing the focal diameter of the light as artists are accustomed to do, proportionally to the dimensions of the reverberator, it is proper to reduce it as much as possible, that is, as much as the free circulation in the inner tube of the wick will permit.

4. That by this means the expense of oil is considerably diminished, while at the same time the intensity of the light is augmented.

5. That when a lighthouse is intended to give light to a sector of a certain extent, it is necessary either to render the reflectors moveable, that they may be directed successively towards different points of the horizon, or multiply them so that the axes do not include greater angles than 6°.

The experiments were made in concert with M. Sganzin, Inspector General of Bridges and Roads, in presence of several Members of the Institute, and of a great number of distinguished Engineers. They seem calculated to determine with accuracy what may be expected from light-houses with parabolic reflectors. The Commissioners, in consequence, propose to the Class to give thanks to MM. Lenoir, who in this new branch of industry have shown themselves deserving of the reputation which they have long ago acquired by important labours.

*Memoir on the Surfaces of Equilibrium of imperfect Fluids, such as Grain, Sand, &c.* By M. Allent. Commissioners, MM. de Prony and Girard, reporter.

"The intention of the author was not to give new developements to the theory of the pressure of earths, nor to add new facts to those that have been collected relative to this question, the complete solution of which would apply so usefully to civil architecture, hydrau-

lics, and the military art. He proposed merely to facilitate the graphical tracing of surfaces *with a natural slope, or a slope formed by the tumbling down of materials* within given limits, from the production itself of these surfaces derived from a single fundamental experiment. This generation is described in the memoir of M. Allent without the assistance of any calculus or figure with so much clearness of style and method as to leave nothing to desire."

*General Demonstration of the Theorem of Fermat respecting Polygonal Numbers.* By M. Cauchy. Commissioners, MM. Arago and Legendre, reporter.

Although the theory of numbers be much further advanced at present than it was in the time of Fermat, yet his fine theorem of polygonal numbers has hitherto been demonstrated only in its first two parts, which relate to the triangular and square numbers; so that every thing regarding the other polygons to infinity remained to be demonstrated.

It is surprising that mathematicians who have been able to overcome so many difficulties have been stopped hitherto before a simple question of numbers resolved by Fermat. (It is true that this solution has never appeared, Fermat having merely announced it. And since he never fulfilled his promise, we may suspect that he was not quite satisfied with his demonstration; that he still wanted something, either in clearness, accuracy, or brevity. Perhaps he only reached this famous problem by induction, and an analogy which had given him the hopes of finding a direct demonstration, which, on attempting, he found too difficult.) This kind of difficulty depends, no doubt, on the little connexion which we find between the different parts of this theory, for which it is necessary in some measure to invent a principle or particular method for each of the cases which we propose to resolve in succession. Hence it happens that the first case, relative to triangular numbers, was not demonstrated till long after the second case, which is quite independent of the general theory of polygonal numbers.

This difficulty, proved by the fruitless efforts of the greatest mathematicians, was a sufficient reason to induce them to desire the complete solution, which M. Cauchy has at last found.

He supposes the first two cases demonstrated. He supposes further, and it is the general result of the theory of polygonal numbers, that we may always resolve into three squares every number, which is not of the form $4^a (8 n + 7)$. To these propositions already demonstrated he adds a principle entirely new.

He remarks, first, that a number, $k$, being given, composed of four squares, whose roots make a sum equal to $s$, the quadruple of this number may be always represented by four squares, one of which is $s$?

Hence he concludes that two numbers, $k$ and $s$, being given, either both even or both odd, if $s$ included between the limits $\sqrt{(4 k)}$ and $\sqrt{(3 k)} - 1$, if besides $4 k - s^2$ is not of the form $4^a (8 n + 7)$, it will be always possible to decompose the

number $k$ into four squares, whose roots taken positively will form a number equal to $s$.

This proposition, very beautiful and very general, is the foundation of M. Cauchy's demonstration. It gives a remarkable perfection to the second case of the theorem of Fermat, since it gives the method not only of dividing a given number into four squares, but likewise of making the sum of the square roots equal to a given number taken within certain limits, which become further and further distant from each other in proportion as the number proposed becomes greater.

For the application which the author has in view, it is an essential condition to admit into the sum $s$ only positive roots, because the theorem of Fermat is restrained to polygonal numbers considered as positive.

This being understood, let P be any number composed of $n$ polygons of the order $n$. The general expression of this number will be of the form $A\,k + B\,s$, in which A and B are constant coefficients which depend only upon $n$; $s$ is the sum of the indexes of all the polygons, and $k$ the sum of their squares. The question will be to determine for each member P, the values of $k$ and $s$, with the condition that $k$ contains only $n$ squares at most, and that $s$ is the sum of their roots taken positively.

This question appears still too vague and indeterminate for analysis to be applied to it with success. M. Cauchy conceived the happy idea of restricting the problem by supposing that of the $n$ polygons which ought to compose the number P there are $(n - 4)$ equal to zero or to unity. Let then $k$ be an odd number not lower than 121, and sufficiently great for there being at least two odd numbers contained between the limits which correspond to the number $s$. M. Cauchy shows that the formula $A\,k + B\,s + r$ will represent all the whole numbers comprehended between the smallest and the greatest value of which this formula is susceptible on account of the limits of $s$. From this it follows that all the numbers may be decomposed into $n$ polygons, of which $(n - 4)$ shall be equal to zero or to unity.

On taking for $k$ greater and greater odd numbers, the formula will represent successively all the whole numbers from that which corresponds to the smallest values of $k$ and $s$ up to infinity. Consequently all these numbers are decomposable into $n$, polygonal numbers, of which $(n - 4)$ are equal to zero or to unity.

On examining, then, the small number of particular cases in which $k$ is below 121, he was soon led to the general conclusion that every whole number may be represented by the formula $A\,k + B\,s + r$, with the conditions prescribed, and that therefore every whole number may be decomposed into $n$ polygons of the order $n$, of which $(n - 4)$ are equal to zero or to unity. The supposition made in order to simplify the solution of the problem is found justified. Not only does M. Cauchy demonstrate the theorem of Fermat in all its generality for all polygons above squares, but

he substitutes for the theorem of Fermat a theorem much more precise and more interesting. Since he proves that of the $n$ polygonal numbers which enter into the composition of any number given there are always $(n - 4)$ equal to zero or to unity.

It results at the same time from the analysis of M. Cauchy that the effective decomposition of a given number into $n$ polygons of the order $n$ may be always done *à priori*, supposing only that we know how to decompose into three squares the numbers which are susceptible of that decomposition. The conclusion of the Commissioners is, that the memoir offers a new proof of the skill and sagacity which the author has shown in other researches equally useful to the progress of analysis and to mathematics, that it is worthy of the praise of the Class, and of being printed in the Recueil des Savans Etrangers.

*Memoir on the Libration of the Moon.* By MM. Bouvard and Nicollet.

The libration of the moon is one of the most curious and singular phenomena in the system of the world. Galileo and Hevelius had already observed that the spots near its edges disappeared and re-appeared alternately in certain circumstances, which they nearly ascertained. D. Cassini was the first that gave a complete explanation of these complex phenomena. He supposes, 1. That the moon turns round her axis so that her mean movement of rotation is precisely equal to her mean movement of revolution. 2. That if we draw three planes through the centre of the moon, the first representing the lunar equator, the second the ecliptic, and the third the orbit of the moon, the first plane will have to the second an inclination of $2\frac{1}{2}°$. Abstracting from the periodical inequalities these three planes have always a common intersection, so that the nodes of the lunar equator always coincide with the mean nodes of the lunar orbit. These beautiful discoveries were first published by Cassini in his History of Astronomy, afterwards reprinted in the eighth volume of the Memoirs of the Academy. The observations did not appear. Mayer undertook the subject again, and investigated it with care. He confirmed the determinations of Cassini except for the inclination, which he reduced from $2\frac{1}{2}°$ to $1° 29'$. Lalande afterwards increased this inclination to $1° 43'$.

Lagrange proved that the phenomenon of the inequality of the mean motions of revolution and nutation of the moon, that of the coincidence of the nodes of the equator and of the lunar orbit, and that of the constancy of their mutual inclination, are connected together by the theory of gravity. The analytical part of the problem is complete; but the difficulty attending this kind of observations occasions the astronomical part still to leave something desirable, and probably this will always be the case. The instruments employed by Mayer were very imperfect, and it is surprising that with such assistants he was able to perform so much. Lalande had much better instruments; but he merely made the number of observations strictly necessary, and he calculated them only by an in-

direct method, which is not sufficiently precise. The method of Mayer, though much better, was only an approximation; though it was easy to neglect nothing, as we demonstrated 23 years ago in a memoir announced by Lalande in the third edition of his Astronomy. M. Bouvard on his side made the same remark when all these reasons had induced him to undertake along with Arago a new suite of observations, which was interrupted by the prolongation of the meridian in Spain. M. Bouvard, who had calculated these first observations, continued them to 1810 inclusive. This new suite is the foundation of the calculations which MM. Bouvard and Nicollet have made at the same time, each separately, to be the more certain of avoiding any appreciable error. We find in their memoir all the formulas which they have employed with an exact discussion of all the terms, in order to show those which might be neglected, and those which it was indispensable to preserve; so that we may be certain that in this respect they have been much more rigid than any of their predecessors. They explain very fully the method which they have followed, and the elements which are the result of their labours. These elements differ very little from those that had been found 70 years before by Mayer. Thus the longitude of the spot observed is only 13′ less, the latitude 12′ less. The inclination is scarcely one minute less; but the difference between the node of the equator and that of the orbit varies almost 6° from that assigned by Mayer, since it was $3\frac{3}{4}$° minus according to Mayer; while from the 62 observations newly calculated by Bouvard and Nicollet, it is $2\frac{1}{8}$° plus. Respecting this, it is just to remark that of all the elements this interval between the nodes is the most uncertain, and the most difficult to determine, both on account of the small inclination, and the very great influence which the smallest geocentric errors have on the selenocentric places. This is easy to conceive when we recollect that the arc of 90° of the lunar globe is seen from the earth under an angle of 15 or 17 minutes. According to theory, the coincidence of the nodes ought to be perfect. In the old and new results the error has different signs, so that we may suspect that the truth may lie between the two results of observation, and consequently very near where the theory points out.

The memoir is terminated by large tables, which give all the details of the observations, the calculations, and the quantities which result from each observation in particular for the two elements of the spot. These results agree as well with each other as the observations allow; but lead us to desire that something could still be added to the accuracy of the instruments, which is by no means probable. It is only, therefore, by increasing the observations very much that we can diminish the uncertainty. But we must acknowledge at the same time that this extreme precision would be merely curious. It is sufficient for us to know that the theory and observations agree sufficiently with respect to phenomena which have no influence in astronomical determinations.

*(To be continued.)*

# Article XI.

## SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

### I. *New Theory of Dyeing Turkey Red.*

(To Dr. Thomson.)

DEAR SIR,

In Dr. Bancroft's late publication on the Philosophy of Permanent Colours, after treating of the various processes that had come to his knowledge for dyeing the Adrianople or Turkey red, he concludes by saying, "That so much uncertainty and obscurity should still prevail, in regard to this very estimable and extraordinary colour, is to me a matter of deep regret." I entertain the highest respect for Dr. Bancroft's talents and perseverance, by which he has been of great use to the practical dyer and calico printer; and while I do not pretend to throw complete light on a subject that has hitherto baffled the skill of scientific men, I may yet be allowed to state some doubts that have arisen in my mind respecting the theory which he mentions of animalization, in dyeing linen and cotton with the Turkey red. Dr. Bancroft, indeed, does not confine himself to this idea; for he allows that the animal substance must be joined to some earthy or metallic basis, to enable the cloth to imbibe and retain the colouring matter more copiously.

From the perfection to which that beautiful colour has been brought in this neighbourhood, I may safely assert that its brilliancy and permanency are unrivalled in any other part of the world. The process followed is to clear the goods of all impurities which they may have received from the spinner and weaver, by boiling in a strong alkaline ley, and afterwards by repeated immersions in an imperfect soap, made from the ley of carbonate of soda, olive oil, and sheep's dung. They are next steeped in warm water, into which is put a small quantity of soda ley; then the immersions are again repeated in the imperfect soap, omitting the sheep's dung.

This is the whole process previous to the galling and aluming. The ingredients used give very little sanction to the idea of animalization, as the sheep's dung, besides ammonia, can contain but a very small portion of animal matter. Dyeing with the madder is followed by another immersion in the oil and soda, which is called the darkening steep, and to this the clearing process succeeds.

The following ideas have occurred to me on this subject, which I suggest with diffidence; but should I be wrong, they may tend to excite some other person to an inquiry, that may prove more successful.

Silk and worsted have a natural varnish which cotton does not possess. To supply this defect, the repeated immersions, followed

by exposure to the atmosphere, and to the heated air of a stove, may give the oil the proper consistency by the absorption of oxygen, for forming a varnish with which the colouring matter unites, and through which it may be said to shine; which causes that superior brilliancy which the goods attain when they are cleared, or as it may be called, polished. I therefore presume that the fixedness and brilliancy of the colour will depend on the quantity of oil imbibed, as every repetition of drying presents new fibres to be varnished with an additional quantity; for I have always found that the permanency was in proportion to the number of manipulations in the saponaceous liquor, and a proportionable freedom could be also used in reducing or clearing. The white immersions, omitting the sheep's dung, are just, applying successive coats of varnish. Clearing is never attempted from the madder copper without immersing the goods again in soda and oil, and drying them in a stove, which I consider to be also supplying them with an additional coat.

The alkaline ley occasions a greater separation in the particles of the oil, by which it combines more closely with the fabric of the cloth. The sheep's dung in the first immersions may serve as a covering or great coat to keep the goods moist for a considerable time, that they may more fully imbibe the liquor, by preventing the evaporation from being too quick in the great heat to which they are exposed.

After the frequent immersions, the cloth feels like leather, no doubt from a superfluity of liquor. It is then steeped in a ley of carbonate of soda, and afterwards well washed and dried, as a preparation for the aluming and galling. The astringent principle has been long known for darkening and fixing common red colours on cotton, by uniting with the earth of alum, and strengthening the basis. To the use of blood in the madder copper, I attribute nothing; as, in the rancid and putrid state in which I have seen it used, were it not for the prejudice of the operator, it might be safely dispensed with.

In proof of the above idea, that it is wholly the oil uniting with the earth of alum that is of use, I may refer to the mode of dyeing that colour in the East, quoted by Dr. Bancroft, viz. soaking their cotton in oil (no matter of what description,*) during the night, and exposing it to the sun and air during the day for seven successive days, rinsing it only in running water, and then immersing it in a decoction of galls and the leaves of sumach previous to aluming.

I would, therefore, request the practical dyer who wishes to arrive at a knowledge of this unaccountable process, to give up the idea of animalization, if by it be meant impregnating the cloth with animal matter; and by the power of the microscope, or any better method, look for the whole truth from some other source than che-

* Olive oil, hog's lard melted, oil of sesamum, &c. have all been used with success.

mical analysis. I am at present inclined to believe that it is a mechanical operation united to a chemical; and that the frequent immersions in the imperfect soap are equivalent to laying on the first, second, third, &c. coats, preparatory to finishing a fine painting in oil.

I remain respectfully, Sir,

Your most obedient servant,

*Tradeston, Glasgow, Oct.* 26, 1816. JOHN THOMSON.

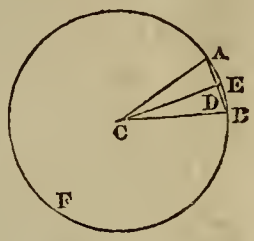
## II. *Demonstration that no Part of the Circle is a Straight Line.*

(To Dr. Thomson.)

SIR,

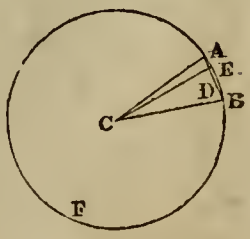
Seeing some of the pages of your *Annals* lately occupied with attempts to demonstrate that no part of a circle is a straight line, I have ventured to trouble you with the following demonstration, which I trust will not only be allowed to be strictly geometrical, but conclusive.

Let A B be any part of the circumference of the circle, A F. Through the points, A, B, draw the right line, A B, upon which, from the centre, C, draw the perpendicular radius, C E, meeting the right line and circumference in D and E; and joining C A, or C B. Then because the points D, E, are between the points A, B, (3 El. 3,) and the angle C D B a right angle, C B is greater than C D (19 El. 1); but C B is equal to C E; therefore C E is greater than C D; and consequently the point E falls without the straight line, A B; that is, the points A, E, B, are not in a straight line. But A, E, B, are points in the circumference of the circle; therefore the arc A B, in which these points are, is not a straight line.—Q. E. D.




Or more generally thus—

Instead of perpendicular to the straight line A B, let the radius C E be drawn any where between the radii C A, C B, intercepting this line, and the arc in D, E. Then in the triangle C, D, A, the exterior angle, C D B, being greater than the interior, and opposite C A D, or its equal C B D, it follows that C B or C E is greater than C D; and the point E, that is, every point in the arc A B, except A, B, is without the straight line A B. Therefore the part A B of the circumference, and the right line A B, must inclose a space. But two right lines cannot inclose a space; consequently the little arc, A B, is not a straight line.




There are many other ways by which this simple property of the circle may be demonstrated; but those I have given are, I appre-

hend, quite sufficient. Indeed, I should have thought that the 2d prop. of El. 3, presents us with a pretty plain proof of the curvature of every part of a circle.

I am, Sir, respectfully,

*Bristol, Nov.* 2, 1816. J. H.

### III. *Another Communication on the same Subject.*

(To Dr. Thomson.)

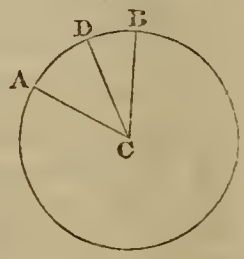
SIR, *London. Nov.* 4, 1816.

In some of the late numbers of your *Annals of Philosophy* there have appeared certain articles, which profess to show that no part of the circumference of a circle is a straight line. But the reasonings therein employed are very different from what the strictness of geometrical demonstration requires.

In order to facilitate calculations, and to make closer approximations to the quadrature of the circle, some geometers have assumed that its circumference is a polygon with very small sides. But in requiring so much to be granted, they have never insisted on its truth, as it is self-evident that the circumference of a circle is formed by the termination of equal mathematical lines drawn from a common centre, no two of which can end in a straight line.

The question to be demonstrated is, therefore, that a circle is not a polygon, or that no sensible magnitude of its circumference is a straight line.

I will not occupy your space by showing that the demonstrations of your correspondents do not accomplish this, but merely add that the question admits of no difficulty, and may be geometrically proved thus:—Assume a part, A B, in the arc of a circle as a straight line, and join A and B with the centre, C. And from any part, D, in A B, draw D C; A C, D C, and B C, are equal. But A C B is an isosceles triangle, having the angles at A and B equal: For the same reason the angles at A and D, and D and B, are also equal. But the adjacent angles at D are equal to two right angles, and they are also equal to the angles at A and B. Wherefore the angles A, B, of the triangle A B C, are equal to two right angles, which is impossible; A B, is, therefore, not a straight line.




In like manner it may be shown that no other sensible magnitude of the circumference of a circle is a straight line. Q. E. D.

If you consider this to be of sufficient importance for insertion in your *Annals*, it must be more for the sake of putting an end to the difficulties of your correspondents than from any merit the proposition possesses in itself.

I have the honour to be, with much respect,

A Student of Euclid.

IV. *Table of the Temperature of the Atlantic Ocean in different Degrees of Latitude and Longitude.*

| Latitude. | Longitude. | Temperature of the Ocean. | Period of the observation. | Observers. | Mean temp. of the air on the basin of the sea. |
|---|---|---|---|---|---|
| 0° 58′ S | 29° 54′ W | 80·96° | Nov. 1788 | Churruça | 80·6° (Cook). |
| 0 57 | 32 31 | 81·66 | April 1803 | Quevedo | |
| 0 33 | 23 40 | 81·66 | March 1800 | Perrins | |
| 0 11 N | 86 35 | 82·40 | Feb. 1803 | Humboldt | |
| 0 13 | 54 2 E | 80·78 | May 1800 | Perrins | |
| 25 15 N | 22 56 W | 68 | June 1799 | Humboldt | 69·8 (La Peyrouse and Dalrymple). |
| 25 29 | 44 14 | 70·88 | April 1803 | Quevedo | |
| 25 49 | 28 40 | 69·26 | March 1800 | Perrins | |
| 27 40 | 19 24 | 70·88 | Jan. 1768 | Chappe | |
| 28 47 | 20 37 | 74·30 | Oct. 1788 | Churruca | |
| 42 34 N | 18 5 W | 51·98 | Feb. 1800 | Perrins | 54·86 (Cook and d'Entrecasteaux). |
| 43 17 | 33 47 | 59·90 | May 1803 | Quevedo | |
| 43 58 | 15 27 | 60·62 | June 1799 | Humboldt | |
| 44 58 | 37 7 | 54·86 | Dec. 1789 | Williams | |
| 45 13 | 7 0 | 59·90 | Nov. 1776 | Franklin | |
| 48 11 | 16 38 | 57·74 | June 1790 | Williams | |

Humboldt's Personal Narrative, vol. ii. p. 65.

V. *Effect of the late Solar Eclipse on the Temperature of the Day on which it occurred.* By Luke Howard, Esq.

The radiation from the sun is so manifestly the cause of a diurnal elevation of temperature on the earth, that in a considerable eclipse of that luminary we ought to expect some diminution, for the time, as well of its heating, as of its illuminating effect; but I do not know that any one has yet submitted this consequence to the test of actual observation by the thermometer. In determining to do this, I thought it right to have a proximate standard wherewith to compare the results I might obtain; and, therefore, although the day was by no means so favourable for the purpose as some preceding clear ones, I caused a number of observations to be taken of the progress of the diurnal temperature on the 18th of the present month; and devoted the forenoon of the 19th, on which the eclipse of the sun took place, to a similar investigation by myself.

It will not be necessary very minutely to detail the observations of the 18th, which, as well as those during the eclipse, were made with the Six's thermometer which I constantly employ. At six, a. m. the thermometer stood at 40°, the sky being overcast with *Cirrostratus* clouds pretty close and dense, and a steady breeze blowing from S. S. W. At $6^h\ 45^m$, and for half an hour after, the temp. did not exceed 41°. At $7^h\ 45^m$, the sun being up, it was 42°; and from this time to $11^h$ it advanced (with some interruption, but no depression, intervening) to 47°. In the interval before noon occurred a depression of *half a degree*, which being over, the temperature attained its maximum for the day, of 47·5°, at half past

twelve. During the time there fell about 0·07 inch of rain. The afternoon was fair, the temperature descended more rapidly than it had risen, and in the fore part of the night (as it appears) touched upon 32°.

A.M. *Eleventh Month,* 19. *Day of the Eclipse.*

| *h.* | *m.* | *Therm.* | |
|---|---|---|---|
| 6 | 30 | — 35° | Temp. going down, having risen some degrees in the night: dawn of day perceptible: light *Cirrostratus* clouds, with a gentle breeze at S.W. Barom. 29·68 in. |
| 7 | 0 | — 34·5 | Day breaks. Bar. 29·69 in. |
| 7 | 20 | — 34 | Breeze increasing: a veil of clouds above, passing off with a definite edge to N. E., but leaving detached streaks below. |
| 7 | 42 | — 33 | Barom. 29·72 in. Sun not yet visible, being hid by a low mist. |
| 8 | 0 | — 34 | The sun has been some time shining; but is now among thin streaks of cloud. |
| 8 | 20 | — 35 | The sun among streaks of cloud, the disk scarcely visible; so that the commencement of the eclipse was not observed. |
| 8 | 45 | — 36 | The sun still behind a light skreen of *Cirrostratus*, through the interstices of which the disk is at times distinctly seen eclipsed. The therm. now ceases to rise. |
| 8 | 55 | — | The temperature now begins to decline, although there is less cloud than heretofore. |
| 9 | 0 | — 35·5 | Barom. 29·75 in.: but hesitating, as if it would fall. |
| | 15 | — 35 | A somewhat thicker bed of cloud now coming over the sun increases the obscurity of the eclipse, which is yet not very considerable. There is perhaps as much *light* as when the sun was 20 min. high. |
| 9 | 20 | — | The thermometer now tends to rise again. |
| 9 | 30 | — 35·5 | Barom. 29·77 in. Cloudiness, by *Cirrostratus*, becoming general. |
| 9 | 45 | — 37 | Eclipse visibly going off. |
| 10 | 0 | — 38 | |
| 10 | 15 | — 39 | The clouds again lighter. |
| 10 | 30 | — 40 | Near the termination of the eclipse. |
| 10 | 50 | — 41 | Barom. stationary: wind more to the W.: the clouds thicken again. |
| 11 | 5 | — 42 | |
| 11 | 40 | — 43 | The clouds tend to form *Cumulostratus.* |
| P. M. | | | |
| 12 | 20 | — 45 | Barom. rising. |
| 1 | 30 | — 45 | In this interval the temp. has fallen half a degree, and risen again: as it did yesterday an hour earlier. |
| 2 | 0 | — 46 | Temp. the same at this hour yesterday: cloudy. |
| 3 | 0 | — 46 | Idem. |
| 4 | 0 | — 44 | Barom. 29·80. No rain nor strong wind has occurred during the observations. |

The foregoing observations, I presume, will be found satisfactory. The temperature on this day was falling (as is very commonly the case) before sun-rise; presently after which it began to rise. This effect continued until a considerable portion of the sun's rays became intercepted, when it fell again, to near the middle of the eclipse (by my watch, which had not been adjusted), and, in proportion as the latter went off, resumed its former movement, rose steadily, and attained its maximum at nearly the same degree as the day before, though later in the afternoon. Had the elevation proceeded from $8^h\ 45^m$ to $9^h\ 15^m$, at the rate which it had assumed previous to this interval, the temp. at $9^h\ 15^m$, instead of 35°, would have been 38°, and the progress of the diurnal elevation would

have been still more uniform than the day before; which was to be expected from the greater uniformity of the sky. Now as the depression coincides sufficiently with the time of the sun's being under eclipse, and as no other disturbing cause is apparent, we may conclude that there resulted from this cause an interruption to the diurnal accumulation of heat at the place of observation, the amount of which, at six or eight feet from the surface, was equal to 3° of Fahrenheit's thermometer.

*Tottenham, Eleventh Month,* 20, 1816. L. HOWARD.

## VI. *State of the Wheat in the Lothians.*

(To Dr. Thomson.)

MY DEAR SIR, *Manse of Cockpen, Nov.* 11, 1816.

I was surprised, and I must say not quite pleased, to see your extract from my letter of Sept. 20 last, in the *Annals* for this month. I certainly never intended any part of that letter to be published, and thought you could not but perceive it to be entirely of a private nature, from the manner in which it was written. However, since you judged the experiment of Mr. W. and myself to ascertain the quantity of the wheat worth printing, no matter: only I could wish you had given it in a less conspicuous form. The experiment was correctly made, and correctly stated, and I think the principle just; but, then, it was not extended far enough to support the conclusion supposed to be drawn. It would have required to be made on a great variety of farms to show the *state of the wheat in the county of Edinburgh.* All it can be properly said to do is to show the state of the wheat on a single farm. I am afraid, however, the character of the grain thus ascertained may be but too applicable to the county in general, and even to the neighbouring counties, though I have reason to think that all the wheat is not so bad. The same sort of experiment was made by a farmer of my acquaintance in East Lothian; and if my memory does not much fail me, the general result was about one third unsound.

Another farmer whom I the other day met from that county, informs me that the wheat there turns out to afford no more than four bolls per acre, and to weigh 14 st. per boll, in as far as he has yet heard of any trials being made. Eight or nine bolls are an ordinary crop, and 16 st. per boll. Mr. W. tells me his wheat yields only three bolls per acre, and weighs 13 st. 7 lb.; but he says others tell him they have more.

A farmer in this parish told me on Thursday last that he had all his grain now cut down, but not a sheaf in, except wheat, and not the whole of that. All his oats and barley still in the field. The peas and beans crop, it is understood (particularly the former), will scarcely supply seed for next year. I have just seen a friend from Falkirk, who tells me that a gentleman of his acquaintance there has threshed out some wheat, and has seven bolls to the acre.

Where I write (on the banks of the South Esk) may be about 250 or 300 feet above the level of the Firth of Forth, and within

sight of it; and in my immediate neighbourhood all the grain is now cut; but within sight of this place, on what is called the Camp Hill, to the southward, about 100 or 150 feet higher, there is perhaps nearly one half still to cut, and much yet very green. On the night of Thursday the 7th, and morning of Friday the 8th, the thermometer here at eight o'clock stood at 26°, and on Friday evening the ground was completely covered with snow, which, however, soon disappeared on the low grounds; but at this moment it still lies in my view on the Camp Hill among the standing or uncut corn. The Pentland and Moorfoot hills are covered with snow. The thermometer here has been at $26\frac{1}{2}°$ or 27°, on Saturday and yesternight, so that we apprehend few of the potatoes which are still untaken up (and half of them probably are in this situation) have escaped.

I am, my dear Sir,

Very respectfully and truly, yours ever,

James Grierson.

VII. *Correction of a Mistake in the Translation of Berzelius's Paper on the old and new Theories respecting the Nature of Muriatic Acid.*

I have received a letter from Professor Berzelius complaining that the following passage in the translation, vol. viii. p. 209, misrepresents his meaning:—

"This I call attempting to decide the question by superiority of reputation. But while I acknowledge superiority with respect, I shall never cease to oppose it with the force of scientific arguments." The original is as follows:—

"Das nenne ich etwas durch *ueberlegenheit* beweisen wollen. Während ich indess die *ueberlegenheit* mit ehrerbietung anerkenne, werde ich doch nie aufhören, ihr die kraft eines wissenschaft lichen beweises streitig zu machen." The literal translation of which is as follows:—

"This I call wishing to prove a thing by *authority*. But while I acknowledge the *authority* with respect, I must firmly dispute its force as a scientific proof."

VIII. *Insanity.*

Dr. Spurzheim, the author of Physiological System, &c. is preparing for the press an extensive work on Insanity, entitled, 'Pathology of Animal Life, or the diseased State of the Manifestations of the Mind, termed Insanity.' The author, after devoting many years of indefatigable labour in investigating the various kinds of insanity, and comparing them with the particular organization and circumstances of the individuals, has at length resolved to submit his labours to the public at a period when the investigation before a Committee of the House of Commons has excited the attention of the public to the sufferings of the insane poor throughout Great Britain; with the hope of throwing some new light on this obscure class of diseases.

IX. *Results of a Meteorological Register kept at New Malton, in the North Riding of Yorkshire, in Sept.* 1816. By Mr. James Stockton.

Mean pressure of barometer, 29·645. Max. 30·15. Min. 28·63. Range, 1·52.—Temperature, 52·886°. Max. 73°. Min. 34°. Range, 39°.—Rain, 2·48 inches. Wet days, 13. Stormy, 4. Winds variable.

The wet weather, which continued with little variation during nearly the whole of the two preceding months, introduced the present; and, with the exception of a few fine days, the character of the period may be regarded as wet, and unfavourable to the harvest. The pressure and temperature have been in constant fluctuation, and a very sudden depression took place in each on the 29th, which was stormy and wet; the loss in the barometrical column in about two days being the range for the month; but during the night nearly an inch was regained. The *Cirrus*, *Cirrostratus*, and *Cumulostratus*, have been frequent, and a most splendid display of the first modification was observed on the 25th, which soon became *Cirrostratus*, and finally *Cumulostratus*, succeeded by a storm of wind and rain at night. Solar and lunar halos on the 1st, 3d, 28th, and 29th, followed, as usual, by wind and rain.

*New Malton, Oct.* 3, 1816. JAMES STOCKTON.

X. *Letter from J. Murray, M.D. F.R.S. Edin. Lecturer on Chemistry in Edinburgh.*

(To Dr. Thomson.)

SIR,

I have reason to believe that in some late numbers of yours, and of other philosophical journals, observations have been ascribed to me which do not belong to me. The ambiguity has, I believe, arisen from the circumstance of the name and surname of another correspondent being the same as mine. I will thank you merely to notice the distinction.

I am, Sir, your obedient servant,

*Edinburgh, Oct.* 21, 1816. J. MURRAY.

XI. *Observations upon the reduction of barytes to the metallic state, by means of a gaseous blow-pipe employed for this purpose, by the Professor of Mineralogy at Cambridge.*—In a letter to the Editor, by the Rev. J. Holme, Fellow of St. Peter's College, Cambridge.

SIR, *St. Peter's College, Cambridge, Nov.* 20, 1816.

Having been present at most of the experiments, and witnessed the results, which professor Clarke has announced to the chemical world by means of your *Annals of Philosophy* and the Quarterly Journal of the Royal Institution, it gave me real satisfaction to find that your confirmation of his discoveries coincided with my own testimony. The most remarkable of those results is certainly

that of pure *metallic* globules obtained in the decomposition of the *nitrate of barytes.* To this, as I was repeatedly an eye-witness, I can speak with the most decided certainty; and, as I have been at the pains to examine with the greatest care the materials he made use of in the preparation of the salt whence these *metallic* globules were obtained, I can also as decidedly state, that there has been nothing in the whole experiment likely to involve it in doubt, or to render it liable to dispute. No metallic substance whatever was brought into contact with the *barytic* salt, it was simply placed within a cavity which he had scooped into a stick of charcoal; and being in a deliquescent state, neither oil nor water was requisite to form it into a paste. It became so liquid upon exposure to the inflamed gas, and its ebullition was so violent, that he more than once wished to desist from any further attempt at its reduction; the appearance being so different from that which had been exhibited in the fusion and reduction of the pure *earth* to the *metallic* state. I with some difficulty prevailed upon him to continue the experiment, and, at length, having examined the *charcoal,* I pointed out to him the brilliant globules of *metal* with which it was studded, and which, for the first time in my life, I had the satisfaction to view. Upon repeating the experiment, we observed the appearance he has described of the separation of the *metallic* globules in the midst of the boiling fluid, easily discernible by persons accustomed to watch the separation of a metal from its ore in experiments with fluxes, by the common blow-pipe. Upon a third or fourth trial, I succeeded in detaching two of those minute globules from the fused mass, and in placing them in *naptha.* They retained their brilliancy, however, but for a few days. The smaller of the two globules disappeared, as I am convinced, before the phial could be sent to you in which they had been contained; and the larger globule having been a month in *naptha* before you had an opportunity of examining its appearance, had lost, as you informed us, its brilliant *metallic* lustre. It still, however, remains in the *naptha,* and by its rapid change of place, when rolling about in the bottom of the vessel, it is evident that, for so minute a globule, its weight as a *metal* cannot be so inconsiderable as we at first believed the *metal* of *barytes* to be.

I shall not enter upon the subject of the other experiments which have been detailed by Professor Clarke, except by adding my testimony to the truth of the results, and I forbear to do this, for the reason which has been assigned by your correspondent Mr. George Oswald Sym; because "I do not wish to intrude upon the path of another." His reduction of *strontian,* as of other *metallic* substances, especially the *semi-metals,* has depended upon the lustre exhibited by those bodies when rased by filing; and I am disposed to agree with him in maintaining as an axiom, that "no substance ever yet was known to disclose a *pseudo-metallic* lustre to the action of the file.

I remain, Sir, very faithfully yours,

J. Holme.

Pl. LXXI

Scale of the BAROMETER and THERMOMETER at Plymouth July to December 1816.

(Place of Observation 112 feet above the level of the Sea)


Engraved for Dr Thomsons Annals for Baldwin, Cradock & Joy, Paternoster Row. Pub.d 1 1817

## Article XII.

# METEOROLOGICAL TABLE.

| 1816. | Wind. | Barometer. Max. | Min. | Med. | Thermometer. Max. | Min. | Med. | Hygr. at 9 a. m. | Rain. | |
|---|---|---|---|---|---|---|---|---|---|---|
| 10th Mo. | | | | | | | | | | |
| Oct. 14 | S E | 30·13 | 30·03 | 30·080 | 60 | 38 | 49·0 | 82 | | ☾ |
| 15 | S E | 30·13 | 29·99 | 30·060 | 57 | 41 | 49·0 | 90 | | |
| 16 | S E | 29·99 | 29·71 | 29·800 | 58 | 34 | 46·0 | 93 | — | |
| 17 | Var. | 29·72 | 29·69 | 29·705 | 52 | 35 | 43·5 | 77 | ·12 | |
| 18 | N W | 29·85 | 29·72 | 29·885 | 53 | 30 | 41·5 | 78 | | |
| 19 | N W | 29·85 | 29·56 | 29·705 | 56 | 41 | 48·5 | 60 | ·17 | |
| 20 | W | 29·58 | 29·52 | 29·550 | 50 | 39 | 44·5 | 55 | | ● |
| 21 | Var. | 29·63 | 29·61 | 29·620 | 49 | 40 | 44·5 | 86 | — | |
| 22 | N W | 29·89 | 29·63 | 29·760 | 54 | 30 | 42·0 | 80 | ·5 | |
| 23 | Var. | 29·92 | 29·74 | 29·830 | 46 | 30 | 38·0 | 65 | | |
| 24 | S | 29·74 | 29·41 | 29·575 | 52 | 34 | 43·0 | 95 | ·10 | |
| 25 | S E | 29·53 | 29·30 | 29·415 | 54 | 29 | 41·5 | 95 | ·44 | |
| 26 | S E | 29·62 | 29·59 | 29·605 | 53 | 37 | 45·0 | 67 | | |
| 27 | E | 29·59 | 29·57 | 29·580 | 58 | 46 | 52·0 | 72 | | ☽ |
| 28 | E | 29·55 | 29·51 | 29·530 | 58 | 38 | 48·0 | 88 | | |
| 29 | S E | 29·51 | 29·15 | 29·330 | 56 | 42 | 49·0 | 90 | — | |
| 30 | S E | 29·16 | 29·09 | 29·125 | 57 | 42 | 49·5 | 83 | ·37 | |
| 31 | S | 29·25 | 29·16 | 29·205 | 55 | 39 | 47·0 | 95 | ·19 | |
| 11th Mo. | | | | | | | | | | |
| Nov. 1 | Var. | 29·32 | 29·25 | 29·285 | 53 | 32 | 42·5 | 94 | — | |
| 2 | S | 29·22 | 29·17 | 29·195 | 47 | 35 | 41·0 | 85 | ·81 | |
| 3 | S W | 29·63 | 29·22 | 29·425 | 48 | 36 | 42·0 | 86 | ·11 | |
| 4 | N E | 29·63 | 29·62 | 29·625 | 51 | 44 | 47·5 | 85 | ·19 | |
| 5 | S E | 29·62 | 29·36 | 29·490 | 53 | 40 | 46·5 | 90 | ·11 | ○ |
| 6 | Var. | 29·33 | 29·29 | 29·310 | 50 | 36 | 43·0 | | — | |
| 7 | N | 29·45 | 29·29 | 29·370 | 41 | 20 | 30·5 | 90 | — | |
| 8 | S W | 29·45 | 28·87 | 29·160 | 43 | 24 | 33·5 | 83 | ·34 | |
| 9 | S W | 29·30 | 28·72 | 29·010 | 47 | 34 | 40·5 | 66 | — | |
| 10 | N W | 29·70 | 29·30 | 29·500 | 34 | 20 | 27·0 | 73 | — | |
| 11 | Var. | 29·23 | 29·03 | 29·130 | 44 | 26 | 35·0 | 69 | ·54 | |
| | | 30·13 | 28·72 | 29·512 | 60 | 20 | 43·12 | 81 | 3·54 | |

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.*—14. Much dew these two mornings past: Gossamer. 15. Dew: somewhat misty air, with an electric odour: sunshine. 16. Misty: the trees dripping: calm: then sunshine, with a breeze at S.E., and *Cumuli*, &c. 17. Morning overcast: rain by nine: rainbow: drips at intervals: fair. 18. Fair: a fine breeze through the day: twilight pale orange, with *Cirrostratus.* 19. Wind and rain in the night. 20. Cloudy day. 21. Obscurity to the S., indicating rain there: after dark, a flash of lightning to the S.E., with a small meteor. 22, a.m. Rain, succeeded by a fair day and night. 23. Hoar frost: clear, a.m.: fine day. 24. Cloudy: windy at S.: wet evening: clear night. 25. Cloudy: windy at S.E.: very wet, p.m. 26. Misty morning: fair day. 27. Various clouds: fair. 28. Overcast with *Cirrostratus:* clear night. 29. Very heavy dew: misty at night, with a *Nimbus* to the S.W. 30. Rainy. 31. Misty: close, electric air: rain, p.m. and evening.

*Eleventh Month.*—1. Misty, a.m.: *Cumulostratus, Nimbus:* a little rain. 2. Misty morning: much rain this day and night: hail. 3. Misty, a.m.: fair day: wet evening. 5. *Cumulostratus:* small rain. 6. *Nimbi*, with other clouds. 7. Snow this morning in the high lands: shower, with hail: snow again at night. 8. Very white frost: much smoke and cloud accumulated over the city: cloudy evening: the *Cirrostrati* appeared convergent to the rising moon: in the night the temp., which had not passed 33° in the day, advanced to 45°, with much wind and rain from the southward. 9. Fair morning: squally, p.m.: after dark a small meteor and lightning to the S., in which direction *Nimbi* were visible at sun-set. 10. Snowy morning: clear day: the hygrom. receded to 45°. 11. Clear, a.m.: wind N.W.: the ground crusted over with some snow which fell last night: a bank of clouds in the S.W., and some attenuated *Cirrostrati* aloft: at night rain: the wind violent at S.W.

## RESULTS.

Prevailing winds S.E. and N.W.

| | | |
|---|---|---|
| Barometer: | Greatest height | 30·13 inches. |
| | Least | 28·72 |
| | Mean of the period | 29·512 |
| Thermometer: | Greatest height | 60° |
| | Least | 20 |
| | Mean of the period | 43·12 |
| Mean of the Hygrometer at 9 a.m. | | 81° |
| Rain | | 3·54 inches. |

The barometer has been throughout unsteady, and its oscillations towards the end considerable, chiefly in depression. Eight days only were without rain.

Tottenham, *Eleventh Month*, 18, 1816. L. HOWARD.

# INDEX.

D.



E.



F.



G.



H.

I.



K.



L.



M.

## ERRATA.

Page 212, line 3, *for* the knife fixed the cup, *read* the knife fixed above the cup.
—— 357, —— 1, *for* Fig. 4, *read* Fig. 6.

END OF VOL. VIII.

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