THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,
M.R.I.A. M.R.A.S. MUNICH, F.S.A. EDIN. AND PERTH, &c.

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

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PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *On Flax-steeping, and its Effects on the Colour and Quality of the Flax.* By Gavin Inglis, Esq.

To Mr. Tilloch.

Dear Sir,—If you consider the following observations on flax-steeping worthy of a place in your valuable Magazine, I will thank you to insert them. They are the substance of answers furnished by me to inquiries made upon that subject by G. Thomson, Esq. of the Trustees Office, Edinburgh.

When in Dumbartonshire in 1801, reducing to practice the process of bleaching by steam, I had a few spindles of yarn given me to prepare for weaving. There was in the *sleekness* of the thread something that attracted my attention. Having soaked it over-night in warm water to prepare it for steaming, I was much surprised at the change of colour, and the quantity of colouring matter dissolved in the water. It was then washed, wrung, and soaked in a weak alkaline ley, and laid for steaming over some brown linens. After steaming the usual time, the covers were taken off. The yarn was found to have attained a degree of whiteness I never had before observed under similar circumstances. It was washed in the stream so long as any colouring matter came from it, and laid to the grass for two days. I remember well the colour was such as to impress me with a strong belief that some great and important discovery might be the result of accurately following up the process this flax had gone through; and I immediately made inquiry of the lady to whom the yarn belonged, who informed me she had it from a person she named, in the neighbourhood: to this individual I made the same application, and traced the yarn to have been purchased at a Kilmarnock fair.

Here the matter rested till the next season of lint pulling. I had a particular wish to trace if possible the matter to its source, and conceived the best plan would be to traverse that part of the country, from Stirling towards Kilmarnock. My time was far too limited: but I saw as much as to satisfy myself that the secret with regard to the bleaching, lay entirely in pulling the flax before it was *too* ripe; and I also found that this great advantage might again be lost by improper watering.

I saw the flax in all its stages, from the pulling to the drying after watering; and upon inquiry I uniformly found the greenest pulled was intended for the finest purposes, and that the whitest flax, after drying, had been watered in the *burn*. They were very particular in watering, and did not allow it to remain so long in the water as I had been led to believe necessary, from the practice here; nor did they spread it on the grass after watering, as is the mode in this quarter, but dried it all from the water, by what is termed *hutting*.

As bleaching alone was my object, my inquiries respecting the different shades of colour after watering were very particular; and I uniformly found that the white flax had been watered in the burn, and the dark-coloured in ponds dug where water could be most conveniently obtained. When I mention a burn, it must be understood to be a stream so small as to require a dam being necessary to receive the water into a temporary pond to cover the flax.

The succession of clean water, I conceive, prevents the deposition of colouring principles, to be hereafter mentioned, by washing or carrying them away, after being extracted from the flax, which I had afterwards an opportunity of proving, in a pond so constructed, which produced remarkably white flax, while the same flax, from several stagnant ponds dug in the same ground, filled with water from the same spring, was very dark in the colour.

In following up these observations, my situation in life did not then admit of experiments to the extent the importance of the subject would have required. I shall, however, narrate these, so far as they extended. The result satisfied me, that the watering of flax must vary with local circumstances, and every where depend on the means afforded by springs, streams, moss, or marsh, that may be in the neighbourhood of the flax-field, so long as the present mode of culture is followed; and the colour of the flax after watering very much depend on the following causes:

The ripeness of the flax before pulling.

The state of putridity of the stagnant water.

The minerals the water may contain.

Whether it is steeped in a pond dug, or one formed by damming a small stream or rill. Or, if a succession of parcels of flax (which is sometimes the case) be watered in the same pond, where every succeeding parcel must partake of the contaminating *dye* produced by the fermentation of the former.

In the course of my observations, I found the quantity and solubility

solubility of the colouring matter in proportion to the degree of ripeness; and in the ripest, on a principle I never till then knew to have an existence in flax, viz. iron, which may be said to abound in ripe flax.

In unripe flax I found the colouring matter soluble in water; but this matter became less and less soluble, till the water made little or no impression upon it. The time necessary for flax to macerate must in some measure depend on the weather, but more on the state of ripeness than most practitioners seem to be aware of.

In unripe flax the juices of the plant are in a mucilaginous state; hence its solubility in water. If flax is watered in an unripe state, the mucilage, from its solubility, tends greatly to facilitate the process of watering, by promoting the fermentation. But if the flax is allowed to stand on the ground till it has attained a rusty-brown colour, and the seed fully ripened, the juices of the plant are then changed from mucilage to resinous matter, and certainly no longer soluble in water, so far as the resin is concerned, unless assisted by solvents.

In this stage, instead of having a large portion of mucilage to expedite the fermentation, the resin defends the flax for a time against the effects of the water, and the fermentation must proceed by slow degrees; consequently the time necessary to steep flax must vary according to the ripe or unripe state of the flax when pulled. What would sufficiently water unripe flax, would hardly penetrate the outer rind of the ripe; and the time required for the ripe would entirely destroy the other.

The choice (where a choice can be made) of the water, and the ground into which ponds are to be dug, or the rill or stream into which the flax is to be laid, is certainly of the highest importance, for the colour, quantity and quality of the flax.

That very great improvements may be made in the mode of separating the flax from the rind and boon, so as to render that process less offensive, far safer, and equally effectual, I have no doubt whatever. But before promulgating any speculative theory on a subject of such importance to the nation, would it not be laudable in the Honourable Board of Trustees to cause a full series of experiments on a fair scale, to be made and followed up by some persons of skill and observation, which would set the matter at rest, solve all doubts on so important a process, and furnish the farmer and flax-grower with such instructions that he could not err.

The presence of iron in the plant was discovered in my attempts to bleach flax, by different modes, to ascertain whether there existed any other principle beside mucilage, resin and oil, in what stage the juices became insoluble in water, and to

 what

what extent these substances existed, with a view to ascertain the safest strength of alkaline applications to be used in the different processes of bleaching. Alkalies are the common solvents used by bleachers; but I did not conceive them altogether adapted to my present purpose. I took alcohol, and succeeded in bleaching to a very beautiful whiteness flax in its unripe state and in its early stages; but as the flax ripened, its power lessened. I exposed full ripe flax to the action of alcohol, both in a liquid state and in the state of vapour, till I satisfied myself of having extracted all the resinous matter;—still a colour remained. I subjected it to the action of an oxymuriate, and was astonished to see the presence of iron so strongly indicated. I took another quantity of this full ripe flax, and boiled it in a ley of prussiate of potash, prepared by calcination of common potash with green whins: from this it was washed, and immersed in oxymuriate of lime, which produced a beautiful light blue. This experiment I repeated till I produced, by apparently the same process, on the unripe flax a beautiful white, and on the full ripe a fine, full, Prussian blue. This explained in a most satisfactory manner many of the phænomena of bleaching I never before could comprehend, and appeared to me a most wonderful work in nature,—the formation of a metal in the juices of a plant, whose existence was not detected, by the same means, in the same plant, only fourteen to twenty days younger than where its presence became so manifest.

Tan also exists in flax, and is very soluble in water.

In steeping flax, the water in the pond becomes impregnated with tan. The process of fermentation comes on, in the progress of which the iron is acted upon. The iron and tan combine, precipitate, and form an almost indestructible dye.

Thus, by inattention to the steeping of flax, the labour and expense of bleaching are greatly increased. The linen loses much of its strength and durability by the necessary process of bleaching, and destroying a colour which, by due care, might be prevented from ever fixing itself. With esteem, I remain,

Dear sir, yours sincerely,

GAVIN INGLIS.

Strathendry Bleachfield,
Dec. 10, 1817.

II. *Supplement to Mr.* BENWELL'S *Paper on the Value of increasing Life Annuities, in our Number for September* 1817.

To Mr. Tilloch.

SIR,—FROM the circumstance of having changed the original form of the theorems for determining the values of increasing life

life annuities (inserted in number 233, for September) it has happened that the symbolic characters denoting the operations to be performed upon the respective quantities which compose them, have inadvertently been misplaced, and in one instance entirely omitted. As a consequence thereof, the theorems are liable to be misinterpreted, and their practical solutions at variance with the conditions and process of investigation upon which they have been established. These corrections, although apparently minute, but really essential, will first engage my notice; when I also beg to avail myself of the opportunity to offer some additional theorems applicable to the purpose above, and which will appear to be much more commodious, and better adapted to computation than those which they properly represent; and so far as regards this object, the only difference between them is, that the latter are not dependent for their numerical results on the tables of single and joint lives, being derived from the celebrated hypothesis of M. De Moivre, as the present ones similarly are by its aid from the real probabilities of life, and which consequently will applyto any tables of observations whatever. It therefore follows as a distinction, that in all cases of real practice the formulæ now inserted should be employed in preference to those alluded to.—I now proceed to the corrections.

In the first theorem the sign (should be put before $6(a+1)$ in order to connect it with the following quantity, the signification being that the whole thus included within the parenthesis is to be incorporated with the expression $\overline{x}^{-a}$.

The second requires no other observation than its being correctly printed.

In the third theorem the sign — is omitted; it ought to be put before the factor $\frac{6+2.x\quad 1}{x-1}$, thus keeping distinct by its proper index of operation all the quantities forming the expression to the right from those composing that on the left.

In the fourth and last the sign (should be removed from before the 6 and set on the left of the factor $\frac{1}{x-1}$, since the two sets of quantities connected thereby are also to be combined with the expression $\overline{x}^{-a}$.

And further note, The general term in the pentagonal series Y should have $-n$, and not -1, as printed.

With respect to the supplementary part of my design:

Put A.AA for the value of an annuity of £1 on the given single, and two equal joint lives. AA′.A′ the values of the same lives each one year older.

Now

Now let the theorems in the paper referred to, as they occur in order, be severally denoted by P. Q. R. S. $\left(\frac{a'}{a}.x^{-1}.\right)$ the expectation of the given life receiving £1 a year hence; *c*. *c'* the complements of the same, and of a life one year older. Then after effecting the necessary substitutions, and by reduction, the resulting equations may be represented as follows:

$$P = \frac{cA + B - D + c - 1. + c^2.(A - AA) - \frac{2x-2}{x-1.}\left(B - \frac{c^2+c}{2}.x^{-c}\right)}{c(x-1)}.$$

B being the value of an annuity 1. 2. 3. 4. for the term certain equal to *c*, and D that of £1 for like term.

$$Q = c(A - AA) + c'.(A' - AA').\frac{a'}{a.}x^{-1}.$$

Now if P' denote the value as found by the first formula for a life 1 year older than A, we shall obtain

$$R = \frac{aA + (P'a')8x^{-1}}{a}.$$

$$\text{And, } S = \frac{aP + P'.a'x^{-1}}{a}.$$

Any given sum being proposed to be advanced in lieu of an equivalent increasing life annuity, one general instance of the utility of these formulæ then would be, to assign what the ratio of the series should be, because allied in principle to that which determines the value of the annuity sought; and I conceive there is a numerous class of individuals in the speculative order of society, and of others having no prospective views as to the future disposition of their property, but who, prudently contemplating a time of adversity happening, would be induced to stipulate for the grant of an annuity beginning with some small annual payment, and increasing upwards, so that it is rendered an object the more inviting as the duration of life is prolonged. These persons, it may be imagined, would infer a peculiar policy in this mode of assurance, independent of the pleasing reflections it must inspire at a season (as we perceive but too commonly evidenced) when human nature hath to recount the sublunary ills of an eventful and chequered life. I aim not at objects merely speculative and curious, but practically advantageous.

Yet I am sensible of an existing cause that tends to counteract a spirit of stimulus for life annuity business in a quarter where most naturally it would be expected, arising from the tables at this time so indiscriminately resorted to being very defective and ill-suited thereto; and indeed its objects, at first confined to the

chartered

chartered companies, we have seen usurped and persevered in to an improvident extent. Hence it is these instruments of analysis may remain dormant as speculative truths, till, by the accession of more copious and efficient auxiliaries, the practical may be said to keep pace with the mathematical means.

Having been prevented by particular circumstances from paying earlier attention to this matter, if you will have the goodness to allot it a place in the next number of the Philosophical Magazine, it will confer an obligation upon, Sir,

Your obliged and humble servant,

Haberdashers' Place, Hoxton, December 12, 1817.

JAS. BENJ. BENWELL.

III. *Aëronautics applied to Meteorology.* By T. FORSTER, *Esq.*

To Mr. Tilloch.

SIR, — I HAD for a long time suspected, from the direction of the flying clouds, that the currents of air which occupied the higher regions of the atmosphere came down afterwards, and blew over the earth's surface in the same direction as they had previously blown above. To ascertain this fact, I observed attentively the various directions of small air-balloons made and sent up by my brother: Out of more than twenty experiments, I have selected the following as confirming this fact:—they were made in different years, and in different times of the year.

In October 1809, a gas-balloon three feet seven inches diameter, on ascending, first moved with an E. wind; at the height of (*about*) 500 feet it got into a NN.W. current; and lastly, at a much greater altitude got into a strong gale from SW., which carried it into Cambridgeshire. The succesive changes of the wind next the earth as indicated by the weathercocks were E., NNW., and SW.

I have minutes of nine experiments made during the two consequent years (1810 and 1811), in which each balloon got three currents, whose directions became successively the directions of the currents next the earth within the space of thirty-six hours.

In four other experiments made in the same years, two of the balloons went uniformly in one direction, and the wind remained steady for several days. The other two experiments failed; that is to say, the currents indicated by them did not come down, or else they came in the night-time, and were unobserved. Of late years, not being stationary in one place, I have not made so many experiments; and I have only accurately observed four balloons made by my brother, whereof three got four, and the other got two currents of air: out of these only two were followed

lowed by successive currents next the earth in the same direction.

Yesterday he launched from Clapton a middle-sized inflammable gas-balloon, at three o'clock P.M., the smoke and weathercocks indicating a W. wind. The balloon consequently went in an E. direction ; but at the height of what on a rough guess might seem to be above five or six thousand feet, it got into a NW. gale, and seemed carried toward SE. At half after four the smoke from the chimneys indicated the same wind, though so gentle as hardly to be perceived, and which did not move the common weather-vanes.

The last experiment confirms also an observation which I have before made, by means of the movement of the higher clouds; namely, that when the thermometer is below the freezing point with a southerly wind, there is then a northerly wind blowing above it.—We have offered a small reward for the balloon, and shall be obliged to any person who may communicate where it fell, that we may ascertain its ultimate direction.

I hope to communicate in future more accurate details of aëronautic experiments on wind; and I merely communicate the above to excite persons in different places to make corresponding observations. The small balloons are easily made of varnished paper; they are preferable to those sent up with rarefied air, as they ascend higher and keep up longer. But the rarefied air-balloons are capable, when made large enough, of indicating several currents of air.

Besides the above experiments of which I have minutes, my brother has sent up a great many balloons, and has almost always observed them moved by two or three currents: a circumstance which shows how little these machines (notwithstanding the sanguine assertions of some French writers) can ever be depended on as instruments to convey intelligence to armies where the ordinary means of communication may have been intercepted by the enemy. I am, sir, yours, &c.

Walthamstow, Dec. 12, 1817. T. Forster.

IV. *An Analysis of Sea-water ; with Observations on the Analysis of Salt-brines.* By John Murray, *M.D. F.R.S.E.**

The composition of sea-water has been variously stated by different chemists, not only with regard to the proportions of the salts which it holds in solution, but with regard even to the ingredients themselves.

* From the Transactions of the Royal Society of Edinburgh for 1816.

According

According to Lavoisier, it contains muriate of soda, muriate of magnesia, and muriate of lime, sulphate of soda, sulphate of magnesia, sulphate and carbonate of lime. The proportions he assigns are, in a pound of water, (French weights) 126 grains of muriate of soda, 14¼ grains muriate of magnesia, 23 grains muriate of lime mixed with muriate of magnesia, 7 grains of sulphate of soda and sulphate of magnesia, and 8 grains of sulphate and carbonate of lime*.

Bergman gives a very different statement. He found only muriate of soda, muriate of magnesia, and sulphate of lime; the proportions in a Swedish kanne, which is equal to about 6½ English pints, are 2 ounces 433 grains of muriate of soda, 380 grains of muriate of magnesia, and 45 grains of sulphate of lime †. Reducing them to English weights, they are equal, in a pint of water, to, muriate of soda 241 grains, muriate of magnesia 65·5, sulphate of lime 8. This, however, is with regard to water from the Canaries, containing 1 part of saline matter in about 23¼ of water. Reducing it to the proportion of the water of our shores, that of about 1 to 30, the proportions will be, muriate of soda 186·5, muriate of magnesia 51, sulphate of lime 6=243·5 grains.

Bergman's analysis is evidently incorrect in the omission of sulphate of magnesia, which every other chemist has obtained, and which is known to be extracted even on a large scale. And, what is singular, this did not arise from his not being aware that it might be present. On the contrary, he made an experiment to discover it; and even now, in reviewing his method, it is not apparent how he had been deceived. He evaporated to dryness, and treated the dry residuum with alcohol, by which he found muriate of magnesia to be dissolved; he then washed the residual matter, consisting chiefly of muriate of soda, with a small quantity of warm water, by which, as he remarked, if any sulphate of magnesia were present, it ought to have been dissolved. But this water showed no signs of the presence of this salt, either in taste or by precipitation, and contained nothing but a small portion of common salt. Now unquestionably, in this way, sulphate of magnesia ought to have been discovered; or if it should be supposed that it does not originally exist, but that sulphate of soda is the primary ingredient, still the method employed was equally proper to discover this latter salt. The only supposition that can be made is, that, in the first step of the analysis, a very weak alcohol had been used in large quantity, by which a portion of these sulphates would be dissolved, though still it is difficult to imagine that in this way they would be entirely abstracted.

* *Mémoires de l'Académie des Sciences*, 1772.

† Bergman's Essays, vol. i. p. 230.

Lavoisier's

Lavoisier's analysis has been considered as incorrect in two circumstances,—in the finding muriate of lime, and sulphate of soda. Neither of these has been discovered by other chemists; and in a late analysis of sea-water by Vogel and Lagrange, one of the objects of experiment was to detect their presence, and the conclusions drawn were, "that sea-water contains no sulphate of soda," and "no muriate of lime." In this analysis the saline ingredients found in sea-water were the same as those assigned by Bergman, with the addition of sulphate of magnesia. In 1000 grammes there were found 25·10 grammes of muriate of soda, 3·5 of muriate of magnesia, 5·78 of sulphate of magnesia, 0·20 of carbonate of lime and magnesia, and 0·15 of sulphate of lime*.

Some other recent analyses have been given; that by Lichtenbergh is noticed by Vogel and Lagrange, from a German journal, as approaching to their own; and that of Pfaff, in which, as in Lavoisier's analysis, there is found a portion of muriate of lime.

It is obvious, that there remains a degree of uncertainty with regard to the ingredients of sea-water, sufficient to give interest to a new analysis. The principle, too, which I have illustrated in a preceding paper, on the Analysis of Mineral Waters,—that the substances obtained are not always to be regarded as the original ingredients, but frequently as products of new combinations established by the analytic operations, may contribute to throw light on the conclusions to be drawn, and seemed to me to admit of being applied to the explanation of some of the preceding results. This led to the experiments of which I now propose to give an account.

The peculiarity in the results of Lavoisier's analysis, and with regard to which the others differ from him, is the obtaining, as ingredients of sea-water, portions of sulphate of soda and muriate of lime. Applying the principle now referred to, it is obvious that, in an analysis by evaporation, the composition of these salts would be subverted by their reciprocal action; neither of them would be obtained; but by mutual decomposition they would be converted into muriate of soda, and sulphate of lime. Sulphate of lime is accordingly obtained in all these analyses, and probably has this origin.

But, admitting this, how had muriate of lime, and sulphate of soda, been procured by Lavoisier? This, supposing the result accurate, can only be ascribed to some peculiarity in his process, by which their mutual action had been prevented, and their ditinct existence preserved. The method he employed was to

* Thomson's Annals, vol. iv. p. 200.

evaporate

evaporate sea-water to dryness; during the evaporation, sulphate and carbonate of lime were precipitated, and were withdrawn: the dry saline mass was lixiviated with alcohol; and the ley being poured off clear, was found to hold in solution muriate of magnesia and muriate of lime; the undissolved matter was then heated, with a mixture of two parts of alcohol and one of water, by which it was almost entirely dissolved; it deposited, however, on cooling, a white powder, which was found to be sulphate of soda, and sulphate of magnesia, and it retained dissolved the muriate of soda of the sea-water with a portion likewise of muriate of magnesia.

Now a portion of sulphate of lime was obtained in this process, which, according to the view I have stated, was probably produced by the mutual decomposition of sulphate of soda and muriate of lime. But it is also possible, that this decomposition might not be complete. I had formerly found, indeed, that when a liquor containing these two salts is evaporated, their decomposition is not entirely effected *; it seemed possible, therefore, that portions of both might remain undecomposed in Lavoisier's process; the alcohol applied to the solid matter would remove the muriate of lime, and thus the sulphate of soda would remain. To elucidate the whole subject, therefore, it seemed best to repeat Lavoisier's analysis as he had performed it, and ascertain the actual results.

A. Four pints of sea-water of the Frith of Forth, taken up near Leith, at a distance from any fresh water, were evaporated by the heat of a sand bath; the evaporation being continued until a pellicle of salt formed on the boiling liquor. A precipitate subsided during the boiling, which being washed, weighed when dry 25 grains.

B. The liquor was evaporated to dryness, and the saline mass was dried thoroughly by a continued heat of about 150°; it weighed 1025 grains. To separate the salts composing it, it was submitted to the action of alcohol. About 4 ounces of alcohol of the specific gravity of 840 were poured upon it in a bottle, and allowed to remain over it for 12 hours, being occasionally agitated; and when poured off an ounce of the same alcohol was added, and after frequent agitation, and being kept over it for some hours, was poured off, and added to the former.

C. The residuum, when dried, weighed 890 grains; 135 grains had therefore been abstracted, consisting chiefly of earthy muriates.

D. The saline matter was digested with 9 ounces of a weaker spirit, composed of 2 of alcohol and 1 of water, heat being applied to it by a sand-bath nearly to ebullition, with frequent

* Transactions, vol. vii. p. 475.

agitation;

agitation; and the liquor having been poured off while hot, 4 ounces more of the same diluted alcohol were added, heated as before, and after it had become clear by subsidence, this liquor was added to the other. The greater part of the saline mass, consisting chiefly of muriate of soda, was thus dissolved.

E. The residue was submitted to the action of successive quantities of a still weaker spirit, composed of 3 of alcohol, and 4 of water, aided by heat, with the view of dissolving the sulphate of magnesia and of soda. A solution was obtained of a strong saline taste.

F. To abstract these salts more completely, the residue was lixiviated with small successive portions of warm water; a solution having a similar taste was obtained.

G. There was left at length a powder, soft, light, tasteless, and insoluble.

It now remained to examine these products more minutely, to determine their nature, and estimate precisely their quantities.

The powder obtained in the first evaporation, A, consists, according to Lavoisier, of sulphate and carbonate of lime. It weighed when dry 25 grains; it was submitted to the action of a very dilute alcohol, acidulated with muriatic acid, which excited effervescence; this being poured off, and the residue being lixiviated, and dried, weighed 22 grains. It was *sulphate* of *lime*, and absorbed water with avidity, becoming solid and dry. The liquor poured off, afforded by evaporation a saline deliquescent matter, which, heated with sulphuric acid, gave products equivalent to 1·7 grain of *carbonate* of *magnesia*, and 1·2 grain of *carbonate* of *lime*.

The solution B, obtained by the action of the stronger alcohol, ought to have contained, according to the results of Lavoisier's analysis, muriate of magnesia and muriate of lime. A small portion of it was diluted with distilled water, and a few drops of a solution of oxalate of ammonia were added, but caused no precipitation, nor even any opacity. The liquor, therefore, contained no muriate of lime. It was distilled to dryness. The dry matter deliquesced on exposure to the air; being lixiviated with alcohol, a small portion of muriate of soda remained undissolved, which was added to the solution D. The liquor being evaporated so far as to be of an oily consistence, afforded, on cooling, *muriate* of *magnesia* in prisms. This, dried until it had no appearance of moisture, weighed 145 grains. Decomposed by sulphuric acid, it afforded 105·9 grains of dry sulphate of magnesia, equivalent to 83·5 of real muriate.

The solution D had a strong saline taste, and, in cooling, had deposited muriate of soda in cubes on the sides of the bottle. A little of it being diluted with distilled water, oxalate of ammonia

monia did not impair the transparency. Carbonate of potash, and muriate of barytes, produced a turbid appearance. The entire liquor was submitted to distillation, until the alcohol was abstracted, and was then evaporated in an open bason, until crystals formed in it while hot. These were cubes of *muriate* of *soda*, and this salt continued to be afforded by successive evaporations. The last product deliquesced a little on exposure to the air, indicating the presence of muriate of magnesia; and the remaining liquor afforded by evaporation a deliquescent saline mass: both these were washed with repeated portions of alcohol; muriate of magnesia was thus obtained, which dried, weighed 17·3 grains, and which, converted into sulphate, gave 12·4 grains, equivalent to 9·7 of real muriate. The matter not dissolved by the alcohol, being dissolved in water, afforded by slow evaporation sulphate of magnesia in prisms, which dried, weighed 6·3 grains. The crystallized muriate of soda, dried at a heat of 200°, weighed 580 grains.

The solution E deposited, on standing, after twelve hours, crystals in flat striated prisms, having every appearance of *sulphate* of *soda*, and which, on more minute examination, were found to be so: freed from sensible moisture, they weighed 18 grains. The liquor diluted with distilled water, was not sensibly affected by oxalate of ammonia; it became slightly turbid with subcarbonate of potash, and with muriate of barytes. The alcohol was drawn off by distillation; being then submitted to evaporation, a crust of muriate of soda formed on the surface, and crystals in cubes were deposited; additional portions of them were obtained by successive evaporations, and the liquor continued to afford a crust of muriate of soda on its surface, while hot, until it was almost entirely evaporated. A small portion of liquor remained, which, on cooling, afforded prismatic crystals of sulphate of magnesia, which, freed from moisture, weighed 8·9 grains. The muriate of soda dried, weighed 170·8 grains.

The first portions of the aqueous solution F had deposited crystals of sulphate of soda on cooling; and the whole quantity being partially evaporated, yielded an additional portion. The crystals of both, freed from adhering moisture, weighed 44·2 grains. The liquor being further evaporated, cubes of muriate of soda were formed on the sides of the capsule, while it was warm, and by continuing the evaporation, a quantity of this salt was obtained, which weighed when dry 12·3 grains. A small portion of liquor remained, which, by further evaporation, yielded crystals of sulphate of soda to the amount of 6 grains, with crystals of muriate of soda 2 grains.

The portions of muriate of soda obtained in the preceding experiments amounted to 765·1 grains. None of them, however,

were

were perfectly pure. Their solutions became turbid on the addition of sub-carbonate of soda, and of muriate of barytes, indicating the presence of sulphate of magnesia, or of muriate of magnesia and sulphate of soda, and probably indeed of portions of all these. The whole was submitted to the action of highly-rectified alcohol for twelve hours, with repeated agitation; the alcohol acquired a bitter taste; being poured off, and distilled, it afforded muriate of magnesia, which, heated with sulphuric acid, gave a product equivalent to 6·2 of real muriate. The residual salt still gave indications of the presence of sulphate of magnesia, by the tests of muriate of barytes and sub-carbonate of soda. The difficulty is so great, of separating a small portion of a salt from a large quantity of another, where the difference in their solubility is not considerable, that instead of attempting to remove the sulphate of magnesia by further crystallizations, it was decomposed by adding to the solution sub-carbonate of soda; the precipitate was collected, and converted into sulphate of magnesia by the addition of sulphuric acid. This, dried at a low red-heat, weighed 16 grains, equivalent to 33 crystallized. The salt had been previously exposed to a red-heat, when it weighed 744·5 grains. The above quantity of sulphate abstracted from this, leaves as the real quantity of muriate of soda 728·5 grains.

The powder G was soft, light, and tasteless. It weighed when thoroughly dried 7·5 grains. It might be expected to be similar to the powder A, and was therefore subjected to the same treatment. Diluted alcohol, acidulated with muriatic acid, excited effervescence; the liquor poured off afforded by evaporation muriates equivalent to 2·8 of *carbonate* of *magnesia*, and 1·3 of *carbonate* of *lime*. And the remaining *sulphate* of *lime* dried, weighed 3 grains.

By this analysis, then, the substances obtained from 4 pints of sea-water, and their proportions, are as follow:

Muriate of soda, ..	..	728·5 grains.
Muriate of magnesia real,	83·5	
	9 7	
	6·2	
	99·4	99·4
Sulphate of magnesia crystallized,	6·3	
	8·9	
	33	
	48·2, or real,	23·5
Sulphate of soda crystallized,	18	
	44·2	
	6	
	68·2, or real,	30·2

Sulphate

Sulphate of lime, real, ..	22	
	3	
	25	25 grains.
Carbonate of lime, ...	1·2	
	1·3	
	2·5	2·5
Carbonate of magnesia, ..	1·7	
	2·8	
	4·5	4·5

The two last ingredients might be accidental products, from the decomposition of muriate of magnesia and of lime. Muriate of magnesia is decomposed by heat; a portion of its acid is expelled; and the magnesia separated in consequence of this, will absorb carbonic acid, from the current of warm air applied during the evaporation, or from the carbonic acid gas which the sea-water itself contains, and which is not immediately expelled by heat. The small portion of carbonate of lime might be produced in a similar manner, or from the action of the carbonate of magnesia on muriate or sulphate of lime. I accordingly found, in a subsequent analysis, that on adding muriate of barytes to sea-water, no carbonate but only sulphate of barytes is precipitated, which proves that these conclusions are just. For the small portion, therefore, of carbonate of magnesia, the equivalent portion of muriate of magnesia, 4·2, raising it to 103·6, is to be substituted. If the lime which afforded the carbonate existed in the state of sulphate, then the equivalent portion of this 3·4 is to be added to the sulphate actually obtained, making it 28·4. If it existed in the state of muriate of lime, it still would, but for this change, have been converted in the progress of the evaporation into sulphate of lime; the same substitution, therefore, is in this view equally to be made. With these corrections, and reducing the proportions to a pint, the ingredients and their quantities will be as follow:

Muriate of soda, ..	182·1	grains.
Muriate of magnesia, ..	25·9	
Sulphate of soda, ..	7·5	
Sulphate of magnesia, ..	5·9	
Sulphate of lime, ..	7·1	
	228·5	

The results of the preceding analysis are different from those I had expected to obtain. I had supposed, that in Lavoisier's method, the sulphate of soda, and muriate of lime, which he stated as ingredients, had been obtained from some peculiarity in the process by which their reciprocal action, and consequent transition into muriate of soda and sulphate of lime had been prevented; and that in the common method of evaporation they are not obtained, because this mutual decomposition takes place. It appears, however, that the results by Lavoisier's method are different from those he stated, and are such as preclude this view. No muriate of lime is obtained, and sulphate of lime is obtained in considerable quantity; of course, the sulphate of soda, which is also found, cannot be considered as being procured, in consequence of its decomposition by muriate of lime being prevented by any peculiarity in the process, and must therefore be ascribed to some other cause.

Besides the peculiarity in this analysis of sulphate of soda, there is another singularity in the result, that little sulphate of magnesia is procured. This salt, it is well known, is extracted in considerable quantity by the common process of evaporation of sea-water on a large scale, being obtained by boiling down the bittern, while, by this method, little or no sulphate of soda is obtained.

The products of this analysis are thus so different from those usually assigned, and so different from those known to be afforded by the usual process of evaporation, that it became desirable to perform the analysis in the common mode, so as to ascertain the actual results of it with precision, with a view to determine on what these differences depend. This I accordingly executed.

A. Four pints of the same sea-water were submitted to evaporation in a sand-bath, and after the crystallization of the muriate of soda commenced, the liquor was poured off at intervals from the salt deposited, and further evaporated. This was continued as long as it appeared to afford no other salt on cooling than muriate of soda. The latter products of this salt were less pure than the first, being deliquescent on exposure to a dry atmosphere; they were therefore redissolved in water; by evaporation, the greater part was obtained crystallized, in a purer state, and was added to the other; and the small portion of residual liquor was added to the residual liquor of the evaporated sea-water.

B. By further evaporation, this liquor afforded crystals in slender prisms, which were permanent in the air, and which were found to be sulphate of magnesia; by repeated evaporations, successive crystallizations of this kind were produced, (small portions of muriate of soda being also obtained, which, after being washed, were added to the salt A); the products of the first crystallizations were nearly pure; those of the latter crystallizations

stallizations were less distinct in form, and were in part deliquescent.

C. The portion of liquor still remaining was evaporated, until, on cooling, it formed a congeries of slender prisms, which, exposed to the air, deliquesced, and soon passed to a state of perfect solution, a proof of their being principally muriate of magnesia.

The products thus obtained, consisted, first, of muriate of soda A; secondly, of sulphate of magnesia B; and, thirdly, of muriate of magnesia C. These, however, could not be supposed to be pure, and they were, therefore, submitted to further examination.

D. The muriate of soda A, gave indications of the intermixture of magnesian salts; the solution of a minute portion of it in distilled water becoming turbid on the addition of carbonate of soda. It was also to be presumed, that there would be mixed with it any sulphate or carbonate of lime deposited during the evaporation. It was therefore redissolved in water. There remained undissolved a residue, which, when thoroughly dried, weighed 22·6 grains. The salt was again procured by evaporation, but it was still not perfectly pure. Its dilute solution gave a milkiness with carbonate of soda; and oxalate of ammonia and muriate of barytes rendered it turbid, indicating the presence either of a little muriate of lime with sulphate of soda, or magnesia, or of sulphate of lime with a portion of sulphate or muriate of magnesia. The whole was redissolved in distilled water; a powder, similar in appearance to the insoluble residue of the former solution, remained undissolved, which, when thoroughly dried, weighed 10·3 grains. To the clear solution a portion of alcohol was added, not sufficient to cause any precipitation of muriate of soda; it produced a slight turbid appearance, and after some hours a powder had subsided, which, after being washed with water, was tasteless: it weighed 1·5 grain. The muriate of soda, obtained by evaporation, weighed, when dried, 718 grains. Being still not entirely pure, it was reserved for another operation.

E. The insoluble residues collected in the preceding operations being put together, were submitted to the action of alcohol, acidulated with muriatic acid, to remove any carbonate of lime, or of magnesia. Effervescence was excited; the liquor being poured off, and the insoluble residue of sulphate of lime being washed with a little water, weighed, after exposure to a heat nearly equal to ignition, 26·3 grains. The alcoholic solution, with the addition of the small portion of water with which the sulphate of lime had been washed, afforded, by evaporation, a matter which entered readily into fusion, and which, treated with sulphuric

 acid

acid, gave 5·6 of sulphate, equivalent to 4·3 of carbonate of magnesia, and 3 of sulphate, equivalent to 2·2 of carbonate of lime. I have already observed, however, that no carbonic acid is detected in sea-water by the test of barytes; these carbonates, therefore, are, as before, to be considered as products of the evaporation, arising from the decomposition of muriate of magnesia, and of muriate or sulphate of lime. The one, but for the decomposition by which it is produced, would have appeared as sulphate of lime; it increases, therefore, the proportion of that ingredient to 29·3 grains. The portion of muriate, equivalent to the other, that is, 4·4 grains, may be added to the quantity of that salt obtained in the subsequent steps of the analysis.

F. The products of the different crystallizations B, consisting chiefly of sulphate of magnesia, with portions of muriate of magnesia, were left exposed to the air for some days, and the liquor formed from them by deliquescence was poured off occasionally, and added to the solution of muriate of magnesia C. The residues were then washed with pure alcohol, to abstract more completely any muriate of magnesia. The portions remaining undissolved, were dissolved together in water. By evaporation, they afforded sulphate of magnesia in bevelled prisms; by further evaporation, muriate of soda in cubes was obtained; and by successive evaporations, there were thus procured sulphate of magnesia in crystals, 46·6 grains; and muriate of soda 39 grains. A small portion of liquor remained, which, containing chiefly muriate of magnesia, was added to the liquor C.

G. This liquor C, to which the portion of liquor formed by deliquescence from B had also been added, was evaporated to dryness. It was then submitted to the action of successive portions of alcohol, employing, first, the alcohol with which the saline matter B had been lixiviated, and afterwards pure alcohol. These liquors, poured off from a portion which remained undissolved, were evaporated to dryness; the dry mass was dissolved in water, and, by a second evaporation, afforded a congeries of prisms of *muriate of magnesia*. Dried by a heat of 150°, the weight amounted to 156 grains. Converted into sulphate of magnesia by the addition of sulphuric acid, the product weighed, after being dried at a low red-heat, 99·2 grains, equivalent to 78·4 of real muriate of magnesia.

H. The matter which remained undissolved by the alcohol G was dissolved in distilled water. The solution was evaporated, until, by a further spontaneous evaporation in a warm apartment, crystals were successively formed; these were sulphate of magnesia, and, in general, bevelled prisms. The whole freed from moisture weighed 48·6 grains. A small portion of liquor remained,

remained, which, when evaporated, gave a deliquescent saline mass: by slow evaporation 2·6 grains of muriate of soda were obtained from it; the remainder yielded muriate of magnesia equal to 3 grains.

I. The crystals of sulphate of magnesia obtained by the successive evaporations, were not all equally well formed; and after they had been left exposed to a dry air for some time, some of them became quite efflorescent, while others did not. The former were picked out, and each portion was redissolved in water. By a new crystallization, there were thus obtained 72 grains of sulphate of magnesia, and 18·5 of sulphate of soda.

K. The muriate of soda obtained in the preceding steps amounted in all to 759·6 grains. After exposure to a red-heat, it weighed 752·4 grains. It has already been stated, that it was not perfectly pure; its solution being rendered milky, both by sub-carbonate of soda and muriate of barytes. The separation of the sulphate of magnesia, which this chiefly indicated, was not to be completely looked for by solution and crystallization. Subcarbonate of soda was therefore added to the solution as long as any precipitation took place; the precipitate, heated with a sufficient proportion of sulphuric acid to redness, gave 16·4 sulphate of magnesia, equivalent to 33·7 of the same salt crystallized. The former quantity abstracted from the weight of the muriate of soda, reduces it to 736 grains.

This analysis, then, affords the following ingredients, and their proportions in their *real* state:

Muriate of soda,	..	..	736 grains.
Muriate of magnesia,	..	..	85·8
Sulphate of magnesia,	..	..	51·2
Sulphate of soda,	..	..	8
Sulphate of lime,	..	..	29·3

Or, reducing them to a pint of the water,

Muriate of soda,	..	..	184 grains
Muriate of magnesia,	..	..	21·5
Sulphate of magnesia,	..	..	12·8
Sulphate of soda,	..	..	2
Sulphate of lime,	..	..	7·3
			227·6

By the two modes of analysis now stated, different results have been obtained. There are common to both as the principal products, muriate of soda, and muriate of magnesia. But in the one, sulphate of magnesia, with only a small proportion of sulphate of soda, is procured. In the other, sulphate of soda in a much larger quantity, with only an inferior proportion of sulphate of magnesia, is obtained. How is this diversity of result to be accounted for?

As the relative quantities of these salts are thus varied, and are indeed nearly altogether dependent on the kind of analysis, it is obvious that one or other of them must be an original ingredient, and the other must be a product of decomposition. If sulphate of magnesia is the original ingredient; then, when it is not obtained, or is obtained only in very inferior quantity, while sulphate of soda is procured in its place, it must be held that it is decomposed; and the only decomposition that can account for these results is, that from the mutual action of muriate of soda and sulphate of magnesia, by which, while portions of them are removed, corresponding portions of sulphate of soda and of muriate of magnesia are formed. On the other hand, if sulphate of soda is the original ingredient, then, when it is not obtained, or is obtained only in small quantity, it must be held that it is decomposed; and the only decomposition of it that can here take place, must be from the action of muriate of magnesia, by which, while quantities of both these salts are removed, corresponding quantities of sulphate of magnesia and muriate of soda will be produced.

Of the two analyses, the one in which sulphate of soda principally is obtained, is that in which the solvent action of alcohol is employed; the other, in which there is the mere separation of the salts by evaporation and crystallization, is that which affords scarcely any of it, but in place of it sulphate of magnesia. Now, it is to be observed, that in both of these the preliminary operation of evaporation to dryness is the same. Since sulphate of soda, therefore, is not obtained by this operation, it is obvious, that, even on the assumption of its being the original ingredient of sea-water, it must, in the progress of the evaporation, be decomposed by the muriate of magnesia, and converted into the sulphate of magnesia; and hence, in the subsequent solvent action of the alcohol, by which it is obtained, it must be reformed. And, on the other hand, if sulphate of magnesia is the primary ingredient, and is obtained as such by the evaporation, it remains to be explained, how it is converted in the subsequent solution by the alcohol into sulphate of soda. The whole question, therefore, resolves itself into the nature of the action of the alcohol, producing sulphate of soda; and of this I perceive no other solution than that which I have now to illustrate.

The fact, however it is to be explained, or to be reconciled with the doctrine of chemical attraction giving rise to combinations or decompositions according to the strength with which it is exerted, seems to be established by an induction too strict and extensive to admit of doubt, that these results are often determined by the force of cohesion, in such a manner, that in principles acting on each other, those on which this force operates

rates most powerfully, in relation to the fluid which is the medium of action, are combined together. So much is this the case, that, as Berthollet has justly remarked, we may, from a knowledge of the solubility of the compounds which substances form, predict what combinations will be established when they act on each other; those always combining which form the least soluble compounds. So far the influence of this has been illustrated by this able chemist. But it appears to me to admit of further extension, so as to afford a solution of the present question.

If the force of cohesion can so far modify chemical attraction, as to establish among compound salts dissolved in any medium, those combinations whence the least soluble compounds are formed, we are entitled, I conceive, to conclude, that the reverse of this force, that is, the power of a solvent, may produce the opposite effect, or cause the reverse of these combinations to be established. Suppose muriate of magnesia and sulphate o soda to be dissolved in water, and the solution to be concentrated by evaporation, the combinations of sulphate of magnesia and muriate of soda being on the whole less soluble in water, this circumstance of inferior solubility, or the force of cohesion thus operating, may determine the formation of these, and, accordingly, their formation is found by experiment to take place. But suppose sulphate of soda and muriate of magnesia to be dissolved by the aid of heat in alcohol so far diluted as to effect their solution, then those combinations will not be established which existed in the watery solution, because, on the whole, sulphate of magnesia, and muriate of soda, are less soluble in alcohol, even in this diluted state, than sulphate of soda and muriate of magnesia. These latter compounds will, therefore, remain undecomposed. But further, this *may* give rise, or, rather, *must* give rise, in conformity to the principle above stated, to the reverse effect; so that, suppose sulphate of magnesia and muriate of soda to be submitted to the action of this diluted alcohol, aided by heat, the solvent power considered, in relation to the reverse combinations, may cause the change in the state of these compounds, and their transition into muriate of magnesia and sulphate of soda.

In the analysis of sea-water, then, by the first of the methods above described, the evaporation may either, if sulphate of magnesia and muriate of soda are the original ingredients, afford them undecomposed in the solid state; or, if muriate of magnesia and sulphate of soda are the ingredients, it may cause, by the influence of the force of cohesion, the formation of sulphate of magnesia and muriate of soda. But when the solid mass is submitted to the action of alcohol, its operation, as a solvent,

 may,

may, on the same principle, cause the reverse combinations to take place, of muriate of magnesia and sulphate of soda; the quantity of this sulphate being, of course, equivalent to the quantity of sulphate of magnesia; and the quantity of muriate of magnesia formed, being added to the quantity of that salt which the sea-water contains as a primary ingredient. Thus is explained the diversity of results obtained by the two modes of analysis; and this diversity itself affords an excellent illustration of the change of combination which may be produced in mineral waters by analytic operations, and a very conclusive proof that the substances obtained by the analysis are not always to be regarded as the original ingredients, since here they are varied according to the mode in which the analysis is performed*.

* The small portion of sulphate of soda obtained with the sulphate of magnesia, in the second analysis, may have been formed by the action of the alcohol, which, though employed much less extensively than in the first, was still introduced to a certain extent. Or it might originate from other circumstances independent of this; for a similar result, I have been informed, sometimes occurs in the large way, sulphate of soda being procured in boiling down the bittern of sea-water to obtain its sulphate of magnesia, or in purifying this sulphate. The circumstances on which this depends, it may be difficult to assign with perfect precision; but it probably arises from the relative quantities of the different salts, and their tendency to crystallization, as influenced by the state of concentration, and the temperature. That both of these have a considerable effect on the combinations established in a compound saline solution, has been sufficiently shown by the experiments of Berthollet and others. A striking proof of it was derived from the very salts which are the subject of the present observations, in a singular case of affinity, first observed by Scheele, and afterwards confirmed by Gren: that of muriate of magnesia and sulphate of soda, which decompose each other in a concentrated solution at a high temperature, producing muriate of soda and sulphate af magnesia; but, at temperatures below 32°, the reverse effect takes place, muriate of soda and sulphate of magnesia reacting, and being converted into sulphate of soda and muriate of magnesia. This singular case is evidently owing to the relation of the solubility of these salts to temperature. Muriate of soda has its solubility little increased by heat, of course little diminished by cold; sulphate of soda is in this respect precisely the reverse: hence, at an elevated temperature, muriate of soda is the less soluble salt; and this determines its formation and separation from a compound solution, containing its elements; at a low temperature, again, sulphate of soda is the less soluble salt; and this equally determines its formation, of course occasions the reverse decompositions. Now, according to the proportion of saline ingredients, and according to the state of concentration, and the temperature favouring the tendency of certain salts to crystallization more than others; it is easy to conceive, that in a compound solution, different combinations may be established, as these circumstances vary, and thus products may be obtained, under certain conditions, which are not obtained under others. Although sulphate of magnesia, therefore, is usually obtained by evaporation from sea-water, sulphate of soda, at some stages of the operation or under peculiar circumstances, with regard either to relative quantity of the elements, or to temperature, may likewise be formed.

Lavoisier had stated muriate of lime as having been obtained in his analysis, being dissolved with the muriate of magnesia in the alcohol with which the solid matter obtained by evaporation had been lixiviated. I found no trace of it; and its presence after the evaporation to dryness, does not seem compatible with that of either sulphate of soda, or of magnesia. Yet if the preceding reasoning be just, it is possible that alcohol, by its solvent action, might cause its reproduction to a certain extent from sulphate of lime. On the other hand, the entire insolubility of sulphate of lime in alcohol, might prevent it from being acted on; this is even more probable; and the result stated of muriate of lime being obtained, is therefore, in all probability, to be ascribed to error, principally perhaps to its not being distinguished sufficiently from muriate of magnesia, the quantity of which is stated by Lavoisier evidently too low.

[To be continued.]

V. *On Dry-Rot. By Mr.* JOHN SHILLIBEER.

To Mr. Tilloch.

Plymouth, Dec. 12, 1817.

SIR,— IF on perusing the letter which is below, you should think proper to give it room in your Magazine, it will be greatly esteemed. It has been submitted officially to the Navy Board, and the writer of it has received the thanks of the Board for the communication.

I am, sir,

Your friend and constant reader.

"Walkhampton, near Plymouth, Oct. 25, 1817.

"HON. SIRS,—The dry-rot which has made its appearance in His Majesty's navy having excited general attention, I have been led to reflect, not only on the cause which has produced it, but the probable mode of its prevention; and the result of these reflections I shall beg to lay before the Honourable Board.

"Any cure for the dry-rot in timber when once it is infected, I believe, has not yet been found; and the disease appears to prevail more in the oak than in any other timber; but the reason is to me evident, as on account of the value of its bark the mode of treatment is very different to that of any other class. To effectually prevent the appearance of the dry-rot, the tree should be cut down about the end of December, or the commencement of January, and left with its bark on, instead of being cut down in April, and having its bark stripped off, which is the mode generally pursued in this country. From the great consideration of the bark the durability of the timber has been lost sight of, and

and the oak is invariably felled immediately after the tree has recommenced its growing—when its pores are open and extended to receive the great quantity of sap which is thrown from the roots up into the trunk and branches of the tree. At this period the tree is soft and easy to be cut, and the bark separates from the trunk with great facility. In this state it is left to season. The sap which should have returned into the roots is dried by the sun; but the pores of the timber never close themselves again; nor does the tree become that solid mass it was before. The pores remain open, and soon become infected with this pernicious disease, which I have no idea can be eradicated.

" Now, if a tree be let stand until the vegetation has entirely ceased (say Christmas), the sap will have returned into the roots; the pores which had been opened in the spring to receive it will be naturally closed;—the bark, which would have separated with ease, will be found inseparable; and the tree when cut and seasoned (for a comparatively short time) will be so hard and impenetrable as to prevent the disease from ever affecting it. The bark under such circumstances becomes a solid mass and secures the tree from injury, and consequently prevents the introduction of the dry-rot.

" If the Honourable Board will compare the difference between two trees of the same age which have grown in the same wood, (the one being cut down in the spring and barked, the other cut down at Christmas and left with its bark on,) it will be strikingly obvious; the former will be soft, open, and coarse-grained; the latter close-grained and solid.

" The miners for their machinery always prefer timber which has not been barked. The millwright for the wheel which is exposed constantly to the water, chooses timber cut at Christmas, because it will last at least as long again as that which is cut in April. The farmer even for his barn-floor cuts his timber at Christmas, and sacrifices the bark; and because a floor made of such materials will last for ages, when another will in a few years be unserviceable. In many old buildings, but particularly churches, erected prior to the time when bark became so enhanced in value, the timber is still firm and solid, or only impaired from great age and the worms. In buildings of a more recent date the oak timber has perished altogether. If then a mill-wheel, a barn-floor, or any machinery made of timber cut down at Christmas be so durable, it is but a natural conclusion that a ship built of such materials will derive a similar advantage. I have no hesitation in saying that, if there be two ships built, the one of timber cut in January, the other of timber cut down in April, the former will be firm and solid when the latter will be entirely

entirely unfit for use—that the one shall (or perhaps may) be infected with the dry-rot—the other free from such disease.

"I have conversed with men of great experience and knowledge in timber, and the result has always gone to strengthen my opinion, that the oak timber has not degenerated; but that the dry-rot is given it by the improper mode of cutting it down and preparing it for use; and I am firmly persuaded that, in a ship built of timber so prepared, this destructive disease will never make its appearance.

"I could expatiate on this subject, and adduce the opinions of others to qualify what I have stated; but I think it will require little argument to show that a tree cut down in its natural state, and seasoned in the mode I have described, will be a more solid substance, finer-grained, and consequently less liable to be infected with any disease, than that of a tree cut down, immediately stripped of its bark, and left exposed to the elements, from which, in fact, the disease is absolutely engendered.

"In making these suggestions I have been actuated by the good of my country; and if they should ultimately prove to be founded on a solid basis, and that benefit will result from them, I feel great confidence that they will, by the Honourable Board, be appretiated accordingly; and,

"I am, Hon. Sirs,

"Your obedient servant,

"*To the Hon. the Commissioners of the Navy, &c. London.*

JOHN SHILLIBEER,
1st Lieut. Roy. Marines."

"P.S. A small reduction in the tax on leather would prevent an inconvenience to the public from any rise in bark, and the expenditure of the navy would be considerably reduced.—J. S."

⁎ I have been just informed that orders are issued not to receive timber into the Dock-yard that has been barked;—if so, and it be attended with success, these suggestions of Lieut. S's will reflect great credit on him.

VI. *Observations on the Temperature of the Ocean and Atmosphere, and on the Density of Sea-water, made during a Voyage to Ceylon. In a Letter to Sir* HUMPHRY DAVY, *LL.D. F.R.S. By* JOHN DAVY, *M.D. F.R.S.**

MY DEAR BROTHER, ACCORDING to the promise contained in a former letter, I proceed to give you a short account of the observations which I made during my late voyage from England to Ceylon. At present, I shall confine myself chiefly to three topics,

* From the Transactions of the Royal Society for 1817, part ii.

pics, the specific gravity of the water of the ocean, and its temperature, and the temperature of the atmosphere; subjects of some importance in the natural history of our globe; and in which, I know, you are interested. Incidentally I shall notice the height of the barometer, the direction of the winds, and the state of the weather.

For the sake of brevity, I shall present the principal results of my observations in the form of a Table, to which I shall add some explanatory notes and general remarks.

[The form of our pages does not admit of our inserting, at length, the table here alluded to, which fills three quarto pages; but this is the less necessary, as the remainder of the paper is devoted to a detail of the experiments and their results—some of which, especially what relates to the diminished temperature of sea-water in shallows, are highly deserving of the particular attention of nautical men, as serving to announce the approach of land.—Edit.]

In all the experiments on the density of sea-water, the results of which are recorded in the journal, the water used was taken from the surface of the ocean, in a large clean bucket. The results introduced before we passed the equator the first time, were procured at sea; the remainder, from 0° 12″ south lat. to Ceylon, were obtained on land from experiments made on specimens of water preserved in well-corked phials. In the experiments on board ship, as soon as the water was drawn, its temperature was ascertained, and then it was immediately weighed. The balance employed was not very delicate, for a very delicate instrument does not answer at sea, on account of the ship's motion; however, it was pretty readily affected by 1-10th of a grain. The glass vessel in which the water was weighed, was such a one as is commonly used at home; its capacity was equal to about 300 grains. In the experiments on shore, the same vessel was used, but a different balance, one of a more delicate construction. I have chosen the temperature 80° Fahrenheit, for which I have calculated all the results, because it is nearly the mean annual temperature of this place, and nearly the mean at sea, in the intertropical regions.

The experiments made at sea I do not of course value so much as those made on land: considered, however, merely as approximations to the truth, which I am sure they are, the results favour the general conclusion already formed by some philosophers, that the ocean resembles the atmosphere in being (*cæteris paribus*) of nearly the same specific gravity throughout.

And further, they lead to the conclusion, that the slight variations of specific gravity observed, do not regularly conform to the difference of temperature.

That

That the specific gravity of the water of the ocean, in all its parts, however remote, should be nearly the same, is easily explained; it is indeed what might be expected from theory. It is more difficult, it appears to me, to account for the slight variations; I may remark, they appeared to me greatest when the sea was rough and agitated; and once the specific gravity of the water seemed diminished by a heavy fall of rain, viz. in lat. 4° north, and in long. 18° 13″ west, where we experienced a quick succession of tropical squalls.

Whether there is a specific gravity peculiar to the water of each zone, as a modern traveller of high authority endeavours to prove, I am greatly in doubt. From my own experiments, in which I cannot but put some reliance, I feel much inclined to infer the contrary, and especially from those made on land, which I know to be perfectly accurate. Several of these agree in giving the same specific gravity to specimens of water taken from parts of the ocean very remote from each other: for instance, the water from lat. 0° 12″ south, and 22° 36″ south, and that from 34° 25″ south, and the water that washes the shores of Colombo.

For ascertaining the temperature of the air and of the water of the ocean, I used delicate pocket-thermometers, the bulbs of which projected about an inch from the ivory scale. In the experiments on the temperature of the ocean, the water was tried the instant it was drawn, before it was affected by the air. To find the temperature of the air, I always chose the coolest part of the ship on deck, and always put the instrument in the shade, and exposed it to the wind, taking care not to bring it near any surface that had the power of radiating much heat; circumstances, I need not remark, of importance to be attended to, and, in consequence of the neglect of which, the temperature at sea, in the intertropical regions, has by most observers been overrated.

During the greater part of the voyage, observations were made every two hours, on the temperature both of the air and of the water; and with the kind assistance of the mates of the ship, Messrs. Sleight and Powell, intelligent and obliging men, they were carried on during the night as well as the day.

I am not aware that the law of the diurnal variation of the temperature of the atmosphere at sea, has been described by any writer. From the numerous observations, which I had an opportunity of making, between and bordering on the tropics, it appeared to me perfectly regular at a great distance from land, when the weather was fine, and the wind steady. In these circumstances, I found the air at its maximum temperature precisely at noon, and at its minimum towards sunrise. I shall give in illustration of the fact two instances from my note book.

April

April 2d. S. lat. 21° 3″. W. long. 27° 27″. Wind E. by S.

Hour.	Temperature.	Hour.	Temperature.
	° ″		° ″
6 A.M.	78	8	78
8	79	10	78
10	79, 5	12	77,75
12	80	2 A.M.	77,75
2 P.M.	79,25	4	77, 5
4	79	6	77, 5
6	78, 5		

April 5th. S. lat. 24° 22″. W. long. 26° 27″. Wind ENE.

Hour.	Temperature.	Hour.	Temperature.
	° ″		° ″
6 A.M.	76	6	77, 5
8	77, 5	8	77
10	78,25	10	77
12	79,75	12	76, 5
2 P.M.	78, 5	2 A.M.	76, 5
4	77,75	4	76

Here we perceive the variation of the temperature of the air, following the course of the sun, pretty considerable whilst it is above the horizon, and very insignificant during the night; and this, I may remark, is a general fact at sea, and one of the principal features of difference between the temperature of the atmosphere over the land, and over the ocean.

The law of the regular variation of temperature is frequently interrupted. Even in fine weather, when the air is not in motion, it is subject to interruption. During a calm, the variation of temperature is nearly the same as on land, the maximum degree of heat not being at noon precisely, but some time after, and for the same reason; because there is an accumulation of heat, and not only in the ship, but actually in the water itself, as I may show by noticing the temperature of the air and of the sea, during even a short calm, hardly of twenty-four hours duration.

August 7th. N. lat. 2° 10″. E. long. 76° 37″.

Hour.		Temp. of Air.		Of the Sea.
		° ″		° ″
6 A.M.	..	78,5	..	80
8	..	79,5	..	81
10	..	80,5	..	81,5
12	..	82	..	82,5
2 P.M.	..	82,5	..	83,5
3	..	82	..	83,5
4	..	81,5		

But the law is more remarkably interrupted during storms and unsettled

unsettled weather, as a couple of instances will be sufficient to prove.

March 17th. N. lat. 4°. W. long. 18° 30″.

Hour.	Weather.	Temp. of Air.	Of the Rain-water.
3 A.M.	Clear ..	80	
11	Rain approaching	77	76
11h 30	Just passed ..	74	73
12	Cloudy ..	79	
1 P.M.	After a shower	76,5	76
4	.. ..	75	74

March 27th. S. lat. 10° 30″. W. long. 24° 25″.

Hour.	Weather.	Temp. of Air.
		° ″
5 A.M.	Fair	79
6 ..	Rain approaching ..	78
6 30 ..	Raining heavily ..	75, 5
7 ..	Rain just ceased ..	76, 5
8 ..	Sunshine	79,25
9 ..	Raining	76
10 ..	Cloudy	79, 5
12 ..	Fair	80, 5

The showers in each instance were accompanied by hard gusts of wind, and thunder and lightning. The rain-water, the temperature of which was ascertained, was collected in a glass as it ran from the awning.

The equatorial regions appear to be particularly subject to storms, violent rain, and electrical phænomena, the effect of which, in diminishing the temperature, seems to afford a natural explanation of the comparative coolness, both of the atmosphere and the ocean, that we experienced each time we passed the line.

The temperature of the sea, it has been asserted by some writers, is subject to little or no diurnal variation. That this remark is far from correct, is evident from the slightest inspection of the Meteorological Journal: it is an opinion that could be formed only from hypothetical views, ill-founded. The fact, as the Journal exhibits, is, that the diurnal change of the temperature of the sea is very nearly as great as that of the incumbent atmosphere. From all the observations I could make, when the circumstances were most favourable to accurate results, when the weather was fine, the sea smooth, and the land at a great distance, it appeared to me, that the maximum temperature is about three in the afternoon, and its minimum towards sunrise. I shall give a single example in detail.

April

April 5th. S. lat. 24° 22″. W. long. 27° 8″.

Hour.	Temp. of the Sea.	Hour.	Temp. of the Sea.
	° ″		° ″
8 A.M.	79,25	8	79, 5
10	79, 5	10	79
12	79, 5	12	78, 5
2 P.M.	80	2 A.M.	78
4	80, 5	4	77,75
6	80	6	76

Like the atmosphere, the ocean is subject to irregularities of temperature. This fact is proved by the Journal in an ample manner. The causes which produce these irregularities may be divided, very generally, into three kinds,—tempestuous weather, shoals, and currents.

Independent of other modes of operation—and they are various in tempestuous weather—superficial currents appear to be established in the course of the prevailing winds. If the wind be from a cold quarter, the temperature of this current is comparatively low, and *vice versâ*. This fact is manifest in the effect of the gales we experienced between the 7th and 12th of April, during which time, being south of the equator, and the wind blowing from the south, the temperature of the sea was considerably reduced.

Where the sea is shallow, it is now a well-established fact*, that the temperature of the water is comparatively low; an important circumstance, highly deserving the attention of the practical navigator; it may forewarn him of a bank in the darkness of night, when nothing else would indicate it, and put him on his guard when approaching low shores and shallows, time enough to avoid their dangers. In advancing towards the Cape of Good Hope, and in doubling that promontory, and in making Ceylon, I collected some observations on this subject, the results of which I shall now introduce. On making Table-bay, before land was to be seen, there was a decided fall of the temperature of the water, viz. from above 60 to 58, thus,

May 11th. S. lat. 34° 1″. E. long. 17° 51″ at

				° ″
8 A.M. the temperature of the water was				62,5
10	..	..	..	62,5
12	..	..	..	61,5
2 P.M.	..	..	..	61
5	..	..	..	60

* Observed by Dr. Franklin, Mr. J. Williams, &c.—See Williams's Thermometrical Navigation. Philadelphia 1790.

Hour.				Temperature.
				° ″
10	..	..	..	58
12	..	..	..	58
2 A.M.	..	..	..	58,5
4 Land in sight		..	..	59
7 About twenty miles from land				58
8	..	..	..	57
10	..	..	..	56
12	..	..	..	56
2 P.M.	..	..	..	55
4	..	..	..	56
8 In soundings		..	..	56,5
10	..	..	..	56,5
12	..	..	..	55
4 A.M.	..	..	..	55
6	..	..	..	56,5
8	..	..	..	56,5

During these two days we were gradually approaching land, at the average rate of about two miles an hour. The observations were continued, till we were within about two miles of the shore. The observations I made on leaving the bay, corresponded with the foregoing, as nearly as could be expected, considering the track was not precisely the same, and the cold season more advanced.

						° ″
June 3d.	8 A.M.	Half a mile from land, temp. of water				53
	10	About three miles from land				54,25
	2 P.M.	Off Robin Island, nine miles from Cape Town, in ten fathoms water				55,25
	4	..	..	..	..	55,25
	12	..	..	..	..	54, 5
	2 A.M.	..	..	..	..	54, 5
	8	..	..	..	..	57, 5
	10	..	..	..	..	57
	12	..	..	..	..	60
	2 P.M.	..	..	..	..	61
	4	..	..	..	..	62

Before four in the afternoon we were out of sight of the Cape of Good Hope, and in deep water.

In approaching Ceylon, and particularly the southern shore of the island, where the mean annual temperature appears to be about 80°, little or no change of temperature could be expected on entering shallow water; yet we experienced a manifest change, a reduction of at least two degrees on coming into soundings. When we were in north latitude 5° 17″, and east longitude by chronometer 79° 42″, the temperature of the water

 began

began to fall; in the morning at eight, it was 78° 5″ and at ten at night it was 76° 5″. Next morning, land was discovered.

From the observations, in general, on the temperature of the water, recorded in the Journal, there is reason to believe, that during the whole voyage we were frequently encountering currents. Many of the results stated, are scarcely to be explained on any other hypothesis. When the temperature of the water became suddenly reduced, I inferred we were either in a current from the poles, or over some high ground in the bed of the ocean; and the former conclusion was almost constantly confirmed by other observations. And on the contrary, when the temperature of the water experienced a sudden increase, I inferred that we were in a current flowing from the equatorial regions. The only current we passed, that appears to me to require particular notice, is the well-known one that flows round the bank of Lagullas, from the south-east coast of Africa. It is marked in all charts, and it has been pretty minutely, and very scientifically, described, and its course explained, by Major Rennell; but hitherto, I believe, no notice has been taken of its high temperature, or of the effect which I believe it has, in producing a curious phænomenon on the summit of the Table-mountain, not yet accounted for, viz. a dense covering of mist called the "table-cloth," which universally appears when the wind blows from the south-east. I shall copy from my notes, taken at the time, the observations I made in crossing this current.

June 10th. S. lat. 35° 57″. E. long. 24°.

Hour.	Temp. of the Sea.	Hour.	Temp. of the Sea.
	° ″		° ″
6 A.M.	61	11	67
8	71, 5	12	67
10	70, 5	1 A.M.	67
11	70	2	67
12	68	3	61
1 P.M.	68, 5	4	61
2	67, 5	5	64
4	68	6	66,75
5	67	7	66
6	66, 5	9	67
7	67	10	67, 5
8	67	12	66
9	67	2 P.M.	67, 5
10	66,75	4	65, 5

Now, judging from the change of temperature, we appear to have suddenly passed from the bank of Lagullas into the current that flows round its borders. Major Rennell, I believe, observes,

observes, that at the border of the bank the current is strongest; the high temperature of the water there, at least ten degrees above the neighbouring seas, is readily accounted for on that idea. We appear to have continued in the current seventeen hours, the course the ship was going was nearly due east, her average rate 7·65 miles an hour; and hence, supposing we were sailing immediately across the stream, as probably we were, or very nearly, its width may be inferred to be about 130 miles; a distance little differing from that commonly assigned to it. Having traversed this current, we seem, from the low temperature of the water for two hours, to have been passing a bank twelve miles wide, and then to have entered a second current running in the same direction as the first.

I have alluded to a connexion between these currents and the covering of dense mist, that occasionally occurs on the Table-mountain, called the "Table-cloth." The connexion is evident, and readily explained. The phænomenon only presents itself when a cold wind blows, viz. the south-east. This wind must condense the aqueous vapour rising from the warm current, and carry it towards the land. During the short stay we made at the Cape, I once had an opportunity of seeing the mist advancing; it came rapidly over the surface of the sea, which it entirely concealed, whilst the air above was perfectly clear; it soon reached the land, spread along the coast gradually, ascended the mountain, and there remained almost stationary, enveloping the summit, sometimes increasing and descending on the opposite side overhanging Cape Town, and sometimes diminishing and retreating. That it should remain so nearly stationary on the top of Table Hill, whilst the south-east wind continues, is not surprising, considering the height of this hill, 3582 feet above the level of the sea, its precipitous sides, and the extensive surface of its top; nor is it strange, that it should rarely descend, except when the wind blows hard, taking into account the situation of the ground beneath, sheltered and warm, and the site of a large town, from which a current of hot air must be constantly rising.

I cannot conclude without insisting with Mr. Jonathan Williams on the use of the thermometer at sea; if commonly employed, and the observations made with it recorded, a general knowledge might soon be obtained of the average temperature of all parts of the ocean, and a fund of curious and useful information might be collected, especially respecting currents and shoals, that to practical navigators could not fail of being highly serviceable.

In another letter, I propose communicating to you the observations I have collected on the temperature of man and other animals in different climates. The experiments were made during my voyage, and during my stay at the Cape, and the Isle of France, and my residence at this place.

I remain, &c.

Colombo, Nov. 3, 1816. JOHN DAVY.

VII. *Remarks on the Geological Principles of* WERNER, *and those of Mr.* SMITH. *By* THOMAS TREDGOLD, *Esq.*

To Mr. Tilloch.

SIR,—IN the last number of the Edinburgh Review it is stated, that "it has been proved (and Werner was the first to make the observation) that the masses or strata that constitute the surface of the globe, present themselves in groups or assemblages, the members of which are generally associated, whenever they occur, and are so connected as to exhibit a certain unity of character.

"To such assemblages Werner gave the name of *formations;* and his doctrine (or hypothesis, if this latter term be preferred) is, *that the exterior of the earth consists of a series of these formations, laid over each other, in a certain determinate order.* Not that the whole series is anywhere complete; but that the relative place of its several members is never departed from. Thus, in a series A, B, C, D, it may happen that B or C, or both, may be occasionally wanting, and consequently D be found immediately above A; but the succession is *never* violated, nor the order inverted, by the discovery of A above the formations B or C, or D, nor of B above those that follow it, &c."

"The only rival claimant to this doctrine, that we know of, is Mr. William Smith, the publisher of the Geological Map of England that has recently made its appearance—a work which it would be unjust to mention without adding, that it is of great and original value; indeed, regarding it as the production of an unassisted individual, of most extraordinary merit. For although the publication of this map was delayed till the year 1815, we have no doubt that Mr. Smith's acute and laborious researches originated entirely from the facts which came before him in examining the stratification of England many years ago; and that he was then, and long afterwards, wholly unacquainted with what had previously been done by Werner. The opinions of Mr. Smith, however, so nearly coincide with the doctrine of *formations* which we have just stated, that it would be difficult to express them in any other terms*."

From this quotation it appears, that this eloquent writer is

* Edinburgh Review, No. lvii. p. 71.

better

better acquainted with the results than with the principles on which Mr. Smith has proceeded in his researches; otherwise he certainly would not have considered them to be the same as those of Werner. It is true that the one supposes his formations are laid over one another in a determinate order, and the other has observed the same in the series of British strata; but the law of succession, of Werner, is purely hypothetical; that of Mr. Smith is the result of multiplied observations, and has been found to be correct as far as relates to the British strata.

Werner's law of succession, which he pretended was universal, evidently flowed from his hypothesis of the formation of the earth; an hypothesis which sets both reason and experience at defiance. The progress of inquiry would, however, have very soon shown its fallacy in the hands of any other person than Werner. But he saw that his law was not the law of Nature,—various strata were found to succeed one another in a different order from what he had assigned them in his hypothesis: this however was easily remedied by creating a distinction without a difference; and the formation was termed a *newer*, or an *older* formation, as the case required. Thus, we have *new granite* and *old granite*, and the same of other substances:—besides, in the class of formations which Werner calls transition, there appears to be no regular order of succession whatever; for Dr. Thomson says, "they all alternate with each other, sometimes one sometimes another being undermost*." But even the classes of Werner do not always succeed one another in the order which Werner assigned them; granite being sometimes found above strata which contain petrifactions†.

Also, there is nothing more evident than that the Wernerians are without any fixt principles of tracing the structure of the earth; for they are always in doubt and difficulty—even in those places where they constantly reside, and where the tracing the strata presents no difficulty whatever‡: they write as mineralogists, but certainly not as geologists;—they say a formation *occurs* in this or that country, (seldom pointing out the place with the least precision,) and that it is *probably* of the primitive, transition, or flœtz class of formations—almost always as if the rock occurred in detached patches,—seldom describing it as a continued stratum; and, instead of attempting to show the structure of the country (on which this far-famed hypothesis is founded) by maps and sections, the Wernerians content themselves with giving a string of technical terms connected by expressions which are scarcely to be understood§.

* System of Chemistry, iii. 558. ed. 1817. † Thomson's System of Chem. iii. 558. ‡ Professor Jamieson's Elements of Geognosy iii. 205. § See the Description of the Hartz. Reg. Elem. Geog. iii. 71

How different is the course which Mr. Smith has pursued in his attempt to develop the structure of his native country! His principles have arisen wholly out of his own observations on the strata of England; and I am not aware that he has attempted to found any general system of geology upon his discoveries. He has ascertained that certain shells are peculiar to certain strata*; and, with the help of this and some other principles equally original, he has succeeded in tracing the principal features of the structure of England; and by selecting a series of strata (many of them in other respects insignificant) he has been able to lay down on his Map the principal outlines of the geology of England and Wales.

If the results of Werner's researches had borne the least analogy to those of Mr. Smith, we might have supposed them to have been conducted on similar principles; but it is too evident that the Wernerians search only for evidence to support a favourite hypothesis, while Mr. Smith attempts to describe the real state of the earth's surface.

I am, sir, yours, &c. &c.

Jan. 14, 1818. THOMAS TREDGOLD.

VIII. *On the Question " Whether Music is necessary to the Orator—to what Extent, and how most readily attainable?"* *By* HENRY UPINGTON, *Esq.*

[Continued from Vol. L. p. 327.]

To Mr. Tilloch.

Blair's Hill, Cork, Dec. 17, 1817.

I PERCEIVE that my last letter, which treated of ancient music, was inserted in your Magazine for November; and in pursuance of my inquiry "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?" I have now to present you with some observations on our harmonical system. I cannot assure you that they are altogether new, for I am well aware that few ideas deserve the epithet "original;" yet, such as they are, I shall submit them to your consideration.

Having taken it for the present as granted, that the Greeks were truly correct in acknowledging the *fourth* as the only perfect concord †, my next inquiry was—Why that concord has been rejected by the moderns; while the subordinate fifth, and its inferior associates the thirds, have constituted (at the expense of perfection)

* See his "Strata identified," &c.

† The term "perfect concord" appertains to that note alone, which being struck in conjunction with any given fundamental and its upper octave shall constitute—not the most decided *jingle,* but the most intimate relation of both.

perfection) the basis of our harmonical edifice? I surveyed then our *common chord* with attention; and soon recollected an observation which I had somewhere read or heard of, viz. that *it* (our common chord, the basis of our system) is composed of an heterogeneous mixture, which passes for *proportion*, but which nothing short of custom could have reconciled to ears naturally chaste and susceptible of sublimity.

Whether, and how far, this charge can be established is the first object for discussion. Let us represent our chord major by the letters C E G c, and the respective relations of the whole will stand as follows:

E	is related to	C	as a	3d major.
G	.. ..	C	..	5th.
c	.. ..	C	..	8th.
G	.. ..	E	..	3d minor.
c	.. ..	E	..	6th minor.
c	.. ..	G	..	4th.

Thus it would appear that within our chord major are contained no less than six various relations—a 3d minor, 3d major, 4th, 5th, 6th minor, and an octave.

The same or nearly the same may be said of our chord minor. It consists of two *thirds* (both minor and major), a 4th, 5th, 6th major, and an octave;—and so far the accusation of "heterogeneous" is not without foundation. To this accusation, however, I cannot yield my unqualified assent, as will be shown in the sequel; and in the mean time, then, I shall proceed.

How happens it that the *fourth*, all perfect as it is, cannot find a situation within these chords?—Because the total derangement of our partial proportions must follow: insert it in its place, (between the major 3d and fifth,) and does it not become a semitone to the former, and a second to the latter?—discords too gross for even the Hottentot to suffer. View it between the minor 3d and 5th, and does it not become the second of both? Such is the fact.

But why should we not estimate a given fundamental, its 4th, 6th, and 8th, viz. C F A c as a perfect chord? Here lies one, and not the only defect of our harmony. We are dependent upon the extraneous circumstance of a contiguous *third* for the very existence of our key-note; as in the present case, where A the sixth becoming a third to F, we cannot force ourselves to acknowledge the fundamental C; sooner submitting even to F itself—or, what is tantamount, to the octave below it.

What shall now be said to the agency of *forte?* If F and A be struck with greater force than C, the matter is still more effectually decided—C is completely banished from its situation.

This proceeding indeed, I mean the operation of *forte*, will equally affect our common chord whether major or minor, and must prove to every dispassionate person that our tonical relations are in a considerable degree at the mercy of our forte and piano; —for if, in our chord major for instance, consisting of C E G c, we strike our 3d and 5th, viz. E and G, in the slightest degree more forcibly than C, all is chaos—our genera are confused, and C itself can with difficulty be acknowledged. This being an indisputable fact so constantly verified in our military bands, when the 3ds and 5ths are executed by French horns,—are we not warranted in supposing that first-rate ears, were they habituated not only to the most delicate shades of forte, but to the cognisance of tonical relations, in defiance of *noise*, must feel dissatisfaction even from the best possible execution of our common chord? They could not in all probability tolerate the overpowering of the 3rd, or more particularly the 5th, by the comparative noise of the fundamental; neither could they satisfactorily acknowledge, even with *equality* of forte, *that* fundamental as the PREDOMINANT. Guido himself was so sensible of this imperfection, that he banished the 5th from his counterpoint. —Has the Benedictine excelled us?

There is not perhaps any musical question which has excited from time to time more strenuous contest than the harmonical value of this very concord, all *moderns* insisting on its preeminence. Let us therefore investigate the source of that authority by which it is supported—the doctrine of *vibration*—whose principle, if I conceive the matter rightly, attaches the highest value to that string whose vibrations shall most frequently coincide with those of the unison or base.

That the periodical coalescence of any two or more given sounds in their passage to the ear should produce a greater volume or a greater degree of condensation, and consequently a greater degree of *loudness*, may be readily conceived. *Time* too, more or less perfect, may be comprehended by the comparatively regular or irregular movement of bodies: but time and forte are as remote from *tone*, which is the origin of concord and discord, as the most opposite things in nature. Experience indeed would seemingly withhold altogether from vibration—or, in more perspicuous language, from undulatory motion—the property of varied intonation; for undulation is but an ordinary effect, which sound in its passage through the air must necessarily produce—an effect too, whose circumstances every puff of wind must everlastingly confuse.

Whatever the nature or manner of transmission of those particles may be, which after being thrown off by the vibrating body arrive at the ear, has not been hitherto discovered;—and yet, that our organ of perception has been gifted by the Creator with the

the faculty of comparing, through the medium of the ear, the relative magnitude or number, or perhaps the peculiar arrangement of those particles, equally as the proportions of a picture through the medium of the eye, appears too reasonable to dispute. This is all we can offer upon the subject.

To obviate the allegation of partiality, I shall meet the advocates of vibration upon their own ground: but, acting much more fairly than they have done, I shall not content myself with the selection of some half-a-dozen of the most *favourable* numbers: on the contrary, I shall follow up their own plan of calculation throughout the scale, and present to you in regular order the result.

Dr. Rees, in his recent Encyclopædia, article *Concord*, has given us the modern table, from the octave to the minor 6th inclusive, rating the octave as 60; viz.

Octave	$= \frac{1}{2}$ string, value	60	coincidences.
5th	$= \frac{2}{3}$ ..	30	
4th	$= \frac{3}{4}$ ..	20	
Major 6th	$= \frac{3}{5}$ ..	20	
Major 3d	$= \frac{4}{5}$ ..	15	
Minor 3d	$= \frac{5}{6}$ ..	12	
Minor 6th	$= \frac{5}{8}$ ..	12	

Here the calculation ceases; and for the best of reasons—because it is fallacious. Let us take then *every* interval upon the scale; and reduce it to a common standard, by dividing the numerator into that number which represents the octave—such being the uniform process *.

Wherefore, supposing the	Octave	$= \frac{1}{2}$ string, value	1000 coindences.	
Then shall follow in regular gradation	5th	$= \frac{2}{3}$	500	
	4th	$= \frac{3}{4}$	333	
	Major 6th	$= \frac{3}{5}$	333	
	Major 3d	$= \frac{4}{5}$	250	
	Minor 3d (as on our public tables) ..	$= \frac{5}{6}$	200	All harmonical equivalents!
	Sharp (or Major) 4th	$= \frac{5}{7}$	200	
	Minor 6th	$= \frac{5}{8}$	200	
	Seventh	$= \frac{5}{9}$	200	
	Second	$= \frac{8}{9}$	125	
	Sharp (or Major) 7th	$= \frac{8}{15}$	125	
	Semitone	$= \frac{15}{16}$	66	
	Minor 3d (as really found on well-tempered pianos)	$= \frac{17}{20}$	58!!	

* As the denominators are supposed to have some trifling influence upon the value, the preference is given by the Doctor to the *lowest*.

I fear,

I fear, sir, that I am tediously minute; and yet I cannot dismiss the question without still further exposing its incongruity. In a word then—If the fraction 1-7th, 1-9th, or any other *discordant* fraction whose numerator is a unit, were submitted for examination by this hitherto infallible rule, must it not prove an equivalent, or nearly an equivalent, to the octave?

So much for the vibratory doctrine, which ever since the day of Galileo has been implicitly acknowledged. That this doctrine, however, is not only fallacious, in its application to musical intervals, but also calculated to mislead—and *that* too for the solitary purpose of exalting the *fifth,* in opposition to the unanimous declaration of ancient Greece, appears too evident indeed. But what more eligible doctrine shall we substitute in its stead? *That* of rational calculations—the pleasure of harmony as well as of simple melody being obviously derivable from certain though hitherto imperfectly defined proportions.

For the ascertainment then of the relations between tone and tone, I would thus proceed—considering the string like any other integer as the *root*.

First. I would lay down a series of numbers in duple progression, commencing with *unity*—as thus, 1. 2. 4. 8.

Secondly. I would take the lowest terms of the series; viz. 1 and 2; and representing the string by 2, I should be driven by necessity to acknowledge the unit as its most intimate relation. Hence the base or unison =2, and its octave =1.

Having thus obtained the numbers 2 and 1 as my extremes, I cannot find an intermediate integer; and must therefore resort to the second and third numbers of the series; viz. 2 and 4. Now, taking the number 4 as the base, and 2 as the octave, the third integer is wanted, which shall spring from these two conjointly, as from a common root. For the production of this new integer I am necessitated to add the numbers 4 and 2 together, and divide their product by the lower term 2, generating by this operation the required number 3, which is neither more nor less than the *mean* *.

Hence the Base = 4
Fourth = 3 *i. e.* a $\frac{3}{4}$ string.
Octave = 2.

A *fourth* integer between the base and octave is next required; which integer, and nothing but which, shall spring from the

* Not the common mean between two ordinary extremes, but that superior that *primary* mean which, springing from the purest geometrical source, becomes the origin of arithmetical progression.

Extend to infinity the series 1. 2. 4—3. 6. 12, or any other equivalent series, and the result is similar; for the addition of any two adjacent terms divided by the less, shall produce the original mean, viz. 3. Ex. gr. 6+12=18, which divide by 6, and the product is 3, as required.

the conjoint operation of the other three: but this being unattainable from any duple series commencing with *unity*, we must necessarily resort to the comparatively imperfect series of 3, 6, and 12, whose geometrical proportions, if the unit were prefixed, would be totally destroyed, the numbers 1. 3. 6. and 12 being in no acknowledged proportion at all.

We must submit then to this comparative imperfection; and changing the numbers 4, 3 and 2 into their representatives 12, 9, and 6, let us multiply the extremes together, and divide the product by the mean; as thus,

Base = 12
× by Octave = 6
÷ by Fourth = 9) 72 (8: which 8 is the *fourth* number required; and being equal to 2-3ds of the whole string 12, is equivalent to that interval in music called the *fifth*. The four numbers therefore obtained by these various procedures, are

	12 .	9 .	8 .	6
In music =	Base	4th.	5th.	8th.

Were it still further intended to create the number next in value to the 5th from our materials 12. 9. 8. 6 (12 and 6 for the harmonical creation of numbers being equivalent), we should, in my opinion, generate neither from 12 and 9, nor from 8 and 6*, whose proportions are as 4 to 3; while the simpler, and consequently more perfect *generative* proportions of 3 to 2 are to be found; viz. in 12 to 8, or in 9 to 6. Now, 12 and 6 being in this instance equal, our choice must rest between the numbers 9 and 8; to the *former* of which, viz. 9, as the better concord, we must give precedence;—and therefore the *minor* 6th which is equal to $7\frac{1}{2}$ † (the mean between 9 and 6) must be held, in harmonical value, as the successor of the *fifth*.

To retrace our subject:—The *octave* springs of necessity from *unity*, called in music the *fundamental*; from the fundamental and its octave in conjunction proceeds the *fourth*; from the conjoint operation of the fundamental, its octave and its fourth, proceeds the *fifth*; and from the octave and the fourth alone is generated the *minor* 6th.

These few and very simple operations are, in my mind, a sufficient guidance for the analysis of our musical numbers;—and hence therefore I shall venture, in my next letter, upon the formation of a *table* which shall comprehend every individual interval within our octave.

[To be continued.]

* Intervals approximating in value to our thirds and sixths may be generated between these,—but are not in *modern* use.

† That is $\frac{7\frac{1}{2}}{12}$ equal to $\frac{5}{8}$ of the whole string 12.

IX. *On*

IX. *On the Case of Miss* Margaret MacAvoy. *By* A Correspondent.

To Mr. Tilloch.

Sir,—The case of Miss MacAvoy has excited so much attention that I trust a few remarks upon Dr. Renwick's statement* will not be deemed incompatible with the subjects of your Magazine. I must candidly confess that I undertook the perusal of the above-mentioned work under the influence of strong prejudice; but I closed it with very different impressions: there are indeed some suspicious circumstances of sufficient weight to induce an impartial reader to suspend his assent till the case is established beyond the possibility of doubt; but I think the subject far too important to be treated with levity, and to call for a closer and more impartial investigation than appears yet to have taken place. The chief grounds of disbelief are founded upon the frequent suspension of her supposed powers, and consequent mistakes in ascertaining colours, printing, &c.: and one or two instances are mentioned with a candour highly creditable to Dr. Renwick (who appears to have no other view than the elucidation of truth), which certainly strongly savour of imposition; but these are counterbalanced by others of a still stronger description, in my opinion, in favour of the reality of the existence of these pretended powers. In the first place, there is no assignable cause for the deception practised; the health of the young woman is so deplorable and precarious, that worldly motives can scarcely be advanced; and pecuniary ones are, it is generally understood, equally out of the question, as she receives no remuneration, and is not in circumstances to require it. In addition to this, Dr. Renwick's character (for if there is imposition he must be a confederate) is at stake; he can gain little or nothing by inducing the world to believe, and he must lose every thing the moment the fraud is detected. Those who have read the book will, I think, be inclined not to judge too decisively of Miss MacAvoy; because with a pulse often at 160, and a habit of body reduced to the last degree, she occasionally declares that a power dependent upon strength of nerve suddenly leaves her, particularly if exposed to any agitating cause. But my object in troubling you with the annexed remarks, is not to defend either Dr. Renwick or Miss MacAvoy, neither of whom have I ever seen; but to offer some observations which appear to coincide with a theory I had previously formed respecting the extraordinary powers possessed by animals and insects, resulting I conceived from a

* For the particulars of this statement, see subsequent account of New Books.

high

high state of irritability in the nervous system; in consequence of which, so far from wondering at the publication of a case somewhat analogous in a human being, my astonishment has often been excited, that in the enlarged practice and advanced state of medical and anatomical knowledge, no instances have before been produced which could throw light upon so interesting a subject. Now, in reading Dr. Renwick's statement, it struck me that the same theory by which I had endeavoured to account for the powers of animals and insects might be applicable to the case in question; namely, the action or influence of caloric. We know that by the intervention of a prism the solar rays are divided into their calorific, colorific and deoxidizing proportions. It will be readily admitted that the calorific division would probably have the greatest influence upon nerves rendered unusually sensible by disease or other cause; and such in the case of Miss MacAvoy appears to be the fact. I proceed to an illustration:—Dr. Herschel's experiments prove that the most refrangible rays have the least heating power; and that the heating power gradually increases as its refrangibility diminishes; the violet ray has therefore the *smallest* heating power, and the red ray the *greatest*; accordingly the effect of the *red* ray ought to be much *stronger* upon an irritable nerve than the *violet* which *terminates* the calorific division. On referring to Dr. Renwick's account there is evidence that such is the fact. Page 59. July 25, "She told a *scarlet* coloured cloth upon the back of her hand uncovered, but she could not tell any *other colour* given her." Page 61, The *red* and *orange* rays of the solar spectrum being thrown by a prism upon her hand, she said it appeared like *gold*. She felt the spectrum *warm*. The *violet* rays were the least pleasant. Page 109, "I prefer the *brighter* colours as they give a *pleasurable* feeling, a sort of glow to my fingers, and indeed all through me. *Black* gives me rather a *shuddering* feeling." Again: we know that metals are the best conductors of caloric, but in very different proportions; according to Dr. Ingenhousz, they are classed in the following order:—Silver, gold; tin, copper, nearly equal. Platinum, iron, steel, lead, all much inferior to the others. If a more rapid action of caloric can therefore be supposed to have a more sensible effect than a slower, Miss MacAvoy ought in the above order to be susceptible to metallic impressions, and we find she is so most accurately. P. 63, "The *silver* of a watch-case felt *finer* than the *gold* of the seal; but *gold* and *silver* had a much *finer* feel than *steel* or brass." In p. 109, her reply to the question, What is the feeling you have of different metals? she repeats the remark: "I feel *gold* and *silver* to be more pleasant than brass, copper, or *steel*." I have already referred

ferred to p. 62, wherein she compares the *red* and *orange* rays to *gold*.

Again: with respect to fluids, it is a known law that the nearer a liquid is to the temperature at which it boils, the greater is the expansion produced by the addition of a degree of caloric. In conformity with which law she ought to be more alive to the impressions received from alcohol which boils at 176, than to water whose boiling temperature is 212, and the proof is equally satisfactory; for, in answer to the question, How do you know the difference between water and spirits of wine, she replies " By the spirits of wine feeling *warmer* than the water." p. 110.

I shall conclude these remarks with one observation more respecting glass. She is asked, p. 110, " Do you feel the colours equally well if two glasses are placed before the object?" She says, " If these glasses are very close to each other, I feel the colour, but it appears more faint; but if they are placed at a distance from each other, I do not feel the object." This is, I conceive, what might have been expected from the nature of glass, which is known to be a bad conductor, though as a recipient medium for the rays of light it is essential in other experiments.

Such, sir, are the remarks I beg to offer upon this extraordinary case:—separately considered they may be of no great weight, but collectively they afford at least a curious coincidence; and the further I have attempted to follow them up, the more satisfactory has been the result. This theory accounts for the curious fact stated p. 83: Her powers appear to be evanescent, and the caloric has not at all times its occasional influence upon the nerves. Accordingly, in feeling the colour possessed of the smaller quantity—*blue*—it ceases to excite, or but feebly. The colour therefore under inspection would appear to her to be immediately changed to black. What are Dr. Renwick's words? " The last colour she told was *blue*, and in an instant it was *black*, and then the power is gone!"

If the coincidence be not purely casual, and the conclusions above mentioned not erroneous, it would be desirable to try a series of prismatic experiments by the light of a full moon. For the lunar spectrum being devoid of caloric, it might be presumed she would feel more difficulty in discovering its colours than when submitted to the solar rays. It might also be satisfactory to know what impressions she received from the action of the solar prismatic rays extending beyond the spectrum which are purely *calorific* and colourless. I have been the more anxious to transmit these remarks to the public through the medium of your Magazine, as I trust they may induce more scientific observers to suggest experiments of a more decisive nature. I am aware that

that in the metropolis the whole is considered as an imposition, and passed over in consequence as unworthy of notice. But surely as long as a *possibility* exists of its not being a fraud; no investigation which science can afford ought to be omitted in a case which, *if true*, will throw an important light upon some phænomena which in our present state of knowledge are deservedly ranked amongst the most interesting and curious in the œconomy of nature.

Your obedient servant,

E. S.

X. *Account by M. Chev.* Dupin, *Corresponding Member of the French Institute, &c. of an Aurora Borealis observed at Glasgow the* 19*th of September, contained in a Letter to M.* Arago. *Communicated to the Academy of Sciences of the Institute of France, the* 29*th of Sept.* 1817.

Glasgow, Sept. 20, 1817.

Yesterday, the 19th of September, a little before nine o'clock at night, I went to visit the Observatory of Glasgow, which is built on the summit of a hill to the north-west of the city. I was accompanied by Dr. Ure, a gentleman of scientific celebrity, who has principally contributed by his zeal and his cares to the formation of a Society, whose voluntary subscriptions have defrayed the whole expense of erecting the edifice and purchasing the instruments, which are numerous and very excellent.

The night was fine, the moon and the stars were of a sparkling brilliancy. When we arrived at the Observatory the heavens towards the north exhibited some whitish shootings, which becoming less and less uncertain, soon displayed the appearance of an *aurora borealis*. We then ascended to the terrace above the Observatory, in order to embrace at one view all the luminous parts. In enjoying for the first time this imposing spectacle, I experienced an astonishment and pleasure which I cannot express to you; nor can I sufficiently prize my good fortune in seeing one of the most beautiful phænomena of which this country can boast.

The light of the *aurora borealis* extended from the north in a space terminated by a vertical circle, the plane of which was nearly perpendicular to the direction of the magnetic needle. The zenith was the part least luminous—it seemed a centre from which the streamers emanated, and which as they developed themselves became more and more brilliant in proportion as they approached the horizon. However, they never descended that length, but terminated irregularly at fifteen or twenty degrees above it, presenting an angulous contour like those glories with which painters environ the throne of the divinity.

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The circumstances which struck me as most remarkable were the play of the rays and their luminous undulations. These rays were projected in large groups which alternately approached and receded from each other: at one time they seemed to rise in a body, in the sheaf-like form of an immense rocket (*comme des gerbes d'artifice*), and at another time to descend like a shower of light. Independently of these general movements, each cluster of rays presented a lateral movement, which could be distinguished by the greater or less intensity of parallel rays: the parts more or less luminous moved in lines paralle lto each other, like regular waves; and what was very remarkable, there often appeared in the same cluster two opposite undulatory movements, in such a manner that the masses of light and shade moved regularly in opposite directions one above the other without being in the least confounded.

The light was generally of a silvery white, or rather of a light-orange hue:—the lower extremity of the clusters, however, sometimes emitted the prismatic colours, as red, yellow, and blue; at one instant a slight green tint diffused itself over one of the clusters.—These are all the observations I was able to make as to the colours of the *aurora borealis*.

During the first twenty minutes the appearances of the *aurora* were faint; during the ensuing twenty-five minutes they were brilliant and continually varying; afterwards the light gradually vanished. The longitudinal projection of the streamers and their lateral movements first ceased, and then the rectilineal figure of the rays became effaced. Nothing now remained in the heavens but a pale glimmering light like to that of the Milky Way, its point terminating in a grand circular arch, concave towards the earth. This light continued visible for about an hour after the *aurora*.

The sky did not seem so transparent as to entitle us to suppose that it was not charged with any vapour; but there were no clouds visible, one small one only excepted, which appeared in the hemisphere occupied by the *aurora* like a sort of rock, against which the streamers played, indenting its edges but slightly. In the intervals between the streamers the stars were distinctly perceivable, even at the time when the *aurora* was most brilliant; but under the streamers as far as the horizon the heavens were blackish, and the stars were not seen without difficulty.

XI. *Notices respecting New Books.*

A Narrative of the Case of Miss MARGARET MACAVOY; *with an Account of some Optical Experiments connected with it. By* THOMAS RENWICK, *M.D. Physician to the Liverpool Infirmary.* 4to. pp. 111.

Hints to Credulity; or, An Examination of the Pretensions of Miss M. MACAVOY, *occasioned by Dr.* RENWICK's *Narrative of her Case. By* JOSEPH SANDARS. p. 69.

THE subject of this case is stated by Dr. Renwick to have been born on the 28th June 1800. A hooping cough with which she was attacked when an infant, was succeeded by an affection of the eyes which severely afflicted her, and was attended with a very considerable and constant discharge of a bloody watery fluid. Her friends despaired of saving the eyes, but no professional means were employed for some time; the disease gradually increased, and the exposure to light caused insupportable pain. When the eyelids were raised up, the eyeballs appeared as one mass of blood. A friend recommended Johnston's golden ointment, which was made use of with great benefit; and in a short time the pain and irritability of the eyes were entirely removed. She now gradually recovered her sight; perfectly with the right eye, but the left was so weak she could scarcely open it, and saw every thing as it were through a mist.

From this period until June 1815, she continued in a very variable and infirm state of health. On the 4th of that month she is said to have been visited for the first time by Dr. Renwick. He found her so much affected with giddiness as to stagger in walking across the room. With the left eye she could scarcely distinguish any object, and with the right for several days previous to his visit every object at a distance appeared white; and those which were near double. On the 7th of June she could not distinguish any object whatever, and appeared totally blind. The pupils were considerably dilated, and did not contract upon the application of strong light.

The succeeding passages of Dr. Renwick's observations we shall give in his own words:

"August 2. Mr. Thomas, surgeon, and I particularly examined her eyes exposed to the light of a candle as near to the eye as possible without burning her, but without observing the slightest contraction or dilatation of the pupil, or the least sensibility in the eye. I have frequently since this period thrown my hand suddenly towards her face; have pretended to dash a pointed penknife at the eye; and have often applied the point of the finger in a quiet and steady manner as near as possible to

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the pupil, without observing the slightest sensation in the eye. Mr. Thomas assured me, he has more than once put his finger on the cornea itself, which then appeared insensible; but when he touched the eyelid or eyelash, she was instantly sensible of it. I have sat for a considerable time attending to her sewing, but apparently indifferent about it; and during these visits I have examined her every action as minutely as possible, and I have been satisfied she could not see.

"August 31. I was induced to visit her again with Mr. Thomas (from hearing from her step-father, Mr. Hughes, that she had recently found herself possessed of certain powers of an extraordinary nature), and took considerable pains in examining the eyes; but we found little or no alteration in their general appearance, except that the pupil was not quite so much dilated as before; but the light of a candle appeared to have no influence upon it. We found her father's account very accurate, and that she really could read by the application of the finger to the letters with considerable fluency. As it was probable any other person who had not the same opportunity of judging with Mr. Thomas and me, might think it possible she could see, I thought it right to bind something over the eyes, and I made use of a Manchester cotton shawl which went twice round the head, crossed at the eyes, and was tied at the back of the head as firmly as she could bear it. I placed in her hand a number of a folio bible, and she read very correctly one verse of a chapter in Genesis. I then requested to have another book, which happened, to be a volume of the Annals of the Church. I opened it, and she read to me several lines, with the alteration in a proper name of only one letter, which upon being desired to read over again she corrected. I then turned to a few lines of errata, and she read them correctly, only reading the letter *l* as an *i* and a dot. The mode she follows, is to place her fingers upon the book, and, when she feels the letters, to proceed from the beginning to the extremity of the word, and back again, until she names it; and so on, to the next word. She often makes use of the fingers of both hands, particularly the fore-fingers, and when they are in good order, she will read from twenty-five to thirty words in half a minute.

"On the following day, I mentioned the circumstance to a friend who was anxious to see a phænomenon of this kind, and he met me in St. Paul's square. Miss MacAvoy again read over to us a verse in the Bible; a few lines in the Annals of the Church, and the title-page, mottoes, and several lines in a 12mo edition of Grahames Sabbath. I placed her fingers upon a blank leaf, and desired her to read. The attempt was made, but she said she could not feel any letters. Her fingers were then

then placed upon another leaf, which she declared was also blank. I then desired her to feel the upper part of the leaf. She did so, and said she felt something, but it was so confused she could not make out what it was. The fact was, a lady's name had been written in the book, and when I took it from my library, I scratched the name out with a pen, so that it was not distinguishable to the eye."—

"She distinguished the different colours of silk, of cotton, or of wool; and the brighter and more vivid the colour, so much greater is the pleasurable sensation it affords. If they consist of many colours in the same piece, she will point out each colour, and trace the line where it terminates. If the silks are of that kind called shot-silks, she will tell the colour of the ground as well as the intermixture. If the different pieces are besmeared with oil, or any greasy substance, she cannot so easily distinguish the colour; but if it is nearly faded, she will point out where it is faded, or where it is bright. She can distinguish the colours of the paintings of enamelled or varnished boxes, will trace the outline of the figures, and will very generally state the subject of the painting with a degree of accuracy which is surprising At times, however, this feeling is suddenly lost; and after describing colours, reading, &c. with great nicety, she will declare she cannot tell the colour, or will say it is black. When this is the case, the fingers become extremely cold, and the power will often return as the fingers become warmer."—

"January 17, 1817 On this day she not only declared the colour of different cloths, cotton and silk, but several pieces of silk which were inclosed in a small phial bottle; she traced with her finger the edge of each; and when another phial was given, which did not contain any thing, she declared it to be empty. Bottles of white glass, holding magnesia, red precipitate, red oxide of mercury, and bark, were separately given to her, and she accurately named the colour, and told how high the bottle was filled with each substance:—two small bottles, the one containing water and the other spirits of wine, were placed in her hand;—she said the first was colourless like water, and the second was similar in colour, but had a different feel, being much warmer. * * * * * Soon after this period the Rev. Edward Glover asked her if she could tell the time of the day by feeling the surface of the glass covering the dial plate of a watch. Her answer was, she had never tried it. A watch was given into her hands; she felt the surface of the glass, and very soon named the hour. She was afterwards so exact, that she not only named the hour, but the number of minutes the minute-hand had passed the hour. Once I gave her my watch when the hour was twelve o'clock. She mentioned the hour, but observed there was only one hand; the minute-hand being exactly over the hour-hand. Mr. Glover assured me that for several days he was afraid of speaking of the circumstance, lest he should be laughed at; but she so often repeated this experiment in his presence, and in that of other persons, that he made no hesitation in mentioning it. She told the colour of the different hands, whether they were of gold or steel. She distinguished a gold from a silver watch. Brass and copper were also submitted to her touch, and she immediately discovered the one from the other. The colours of various seals and stones, whether mixed or not, were correctly named; as well as those of gems; but it was only the colour and not the nature of the stone, unless she had before been acquainted with it. The eyes were covered with gogglers*."

Various other experiments are stated by Dr. Renwick; but we cannot gather from them any thing new in addition to the preceding observations, unless indeed we may except the following, which is certainly novel enough.

"*Exp.* xix. With her hands upon the window perceived two newly-cut stones of a yellow colour, lying one on the other against a wall on the other side of the street; distance about twelve yards: also a heap of cast-iron railing piled upon each other. One of the company being dispatched to place himself upon the ground, stones, rails, &c. she mentioned whenever he moved his position; perceived him jump off the railing; mentioned the colours of his dress correctly, only said that a plum-coloured coat was black;—mentioned two children accidentally passing by at the time. She said, they appear very small indeed; the person who was sent appeared about two feet high when at the distance of twelve yards; as he came nearer, she observed that she felt him grow bigger. All objects appear as if painted on the glass."

Dr. Renwick has added to his Narrative, some "Remarks upon Miss MacAvoy's Case;" but they are wholly of a medical nature, relating to certain convulsive affections to which the young lady is subject. "The peculiar power," says the author, "which she appears to possess of distinguishing colours, reading, &c. with her fingers, are of so extraordinary a nature, as with our present information to preclude all reasoning upon the subject." In this we differ from the author. The powers ascribed to Miss MacAvoy are to be sure extraordinary enough; but, as may be seen from the paper of an ingenious correspondent in a preceding part of this number, there is much less difficulty in

* These gogglers are described as forming a complete mask coveriug the whole of the face, except the nostrils and mouth. They were tied by several pieces of tape at the back of the head, in almost every direction, and two pieces of tape crossed each other under the nose, just below where a line of cotton-wool was sewed in, ao as to prevent any ray of light from passing upwards.

reasoning

reasoning about them than in ascertaining whether such powers really exist at all. Indeed Dr. R. himself elsewhere informs us that, but "for the fact of blindness *not being unanimously agreed upon*, he would have endeavoured to have given an explanation of some of the phænomena detailed above."

The chief antagonist of the reality of the powers ascribed to Miss MacAvoy is a Mr. Sandars, a merchant of Liverpool, the title of whose pamphlet on the subject is also prefixed to this notice. Some facts contained in this pamphlet are certainly of a very stubborn description; and until contradicted or explained, which the parties whose credibility is attacked will certainly feel it their duty to do, if it is in their power, the case of Miss MacAvoy must, notwithstanding all that has been said and believed about it, be considered as only one chapter more added to the apocrypha of Philosophy. The spirit in which these "Hints to Credulity" are written we are far from approving,—as, relating to the individuals concerned, they are acrimonious, and, with a view to the interests of science, more remarkable for their egotism than philosophical acumen. But at the same time there is a veritableness and an explicitness about the matter of fact details contained in them, which entitles them to every attention. A few of the more prominent passages we shall subjoin, and leave the case for the present in that state, in which it can only be allowed to remain, if these passages are true both in point of fact, and of inference.

"*Particulars of what passed on the* 13*th October.*

"A number of experiments were tried on Miss M'Avoy, in some of which she w s successful, in others quite the reverse. She had two glass phials put into her hands, in one of which was water, and in the other spirits of wine; she named each very accurately, which Dr. R. regarded as a very extraordinary circumstance; but upon examination, it appeared, that although the phials might be of equal weight, they were not of equal size, one being much longer than the other, and the corks were not (as they ought to have been) sealed with wax; indeed they had not the least covering over them, to prevent any one, who had a peculiarly fine sense of smelling, from ascertaining the difference. A gentleman present gave into Miss M'Avoy's hands a seal, and requested her to tell him the colour of it; he placed his hand between the seal and her face, and she declared the seal to be black, whereas it was white. At the close of the experiments, another person present expressed an opinion that Miss M'Avoy could see; upon which Dr. R. proposed that the gold-beater's skin and the adhesive plaster should be applied to that person's eyes, which was assented to, and uncommon pains were taken by Dr. R. in placing them, so as to prevent all possibility of seeing. When fixing them, Dr. R. said, "Now close your eyelids," to which an answer was returned by some one, "You were not so particular with Miss M'Avoy." However, the request was complied with, and when the bandages were properly placed, a watch was presented, and *the hour was immediately pronounced*; a glove was given, *and the colour was told*; a letter was produced, *which was read with great facility*, although the writing was very small."—

"The coming and going of the power is a most convenient resource. As Sancho says of sleep, it covers her over "like a cloak." Whenever any unusual means are adopted to blindfold her, she becomes agitated, she cannot proceed—the power goes. Her medical friends all agree in stating her to possess excessive sensibility; but may not the dread of *detection* produce this agitation? It is surely as reasonable, (though not perhaps so courteous) to impute it to this, as to a high indignant feeling of suspected integrity. At other times, *without* being agitated, she loses the power; every attention is paid to her, bandages are loosened, and coverings removed;—after a short time experiments are resumed, and she succeeds:—Is it uncharitable to say, that in the one case she was completely blindfolded, that in the other she was not? If, when she declared she always found it necessary that the breath of her nostrils should fall upon the object presented, she had stopped short at telling colours and reading with her fingers, she would have been less vulnerable; but suddenly she evinced new powers, to which her breath could not by possibility be necessary: she began to distinguish objects which she could neither touch nor breathe upon—she could not touch the dial-plate of a watch, yet she told the hour by feeling at the glass; she began to name colours placed behind her back: of what use then was her breath?—It could not in either case be of the least importance to the developement of these powers. When by putting out her feelers, (just as a snail protrudes its horns on issuing from its shell,) she distinguished through a window, and at the distance of twenty or thirty yards, men labouring at their vocation, the absurdity of the thing could have no parallel but in the attempt to explain it by the intervention and influence of the breath. This declaration regarding her breath is most important, and gives the key to the whole mystery.

"Having shown that her breath is not *always* necessary to her performances, I shall proceed to show that this is not the only instance in which she proves too much. Her followers say that her eyes are blind, and that she reads by the agency of her fingers! What is the nature of this agency? If it be the sense of touch, she would be able to read in the dark as well as in the light; but on one occasion, when Dr. Traill proposed that the room should be be darkened, he was asked whether, as *he* could not read in the dark, it was reasonable to expect a blind person to do it? Admitting that her fingers really can see, light is declared to be necessary; but what is the process of her reading? Her fingers come in *close contact* with the words; she feels them, and most effectually *obstructs* the light. If instead of touching the words, she held her fingers at a sufficient distance to *admit* the light, then the theory would be supported by *one* of the laws of vision; here again "the good Lady promises too much."—

"In October 1816, Mr. Bradbury, author of Travels in America, accompanied by a friend, visited Miss M'Avoy. His friend had seen this young lady several times before, and from witnessing some experiments that were tried upon her, was of opinion that she could see; but being anxious that Mr. Bradbury should also obsesve her, he prevailed upon him to visit her. After a few experiments, Mr. B. was so well satisfied that Miss M'Avoy could see *with her eyes*, that he ceased examining her further, and seated himself by the fire to wait until his friend was ready to depart. His friend made a few more efforts, to be convinced whether the opinion he had previously entertained was well or ill founded, and proposed, as a final test of her powers, that she should determine, with her hand behind her back, the colour of a piece of cloth which he brought with him for that purpose.

"After feeling it for a short period, and being informed that she was mistaken in the colour that she guessed it to be, Miss M'Avoy requested permission to use her other hand, which of course was instantly acceded to. After some time had elapsed, in which she was unsuccessful in her efforts to determine the colour, her mother, who was in the room, and had been engaged in affixing leather to the goggles, handed them over to Mr. B.'s friend, and asked his opinion, whether it was possible, in their amended state, for a person to see with them? He unwarily put them on him for a few moments, to ascertain the point demanded, and when he took them off, he was surprised to find that Miss M'Avoy could *then* state correctly tbe colour of the cloth. He shortly after took leave of her, as he was extremely desirous of learning from Mr. B. what had transpired, daring the time he had incautiously put on the goggles. Mr. B. was equally anxious to communicate what he had observed during that period, in which, although apparently heedless of what was passing, he was an attentive observer. Mr. B. then related, that as soon as his friend had put on the goggles, he saw Miss M'Avoy rapidly glance at himself and his friend; and acting as from an apparent conviction of not being observed, she drew the cloth from behind her back, gazed at it quickly, and restored it to its former situation, before the goggles were removed from his friend's eyes.

 This

This fact Mr. Bradbury has related to several respectable gentlemen, who are fully satisfied of his candour and integrity."—

"Our experiments were now drawing to a close, when Mr. Steele, from London, entered the room, with a new kind of apparatus, which appeared to possess all the necessary powers of interrupting sight, and yet admitting the needful communication of the fingers with the month, which was declared indispensable, for the purpose of breathing on them occasionally when touching the object the colour of which was to be identified. This apparatus was nothing more than a small sheet of pasteboard, out of one side of which a circular piece was cut, so as to admit her neck. Being placed under her chin, the goggles were considered unnecessary, aud were removed; as there could be no doubt of her possessing the faculty of seeing with her fingers, if with *this projecting* appendage she could describe objects, and tell the colours of them when placed beneath it. It was not our business to determine if the Lady could see with her eyes; our doubts only related to her possessing that faculty with her hands. I think the first trial we made, under this new arrangement, was with a watch. Now it happened that two gentlemen present had gold watches, very nearly alike in size and fashion. One of these was held carelessly, though designedly, at such a distance from her face with the dial upwards, as to be seen, if she had the power and inclination to look at it. A few minutes elapsed in conversation, when it was proposed to put a watch into her hands, beneath the pasteboard; and taking the one just alluded to for that purpose, I professed to give it her, but in fact put *another* watch into her hands, the fingers of which had been privately altered *one hour* forward. She drew her fingers over the glass, and declared the time to a minute; but observe, it was not the time of the watch she held in *her* hand, but the time of the watch *I held concealed in mine*, the one which had previously been placed carelessly at a distance with the dial upwards. This artifice she was not made acquainted with; and an exclamation of "*astonishing*" being uttered by some one of the party, she concluded that every thing so far was right."

Mr. Daniel Dowling, Master of the Classical, Commercial, and Mathematical School, Mansion House, Highgate, has just published "A Key to the latest Edition of Dr. Hutton's excellent Course of Mathematics." The Course is in three volumes octavo, and the Key in one handsome volume to correspond. The lucid manner in which Mr. Dowling has treated the several mathematical and philosophical subjects in the Course, demonstrated the various propositions, and investigated the theorems, not only evinces that he possesses a thorough knowledge of those branches, but

but exhibits him as a man well calculated for the profession he has chosen, since no requisite is more essential in the formation of a school-master, than the art of conveying instruction. The work is illustrated by 100 diagrams neatly cut on wood. In a word, we are of opinion, that this book will be found to be what Mr. Dowling suggests in his Preface, "an acquisition to every Class of Mathematical Students, and to Officers of the Army, Navy, and Honourable Company's service."

Mr. Robert MacWilliam, architect, has in the press "An Essay on the Origin and Operation of the Dry-Rot; in which the source of the disease is investigated, with a view to establish the modes of prevention and cure on rational principles. It will make a quarto volume, illustrated with plates; and to it will be annexed suggestions on the cultivation of forest trees, with abstracts of the forest laws from the earliest times.

Dr. Kitchener has just published a Third Edition of his useful little work entitled "Practical Observations on Telescopes, Opera Glasses, and Spectacles." We gave a short notice of this work on its first appearance in our Number for December 1814.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On the 20th of November the Croonian Lecture was read by Sir Everard Home, describing the changes which blood undergoes in the act of coagulation. It resulted from some microscopic experiments, in which Sir Everard was assisted by Mr. Bauer, that 2,560,000 globules of human blood, when enveloped in their colouring matter (which he conceives to be something superadded to their proper substance) would be required to cover a square inch. He supposes that the globules possess a regularly organized structure: they were observed to range themselves in lines, which when examined and compared with the muscular fibre, under high magnifying powers, led to the conclusion that these particles are the constituents of the fibre. The blood in coagulating assumes a tubular texture, an effect produced by the extrication of gas during the coagulation.

Nov. 27. A paper by Mr. Seppings on the strength given to ships by diagonal braces was read, and claiming for the author the originality of the invention.

Dec. 1. The election of Office-bearers took place.—(See our Number for December.)

Dec. 11. A Memoir by Captain Burney was read, On the Geography

graphy of the North Eastern part of Asia, examining the question whether Asia and America are united? which he concludes in the affirmative: of course that no north-west passage, so often sought for, exists.

Dec. 18. A paper by James Smithson, Esq. was read, on the Colouring Properties of certain matters. Among the substances examined were litmus, the colouring matter of violet, the blue paper employed to cover loaf-sugar, the blue of hyacinth, the mulberry, the corn-poppy, and sap green. Mr. Smithson thinks it probable that some vegetable colours may be produced by combining their principles.

A paper by Mr. John Davy was read the same evening, giving an account of some observations made on Adam's Peak, in the Island of Ceylon. The print of a foot of immense size which is shown on the top of this mountain as that of Adam, who is fabled to have fallen on this island when cast down from Paradise, Dr. Davy believes to be an artificial formation designed by priestcraft to impose on the vulgar. The mountain is above 6000 feet high, is composed of gneiss, and contains feltspar and garnets.

ROYAL SOCIETY OF EDINBURGH.

On November 17th, the first meeting of the Society for the season, a paper on Muriatic Acid, by Dr. Ure of Glasgow, was read. After giving a condensed view of the chlorine controversy, he proceeded to detail a series of experiments which he had executed last summer, for the purpose of deciding the opinions of chemists on this fundamental doctrine. Having proved the composition of dry sal ammoniac, in whatever way prepared, to be identical, and to be definitely fixed, by the concurrence of his own experimental results with those of M. Gay Lussac, at 32·3 ammonia + 67·7 muriatic acid gas; he exposed thin laminæ of the pure metals, silver, copper, and iron, ignited in tubes of green glass out of contact of the air, to the action of the vapour of the above salt in a state of complete siccity, and found in each case, the metal converted into a muriate, whilst a portion of water, nearly equal to one-sixth of the weight of the dry salt made its appearance. Thus by the formation of muriates which require no water of composition, Dr. Ure succeeded in liberating the water of composition of muriatic acid gas and muriate of ammonia.

The principle of his experiments is therefore totally different from that of Dr. Murray's, recorded in his System of Chemistry, and so keenly censured by Berzelius in his *comparison of the old and new theory*. "I combined," says Dr. Murray, "over dry mercury, dry ammoniacal gas, with muriatic acid gas; and having collected the salt formed, I exposed it to heat in a retort, and found water to be expelled, and condensed in the neck of the

the retort. The whole quantity from the details of the experiment, I found reason to conclude, amounted to about one-sixth nearly, of the weight of the acid *." In direct opposition to this statement, Dr. Ure found it impossible to obtain water by merely heating, or passing over heated charcoal or quartz, the salt resulting from the union of the acid and ammoniacal gases, when they were properly dried beforehand, as they ought to be; thus confirming the fidelity of Sir H. and Dr. Davy's experiments in this particular, and the incorrectness of Dr. Murray's. Dr. Ure infers from his experiments, that muriatic acid gas is composed of an atom of dry acid, combined with an atom of water, like the vapours of nitric and sulphuric acids; and consequently that chlorine is oxygenated muriatic acid. To his paper is subjoined the description and drawing of a new exploding Eudiometer, which he employed for analysing the gaseous products in the above series of experiments; and which uniting simplicity, safety, and precision, it is hoped, may prove generally useful to chemists, in pneumatic analyses by explosion †.

An account of Dr. Ure's experiments was transmitted to Dr. Wollaston on the 16th September, an abstract of them was published in The Philosophical Magazine on the 1st October, and the paper itself was left in Dr. Murray's hands on October 25th. It having been suggested by some members of the Society, after the reading of the paper, that it would be desirable, if Dr. Ure would repeat his experiments, with the salt resulting from gaseous combination, he accordingly did so; and transmitted an account of his successful results to Dr. Murray, on the 26th November. The second part of Dr. Ure's paper, containing the supplementary verifications, was intrusted to Dr. Brewster on December 6th, and by him immediately presented to the Secretary of the Society. This part contains a new and striking demonstration of the identity of the two differently prepared muriates of ammonia; only *that* which results from gaseous combination, being more powerfully absorbent of moisture, from its finer state of comminution, is liable to greater fallacy, and less fitted for the above experiments than common sal ammoniac recently heated for some time to near its subliming temperature.

By transmitting dry muriatic acid gas over the ignited metals, Dr. Ure obtained likewise water, corresponding in quantity to the metallic muriate formed. The muriate of iron thus obtained, seems peculiar. It is in small white scales, of a micaceous lustre, and appears to contain a smaller proportion of iron, and that in a lower state of oxidizement than the common green muriate. It is also demonstrated that the water formerly obtained by Dr.

* System of Chemistry, edit. 1812, vol. ii. p. 642.

† One of these instruments was exhibited to the Society.

Murray

Murray was the *hygrometric* and not *combined* water of sal ammoniac; for, as the whole ponderable matter of the two constituent gases is condensed into the salt, whatever water of composition muriatic acid gas may be supposed to contain, has become an indispensable constituent of the solid; and can no more be separated by mere heat than the combined water of concentrated oil of vitriol. Nay, supposing that by intense ignition we should resolve the salt into its ultimate elementary gases, still the water would remain latent in the resulting muriatic acid gas, as it did before the union, even according to Dr. Murray's own views.

Dr. Ure is still inclined to consider his original experiments on dry sal ammoniac, of which a notice was published in this Magazine four months ago, as most decisive.

Jan. 12, 1818. The continuation of Dr. Murray's Paper on Muriatic Acid was read. In the preceding part of it the results of experiments had been stated, whence it appeared that from the action of metals on muriatic acid gas water is deposited This is a result obviously incompatible with the doctrine in which chlorine is considered as a simple substance, since, according to that doctrine, muriatic acid gas is the real acid, altogether free from water. As the opposite doctrine holds the existence of combined water in the gas to the amount of a fourth of its weight, a portion of this may be supposed to be liberated by the action of the metal. A difficulty however presents itself even on this view of the subject. The action consists in the acid enabling the metal to decompose the water and combine with its oxygen; with the oxide thus formed the acid unites, and no water remains to be deposited, since none is liberated from its combination with the acid, but what is spent in the oxidation of the metal. The products therefore ought to be the same on this hypothesis as on the other, namely, a dry muriate or chloride, and hydrogen gas.

It was shown that the water obtained in the experiments could not be derived from hygrometric vapour; that it could not be accounted for from the supposition of a portion of water being combined with the acid in the gas beyond that which is strictly essential to its constitution;—and that it could not be ascribed to any lower degree of oxidation of the metal being established. One explanation remained, that it might arise from the formation of a super-muriate, the quantity of water combined with the quantity of acid, which forms a neutral muriate, being sufficient for the oxidation of the metal; so that if an additional portion of acid entered into the combination, the water of this might be liberated. It was accordingly found that the products in all these cases were sensibly acid, and this even when any source of fallacy, from a subversion of the combination by the agency of water, was obviated. In the sequel another explanation was sug-

gested

gested on a different view of the subject, if this should not be considered as sufficient.

Dr. Murray considered the results of these experiments as confirming, in addition to what he had before done, the fallacy of the opinion in which chlorine is regarded as a simple substance, which, with hydrogen, forms muriatic acid. The opposite opinion, that it is a compound of muriatic acid with oxygen, and that muriatic gas is a compound of muriatic acid and water, might be held to be established; and it undoubtedly may be maintained. But he has presented a different view of the subject, as being more conformable to the present state of chemical theory.

The progress of chemical discovery has shown that oxygen cannot be regarded as exclusively the principle which communicates acidity. The same property is in different cases communicated by hydrogen; and this fact he regards as affording the only argument of any weight in support of the new theory of chlorine.

When water is obtained from muriatic acid gas, it does not necessarily follow that it has preexisted in the state of water. It is equally possible, *à priori*, that the elements of water may have existed in the gas. On this view oxymuriatic acid will be a binary compound of a radical at present unknown with oxygen, and muriatic acid a ternary compound of the same radical with oxygen and hydrogen. And when muriatic acid gas is formed from the mutual action of oxymuriatic gas and hydrogen, it is simply from the hydrogen entering into the combination. In the processes by which water is obtained from it, the water is formed by its hydrogen and part of its oxygen entering into union. The same view he extends to the other acids which have been supposed to contain combined water. Sulphurous acid is the proper binary compound of sulphur and oxygen; sulphuric acid is a ternary compound of sulphur, oxygen, and hydrogen; and nitric acid is a ternary compound of nitrogen, oxygen, and hydrogen.

While each of these elements, oxygen and hydrogen, communicates acidity, their combined action seems to do so in a still higher degree. Sulphur with hydrogen forms a weak acid;—with oxygen another acid somewhat stronger;—with oxygen and hydrogen one of still greater power. Nitrogen with hydrogen forms a compound having no acidity; with oxygen in two proportions it forms oxides; with oxygen and hydrogen a powerful acid. Carbon with hydrogen forms compounds which are not acid; with oxygen in one proportion it forms an oxide, in another a weak acid; with oxygen and hydrogen the different vegetable acids which are of much superior strength.

This explains the apparent anomaly which appeared in the old doctrine

doctrine with regard to oxymuriatic acid, that it is a weaker acid than the muriatic, though it has received an additional portion of oxygen. It is so precisely as sulphurous acid is weaker than sulphuric. The proper points of resemblance are the sulphurous acid with the oxymuriatic, and the sulphuric with the muriatic. It was shown that oxymuriatic acid has a stricter analogy to sulphurous acid than to any other body; and that any deviation from this analogy arises from the large proportion of oxygen which the former contains.

The relations of iodine, the analogy of which in some respects to those of chlorine has chiefly given predominance to the new doctrine, with regard to the latter accords perfectly with these views. The nature of the compounds of inflammable bodies with chlorine accords also better with them than with either of the other doctrines. And they serve to explain a number of other facts connected with the action of acids and their combinations. They afford for example a solution of the difficulty which gave rise to the investigation—that of the production of water in the action of metals on muriatic acid gas.

Dr. M. extended the same view to the constitution of the alkalis. Alkalinity is as well as acidity a result of the agency of oxygen,—the fixed alkalis, the earths, and metallic oxides, all of which contain oxygen as a common element, forming a series in which there is no well defined line of separation. Ammonia stands insulated; it contains no oxygen, yet its alkaline properties are energetic, an anomaly which has led generally to the belief that oxygen must exist in one or other of its constituent principles. It may be explained, however, on a very different principle. As hydrogen like oxygen communicates acidity, so it may like oxygen give rise to alkalinity. Ammonia therefore will be a compound, of which nitrogen is the base, deriving its alkaline quality from hydrogen; and hence stands in the same relation to the other alkalis that sulphuretted hydrogen does to the acids. If the claim of the newly-discovered principle in opium to the rank of an alkali be established, it may stand in the same relation to the others that prussic acid or some of the vegetable acids do to the acids.

The fixed alkalies, barytes, strontites, and lime have been supposed to contain combined water essential to them in their insulated form. It is probable that the elements of water rather exist in direct combination with their metallic base: that potash, for example, is a ternary compound of potassium, oxygen, and hydrogen; and thus the entire class will exhibit the same relations as the class of acids, some being compounds of a base with oxygen, ammonia a compound of a base with hydrogen, and potash, soda, &c. compounds of a base with oxygen and hydrogen; and these

these last, like the analogous order among the acids, exceed the others in power. When an acid and alkali unite, the hydrogen of both is expended in forming water. The neutral salts, according to these views, will therefore be either sur-compounds of two binary compounds, one of the radical of the acid, the other of the radical of the base with oxygen, or they are ternary compounds of the two radicals with oxygen. The latter is the more probable opinion.

XIII. *Intelligence and Miscellaneous Articles.*

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for December 1817, it appears that during that month the following was the work performed by the engines reported, with each bushel of coals.

	Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
25 common engines averaged	22,409,878	various.
Woolf's at Wheal Vor ..	29,467,621	17·2 lib.
Ditto Wh. Abraham ..	38,812,073	16·8
Ditto ditto	27,236,924	4·76
Ditto Wheal Unity ..	32,265,991	13·1
Dalcouth engine	39,561,904	10·6
Wheal Abraham ditto ..	31,060,512	10 9
United Mines engine ..	31,315,593	17·3
Treskirby ditto	38,187,872	10·7
Wheal Chance ditto ..	30,535,439	15·1
Erratum in November Report; Woolf's engine at Wheal Vor, *read*	29,693,915	

SAFETY-LAMP CONTROVERSY.

In our Number for December, we alluded to a Report upon the Claims of Mr. George Stevenson, relative to the Invention of his Safety-lamp, by the Committee appointed at a Meeting holden in Newcastle on the 1st of November 1817; with an Appendix containing the Evidence.

In the same Number we laid before our readers the Resolutions passed at a General Meeting of the Coal-owners of the Tyne and Wear, held at Newcastle on the 26th of November, for the purpose of taking into consideration certain Resolutions passed at a Meeting of the friends of Mr. George Stephenson on the 1st of November, and which were inserted in our Number for that month.

The Resolutions of Mr. Stephenson's friends just alluded to, are prefixed by way of Introduction to the Report of the Committee on his claims. The Report itself occupies thirteen 8vo pages, and there is added to it an Appendix consisting of thirteen

teen pages, detailing the evidence on which the Committee have founded their Report.

Before proceeding to lay before our readers the observations which we mean to submit to them on this Report, we shall, as intimately connected with the subject, first insert the following exposure.

An Exposure of the Falsehood of an Opinion given in the Form of a Resolution at a Meeting of certain Persons in Newcastle, Nov. 1, 1817, CHARLES J. BRANDLING, *Esq. in the Chair*.*

Sir Humphry Davy's attention was first called to the subject of the prevention of explosion from fire-damp by the Rev. Dr. Gray. Towards the end of August 1815, he visited the neighbourhood of Newcastle, that he might become acquainted with the causes and operations of fire-damp, and learn from the miners themselves what was wanting for their security; and he stated to some of the gentlemen who offered him their assistance on this occasion, his hopes, that a chemical examination of the properties of fire-damp, might furnish to him some means of procuring or guarding a light so as to prevent it from firing the inflammable air. From August 1815, till the beginning of October, the subject occupied his attention as an object of speculation. In the beginning of October he commenced his experiments on fire-damp, and before the end of the third week † in that month, discovered certain facts respecting that inflammable substance, which enabled him to expose a light in safety to an explosive atmosphere, and to make an apparatus, which he immediately named a *safe-lamp*. These facts were, "that the fire-damp was the least inflammable of explosive elastic fluids, and that certain cooling influences of inexplosive elastic fluids, as azote and carbonic acid, or of solid surfaces, as small tubes, arrested its explosion." He made no secret of these facts; they were immediately communicated to various scientific persons, and letters respecting them were written to the President of the Royal Society, the Lord Bishop of Durham, the Rev. Dr. Gray, and the Rev. John Hodgson; so that by the end of October or the beginning of November they were pretty generally known. The application of these results was obvious; but multiplied and delicate experiments were required to determine all the circumstances necessary for security, and to ascertain the best methods of reducing them to practice; and Sir H. Davy knew too well the dreadful consequences of a failure, to put any rude and imperfect instruments into the hands of the collier. His first lamps were made with small apertures below, and larger

* From the Durham County Advertiser.

† Sir H. Davy can fix the 18th, from various evidence.

ones above, and the flame burnt in them at a distance from the apertures with diminished light, and was extinguished in explosive mixtures by the azote and carbonic acid formed in combustion. He next used systems of safety-tubes and safe-air canals, both below and above; and he gradually increased the number, and reduced the length of his tubes and canals, till he was led to adopt a tissue permeable to air and light, and impermeable to the explosion of fire-damp, even when red-hot. He first made air-feeders and chimneys of this tissue, and then adopted it as the guard of flame. From the first sitting of the Royal Society, the 9th of November that year, he regularly communicated the progress of his researches to that illustrious body; and before the 31st of December 1815, he presented to the miner the wire-gauze safe-lamp, which not only affords a safe light in explosive atmospheres, but which even consumes the element of destruction.

This simple statement, which might be increased ten-fold by authorities and details, is given for the purpose of contradicting an opinion which has appeared in the form of a resolution of certain persons at Newcastle, Charles J. Brandling, Esq. in the chair; to this effect:—" That it is the opinion of this meeting, That Mr. George Stevenson having discovered the fact that explosion of hydrogen gas will not pass through tubes and apertures of small dimensions, and having been the first to apply that principle in the construction of a safety-lamp, is entitled to a public reward."

To show that there is any foundation for this opinion, it ought to be proved,

1st. That the first lamps which this person sold as safe-lamps were founded upon the principle of explosion not passing through tubes and apertures of small dimensions.

2dly. That this person had made, and communicated to credible witnesses, a practical demonstration of this principle before the middle of October 1815, or had published it before November 1815.

Now the lamps which Mr. G. Stevenson sold in January 1816, had in them neither safety-tubes nor safety-apertures. One of three bought for the Wallsend colliery is preserved in the Royal Institution. It is furnished with holes from one-eighth to one-tenth of an inch in diameter, connecting an air chamber below, and has a chimney with holes in it of from one-third to one-fourth of an inch in diameter above, and it produces the explosion of explosive mixtures almost as readily as a common candle. The apertures are *too many* and the flame *too near them*, to render it safe by a diminished circulation of air; and infinitely *too large* to render it safe by the cooling effects of their sides. The first lamp of this kind was produced in public on December 5, at Newcastle, (*three weeks* after it was known there that Sir H. Davy had made a safety-lamp with *small apertures*, and was believed to be safe by the persons who saw it,) because the flame was extinguished in it by admitting *unmixed* fire-damp, or carburetted hydrogen! which would have been the case had it been a common lantern.

Having shown that the lamps sold by Mr. G. Stevenson in January 1816 have in them *no principle* of security, it is hardly necessary to prove, that he could not have discovered and applied a principle of security before the middle of October 1815.

It may, however, be proper to examine some statements that he has made, or that have been made for him. It was said, but not published till September 1816, that he showed a lamp having three capillary tubes to his employer, Mr. Lambert, on November 17th, and tried it before him with hydrogen; and Messrs. C. and R. Brandling state for him, that he tried a similar experiment before them November 24th. There can be no doubt that these trials were conducted as those of December 5th, at Newcastle, by throwing unmixed fire-damp into the lamp, and therefore they can have been of no value; and it is certain that a light could *not be supported*, if fed with air by capillary tubes or by safe tubes of the length and figure of those engraved in his pamphlet* published in 1817; and besides, a *fortnight before* this period, Sir H. Davy's fact that explosion of fire-damp would not pass through certain small tubes was known at Newcastle, and a week before his paper had been read at the Royal Society.

Mr. George Stevenson himself asserts, that he tried an experiment with a lamp furnished with what he calls a tube and a slider, on October 21, 1815, and that he had tried the lamp with the three tubes November 4th; but he did not *publish* this account till September 1816; and the Rev. John Hodgson states that he had circulated, about the 2d or 3d of November, manuscript copies of notices of Sir H. Davy's discoveries, which were communicated to him briefly the 19th of October, and fully the 30th of October 1815, amongst the viewers of the neighbourhood; so that even if it be allowed that Mr. George Stevenson began his *experiments* on *capillary* tubes the 4th of November, still, at the time, he *might* have heard of Sir H. Davy's facts respecting them.

With regard to his experiments on a lamp with a tube and a slider, this apparatus must be regarded as original; but it never could have been founded upon any idea of safety-tubes or safe apertures, and never could have led to such an idea; for the flame of a wick placed as his is figured in 1817, round a tube,

* A Description of the Safety-lamp, &c. Baldwin, Cradock, and Joy.

could

could not have been supported with air, except by an aperture *fifty times too large* to have been safe, and the wick is too near the source of air to have *produced safety* by the production of azote and carbonic acid.

When he first showed his lamp of December 5, he did not pretend to have discovered *any principle* of security, nor was any stated for him; nor in September 1816, when he first published his experiment of October 21, 1815.

Mr. R. W. Brandling, in September 1816, however, in speaking of his lamp of November 24, states for Mr. G. Stevenson, that *he had embraced the idea two months before,* "that hydrogen gas admitted into a lamp in small detached portions might be consumed by combustion."

Allowing this to be his principle, if an *incorrect* idea can be called a principle, it becomes easy to explain his experiments with the tube and the slider. The slider is an apparatus fitted to *detach*:—the substitute of a person ignorant of chemistry for the ingenious water valve of Dr. Clanny.

To go back two months, would give the conception of this idea on the 24th of September, when he must have heard that Sir H. Davy was engaged in the inquiry respecting a safe light.

From November, all that can be traced in his views, is a rude attempt to identify his own crude notions and unscientific practical efforts with the results and principles of Sir H. Davy.

In 1817, he has made a lamp which is a coarse imitation of one of Sir H. Davy's earlier lamps; but provided with his safety-screw, his safety-trimmer, and safe apertures and canals below, and a chimney-top of the same structure as wire-gauze above; yet even this apparatus, such is the want of chemical information of the constructor, is highly dangerous—it is furnished with a thick glass cylinder*, which *breaks* soon after the lamp is introduced into an explosive atmosphere.

It would be wasting time to show by minute details how little Mr. G. Stevenson's evidence is to be confided in.

In December 1815, when his original ideas, if he had any, and researches, if he had made any, must have been fresh in his mind, he could give *no account of them*; and in 1816, the first idea that he is said to have embraced, was that of burning detached portions of hydrogen; but in 1817 every thing is perfectly clear to him, and he states, that experiments which he had been making for four years, led him to the discovery of the principle of the non-transmission of flame through small apertures!!

* The Messrs. Brandling, it is well known, do not use *this* lamp, but Sir H. Davy's.

That Mr. G. Stevenson's pretensions as the inventor of a safe-lamp are unfounded, has been sufficiently proved; but even if he had been a real and independent discoverer, still *his dates* of trials upon lamps, taken even upon *his own assertions*, are posterior to those of Sir H. Davy; whose results were witnessed by men of science, and communicated by letter, before this person can be even imagined to have made an experiment.

The opinions of a few country gentlemen connected by family ties, and deriving their information from persons who do not know the difference between *hydrogen* and *fire-damp* or *carburetted hydrogen*, can have no relation to the history of science; and can never destroy, in the scientific world, the knowledge that Sir H. Davy is the discoverer of the "principle that explosions from fire-damp* in close vessels can be arrested by systems of tubes and apertures, and by metallic tissues permeable to air and light, and that he was the inventor of the safe-lamp on this principle."

Sir H. Davy has thought it his duty to expose the falsehood of the opinion expressed in the Resolutions of the Messrs. Brandling. Had they claimed a public reward for the persons named in their resolutions, upon *any other* ground, if he could not have encouraged it, he should have been silent.

After the foregoing exposure, but few remarks are called for on the Report of Mr. Stevenson's Committee. On perusing it, it is difficult to say whether most surprise is produced by the nature of the evidence, or by the conclusions drawn from it by the Committee.

Two tinmen are brought forward to prove that Mr. G. Stevenson ordered certain lamps which he says were tried the 21st of October and 4th of November 1815; but Mr. Hogg, the first of these tinmen, fixes no date, and only says Mr. Stevenson ordered a lamp from him some time in September or October, which was a fortnight in making: and Mr. Matthews, the other tinman, whose evidence is of the first importance, as intended to relate to a lamp with pretended capillary tubes, asserts that he merely recollects making some trifling alteration in a lamp, *but he does not know what or when.*

* The late Mr. Tennant discovered that explosions would not pass downwards in gas-light burners of small dimensions, when an explosive mixture issued from them was fired in *the air*. Mr. Tennant did not publish his discovery, and Sir H. Davy did not hear of it till after he had made his experiments on the powers of small tubes to arrest explosions; and he has quoted Mr. Tennant in his first paper on fire-damp, and by mistake connected Dr. Wollaston, who only witnessed his experiments, with Mr. Tennant.

It

It is evident from all that Mr. Stevenson has said in his evidence, and from every thing that has been said for him, that his original idea was to burn the fire-damp, or, as he and the Committee choose to call it, hydrogen, from a blower, at a small tube or tubes in a vessel so confined that the whole current of the blower could not be fired from it. This explains every thing; "his burning the hydrogen in detached portions," the top of his apparatus being open to admit air, and his putting his tube-lamp in the current of a blower.

That he had no notion of the nature of explosive mixtures, or of explosions not passing through small apertures, till long after Sir H. Davy's researches were published, is certain; and the best proofs of it are found in experiments upon his early lamps; for models of these, whenever they will burn in explosive mixtures, instantly communicate explosion.

Models of his two first pretended safe-lamps, that with the tube of half an inch in diameter and slider, and that with the three tubes of between 1-7th and 1-8th of an inch diameter, and three inches and a half long, have been made by Mr. Newman; and one of his own *original* aperture lamps is preserved in the Royal Institution. Any person who will take the trouble to make experiments upon these lamps will find that they are *exploding* and *not safe*-lamps. They may be made to burn *pure fire-damp* as gas-light burners, when open at the top; but in *really explosive mixtures* they are almost as dangerous as naked lights; and Mr. Stevenson's own lamp, sold by him in January 1816, *communicates* explosion even when the *larger apertures* in the bottom are closed.

As to the lamp now used in the Killingworth colliery, it is in reality one of the first forms of Sir H. Davy's lamp. Mr. Stevenson does not deny *all the details* of the piracy on which the Committee thought proper to question him.

However the Committee may be deceived, by their want of chemical information, in confounding large apertures with small ones, the powers of long curved tubes with those of short straight ones in feeding flame, and the difference between tubes of half an inch in diameter with sliders, and systems of tubes of 1-10th of an inch, in arresting explosions; yet upon the question of dates they have not any excuse of this kind to plead. They quote a letter of Sir H. Davy to the Rev. John Hodgson, of the 19th of October, as containing a passage relating to his (Sir H.'s) lamp with apertures above and below; but omit the passage relating to his discovery that explosive mixtures will not fire in small tubes; and they refer to Mr. Stevenson's experiments prior to the 21st of October, as if he had been trying experiments on the powers of tubes to arrest explosion; whereas he says himself that his experiments were *on putting out blowers by burning candles to windward of them;* and that the first trial which he made with any lamp, and which was any thing but a safe lamp, was on October 21.

The question of priority is as clearly against the Committee as every other part of the case. It would indeed be wasting time to dwell longer upon the subject; for, other circumstances apart, a complete refutation of the Report of the Committee is to be found in a comparison of Mr. G. Stevenson's pamphlet, with his *evidence*, which *seems to have been given upon oath.* The perforated tops, safety-trimmers, cylindrical form, and perforated external case of his first two lamps, *imagined* to make them look like the wire-gauze lamp, are allowed to have had no existence; and it is certain that he showed *no lamp*, even in private, except to persons who may be considered as his assistants, till towards the end of November; and then he showed a lamp to gentlemen who have not come forward to state what experiments were made with it; but who must from the newspapers have been at the time acquainted with Sir H. Davy's researches: and it is remarkable that after this time, tubes, and a top open above, were changed for small apertures below, though not safe ones, and larger ones above;—an apparatus the very same in construction as Sir H. Davy's first lamp, of which a notice was communicated by the Rev. Dr. Gray, and the Rev. John Hodgson, to a large meeting of the coal-trade November 10.

As to the evidence of some of the members of the Literary and Philosophical Society of Newcastle, respecting experiments with a lamp in the month of *December* 1815, the Committee might as well have brought forward experiments made last week. But enough of this.

The labours of the Committee, however, have been of essential benefit to Mr. Stevenson; for we find that on Monday the 12th of January 1818, at a numerous assembly of Mr. Stevenson's friends, and which was attended by some noblemen and gentlemen of great respectability, Mr. Stevenson was presented with a "beautiful tankard," (silver we suppose,) "and about seven hundred pounds" subscribed by Messrs. Brandling and their friends. No person can be sorry that the exertions of any individual to serve the cause of humanity, should have met with so ample a reward, however much the attempt to rob the real inventor of the safety-lamp of the merit of his singular and unhoped for discovery, must be reprobated by every real friend to science.

That Mr. Stevenson might, in September 1815, have been thinking about lamps for preventing explosions in mines, furnishes

nishes no argument in favour of priority of invention, any more than Sir H. Davy's cogitations in August would establish his priority, had he not actually been first in publishing his principles. Many were turning their attention to the subject, as Dr. Murray and Dr. Clanny; and if the object of the Committee was to reward priority and ingenuity, why did they pass over the latter of these gentlemen, who actually did produce a lamp that might have been of real use to the miner, had a simpler and better not been discovered by Sir Humphry—a lamp which owed its origin neither to accident nor plagiarism, but to reasonings founded on science, and the deductions of a philosophical mind.

LECTURES.

Mr. Bakewell will commence his Series of Lessons on Geology at the Argyle Rooms early in March, to be elucidated by a magnificent *suite* of Rock Specimens recently collected by himself, and by a great variety of new and original drawings, sections, and models.

Mr. Bakewell is also preparing for publication A Treatise on Practical Geology, with Plates: to which will be added, A Series of Questions addressed to British Geologists on certain undetermined Parts of English Geology, &c.

Mr. Dowling's Lectures on Natural Philosophy, Astronomy, and Chemistry, begin after the Christmas Recess, on the 3d of February. The Lectures on Philosophy are illustrated by a complete set of apparatus; those on Astronomy, by machinery, transparencies, transit instruments, and several very powerful telescopes; and the Lectures on Chemistry are rendered peculiarly interesting by numerous beautiful and useful experiments. These Lectures are regularly delivered at the Lecture Room and Observatory, Mansion-House, Highgate; and Mr. Dowling spares no pains to render them as effective as possible, by familiarly explaining the difficulties that occur, and by blending the *utile cum dulci*.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Papps, of No. 4, Clayton Place, Kennington, Surrey, for certain new improvements in certain books of account, commonly known under the names of denomination of cash book, bought-and-sale day books, or journal and ledger.—19th December 1817.—2 months.

To William Cleland, of Bolton Le Mores, in the county palatine of Lancaster, for his improvement in the bleaching of flax and hemp, and also in the bleaching of yarn and cloth, or other goods made of either of those articles.—20th December.—6 months.

To Edward Cowper, of Nelson-square, in the county of Surrey, printer, for his certain improvements in printing-presses or machines used for printing.—7th January 1818.—2 months.

To John Collier, of Frocester, in the county of Gloucester, engineer, for his certain improvements on a machine for the purpose of cropping woollen cloths of every description.—15th January.—2 months.

To John Lewis, clothier; William Lewis, dyer; and William Davies, engineer; all of Brimscomb, in the county of Gloucester, for their certain improvements on shearing-machines for shearing and cropping woollen and other cloths that may require such a process; the same being further improvements on a patent obtained by John Lewis, for an improved shearing-machine, dated the 27th day of July 1815.—15th Jan.—6 months.

To Philip Taylor, of Bromley, in the county of Middlesex, operative chemist, for his new method of applying heat in certain processes to which the same method hath not hitherto been applied; likewise for improvements in refrigerators.—15th Jan.—6 months.

To William Moult, of Bedford-square, in the county of Middlesex, for his certain improvements in steam-engines.—15th Jan.—6 months.

To John Holworthy Palmer, of Regent-street, in the parish of Saint John, Westminster, in the county of Middlesex, gentleman, for his new mode of purifying certain descriptions of gases. —15th Jan.—6 months.

To John Theodore Koster, in the county of Lancaster, merchant, for his new or improved method of building or constructing wheeled carriages; and also for making wheels for carriages. —15th Jan.—2 months.

To James Fraser, of Long Acre, in the parish of Saint Martin in the Fields, in the county of Middlesex, engineer and coppersmith, for his cooking machine, for the more simple and effectual decomposition of salt-water, and to render the said salt-water more useful to the general purposes of ships' crews, &c. at sea, without any extra apparatus except the said cooking machine; or, in other words, its structure will answer the end of worm or condenser and worm tub, &c.—15th Jan.—2 months.

To Charles Brightly, of Bungay, Suffolk, printer, and Bryan Donkin, of Grange Road, in the parish of Bermondsey and county of Surrey, engineer, for their improved machine or printing-press for printing from types, plates, or blocks.—17th Jan. —4 months.

To

To Marc Bambard Brunel, of Lindsay Row, Chelsea, in the county of Middlesex, civil engineer, for his method or methods for forming tunnels or drifts under ground.—20th Jan.—6 mo.

To Hugh Ronalds, of Hammersmith, in the county of Middlesex, gentleman, for his certain improvements in the art of making leather.—23d Jan.—6 months.

To Joseph Corty, of Harley-street, Cavendish-square, in the county of Middlesex, merchant: in consequence of a communication made to him by a certain foreigner residing abroad, he is in possession of certain improvements on and additions to stills, or the apparatus used for distilling, and also in the process of distilling and refining.—20th Jan.—6 months.

To Benjamin Wilson, of Abbey-street, Bermondsey, in the county of Surrey, flax-manufacturer, for his new machine for breaking, swingling, and preparing flax or hemp.—23d Jan.—6 months.

To Richard Banks, of Hadley, in the parish of Wellington, in the county of Salop, engineer, for his certain further improvements in wheeled carriages.—23d Jan.—6 months.

To Thomas Calderbank, of Liverpool, in the county of Lancaster, plumber, for his certain improvements in the working of pumps and other machinery.—23d Jan.—2 months.

ASTRONOMICAL PHÆNOMENA, FEBRUARY 1818.

D. H. M.		D. H. M.	
1. .	☾ in perigee	17.20.49	☾ ψ^2 ♋
2. 6.53	☾ φ ♐	18.16.37	☉ enters ♓
2.10.32	☾ σ ♐	20. 1.42	☾ η ♌
10 6.20	☾ ο ♓	22. 0.15	☾ ν^3 ♍
13. 5.56 Im	} A ♉ * 5′ S. of	23. 4. 0	☾ γ ♍
13. 7.20 Em	} ☾ Cent.	23.17.50	☾ θ ♍
13. .	☾ in apogee	25. 1.48	☾ λ ♍
14. 8.36	☾ ♂	25.21.24	☾ δ ♏
15. 2.44	☾ 125 ♉	27. .	☾ in perigee
17. 5.44	☾ υ ♊		

METEOROLOGY.

To Mr. Tilloch.

Dear Sir,—I was happy in understanding from you that experiments were making at Edinburgh, to ascertain the ratio of quantity of rain caught at different heights; as that when ascertained will materially assist the meteorologist in comparing the results of quantities of rain shown by gauges in various situations. At present our information is very defective, as we know little more than the fact that the quantity of rain caught at a small variation

variation of height,—as between the top and bottom of a steeple or house,—is materially different. In the year 1767 some experiments were made at Westminster. Two similar gauges were placed one at the top, the other at the bottom of the tower of the Abbey, and it was found in the course of twelve months that the highest gauge received or caught only twelve inches of rain; while the one placed on the ground caught nearly double; viz. twenty-two inches and a half. Other experiments were made in different places, with the same general result; which shows that the height the gauge is placed from the ground must be noted before comparative results can be deduced. We are in want of the ratio of quantity caught at any stated height when compared with that which may have fallen on the surface of the earth beneath. One step has been made by Mr. Dalton, who has shown that a gauge elevated 150 feet from the ground, caught in winter *one half*, and in summer two-thirds as much as a gauge placed on the ground below. Yet still the general ratio of height and quantity is wanting, and the ascertaining this question will be a valuable acquisition to meteorological science.

While this research is in progress, it may be agreeable to your readers to have a periodical statement of the quantity of rain that falls, and at the same time the quantity of evaporation that takes place from the surface of water.

The apparatus with which these observations are made, consists of a rain-gauge or ring (such as used by the Royal Society) of twelve inches in diameter; the water caught in this area is measured in laminæ of the one-thousandth of an inch thick, and the area of the ring.

The evaporating instrument consists of a vessel of the same area as the rain-gauge, viz. twelve inches diameter and five inches deep, and is placed at the same height from the ground, and consequently receives an equal quantity of water as the rain-gauge. In the evaporator a known quantity of water is put, and the vessel is left open to the action of the air and sun; the surface of the water being protected from the depredation of birds, &c. by the intervention of a net of large meshes composed of fine wire. The registry of quantity of rain and evaporation is made weekly on the same day and hour; and the apparatus is fixed so that the upper edges or gauge-rings are four feet from the ground.

I shall now proceed to give you the observations made since January 1817, and they will I trust prove sufficiently curious to attract notice. I am, dear sir, yours,

Croydon, Jan. 1818. H. Lawson.

[In the subjoined Table the heights are expressed in inches and decimal parts.]

Months.

Months.	Rain.	Evaporation.	Months.	Rain.	Evaporation.
1817.			1817.		
Jan. 1 to 19	3·138	0·530	13 to 20 July.	0·977	0·693
19 to 26	0·132	0·238	20 to 27	0·556	0·679
26 to 2 Feb.	0·031	0·076	27 to 3 Aug.	0·561	0·785
2 to 9	0·086	0·216	3 to 10	0·265	0·811
9 to 16	0·712	0·193	10 to 17	0·785	0·818
16 to 23	0·145	0·250	17 to 24	0·470	0·495
23 to 2 March	0·176	0·317	24 to 31	1·056	0·525
2 to 9	1·440	0·269	31 to 7 Sept.	0·013	0·661
9 to 16	0·003	0·289	7 to 14	0·319	0·420
16 to 23	0·009	0·374	14 to 21	0·114	0·231
23 to 30	0·325	0·303	21 to 28	0·354	0·411
30 to 6 April.	0·016	0·751	28 to 5 Oct.	0·010	0·269
6 to 13	0·052	0·449	5 to 12	0·011	0·326
13 to 20	0·015	0·657	12 to 19	0·818	0·105
20 to 27	0·056	0·631	19 to 26	0·283	0·083
27 to 4 May.	0·139	0·500	26 to 2 Nov.	0·836	0·115
4 to 11	0·844	0·867	2 to 9	0·513	0·177
11 to 18	0·440	0·849	9 to 16	1·099	0·118
18 to 25	2·205	0·500	16 to 1 Dec.	0·283	0·242
25 to 1 June.	0·455	0·382	1 to 7	0·580	0·039
1 to 8	0·097	0·937	7 to 14	0·691	0·054
8 to 15	1·026	0·546	14 to 21	1·931	0·223
15 to 22	0·000	1·203	21 to 28	0·064	0·022
22 to 29	0·138	1·114			
29 to 6 July.	1·023	0·669	Total in year	25·349	22·227
6 to 13	0·057	0·815			

RAIN TABLE.

The following is a statement of the quantity of rain which fell during the last year, at Carbeth (Stirlingshire), Greenock, Bothwell Castle, and M'Farlane's Observatory, Glasgow, according to the indications of very accurate and uniformly made rain-gauges:

	Carbeth. Inches.	*Greenock.* Inches.	*Bothwell.* Inches.	*Glasgow.* Inches.
January	4·696	4·622	3·405	2·624
February ...	4·562	4·342	3·628	3·103
March	4·61	4·725	3·67	·627
April	·322	·21	·257	·072
May	3·371	2·465	3·81	1·93
June	4·405	3·1	2·92	2·312
July	3·246	3·637	2·1	1·773
August	6·185	4·676	3·707	2·854
September ..	2·163	1·51	1·432	·629
October	1·35	1·23	1·24	·892
November ..	4·47	4·642	3·476	2·546
December ..	5·585	4·226	2·904	3·058
Total inches .	44·965	39·385	32·549	22·42
Total in 1816	39·589	34·973	25·907	23·799
Total in 1815	41·393	36·763	24·72	22·344

METEOROLOGICAL TABLE

Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23′ 30″.—Above the level of the Sea 129 feet.

1817.	Morning 8 o'clock. Mean height of Barom.	Morning 8 o'clock. Mean height of Ther.	Evening, 10 o'clock. Mean height of Barom.	Evening, 10 o'clock. Mean height of Ther.	Mean Tempr. by Six's Ther.	Depth of Rain. Inch. 100	N° of Days. Rain or Snow.	N° of Days. Fair.
January.	29·50	38·41	29·55	38·71	39·580	1·75	12	19
February.	29·60	40·37	29·54	39·01	41·070	1·90	17	11
March.	29.57	38·00	29·58	37·77	39·700	1.10	11	20
April.	30·18	43·86	30·18	42·73	45·600	0·50	7	23
May.	29·65	46·19	29·66	44·03	47·774	3·10	13	18
June.	29·67	55·03	29·69	52·90	57·600	4·80	15	15
July.	29·60	55·74	29·61	53·71	57·480	3·85	20	11
August.	29·57	53·87	29·55	52.13	54·900	5·25	20	11
September.	29·84	51·30	29·83	51·10	53·833	0·85	9	21
October.	29·95	40·06	29·96	39·77	42.130	1·55	9	22
November.	29·70	43·50	29·70	45·20	45·800	2·70	16	14
December.	29·43	34·09	29·45	34·06	35·548	3·66	11	20
Average of the year.	29·685	45·035	29·691	44·260	46·751	31·01	160	205

ANNUAL RESULTS.

MORNING.

Barometer. Observations.	*Wind.*		*Thermometer.* *Wind.*	
Highest, 6th April	NE.	30·57	25th June, NW.	68°
Lowest, 18th Dec.	SW.	28·40	12th Dec. W.	22°

EVENING.

Highest, 6th April,	SE.	30·61	25th June, NW.	62°
Lowest, 18th Dec.	SW.	28·40	22d Dec. NW.	15°

Weather.	*Days.*	*Wind.*	*Times.*
Fair	205	N. and NE.	25
Rain or Snow	160	E. and SE.	91
		S. and SW.	133
	365	W. and NW.	116
			365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 23d December	Wind NW.	15°
Hottest, 24th June	Wind SE.	76°
Mean Temperature for 1817		46° 75′

Result of Three Rain Gauges.	In. 100
No. 1. On a conical detached hill above the level of the Sea 600 feet	44 40
— 2. Centre of the Garden, 20 feet	31·01
— 3. Kinfauns Castle, 129 feet	23·56
Mean of the 3 Gauges	32·99

METEORO-

Meteorological Journal kept at Walthamstow, Essex, from December 15, 1817, *to January* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between *Noon* and Two P.M.]

Date.	Therm.	Barom.	Wind.
December			
15	38 43	29·54	W—SW.—Fine clear star-light morning; fine sunny day; showers; a dark night. Moon first quarter.
16	39 47	29·62	SE.—Clear, and *cirrostratus*; rain, and very damp, dark, and rainy.
17	39 42	29·54	SW—W—SW.—Clear, high; *cirrostratus* low; very fine day; rain and wind.
18	43 46	28·98	SW.—Clear, and *cirrostratus*, and wind; very fine day; moon and star-light.
19	42 46	29·88	SW—W.—Rain early; very fine day; showers after 3 P.M.; wind and showers.
20	40 40	29·42	N.—Clear, and *cirrostratus;* very fine day; cloudy, and windy.
21	30 35	29·55	NE.—Clear, and *cirrostratus*; hazy; fine cold gray day; night cloudy.
22	31 35	29·50	NE.—*Cumuli*, and clear; fine sunny day; cloudy, but light.
23	31 34	29·50	NE.—Cloudy; very fine day; *cumulus*, stars and moon. Full moon.
24	30 31	29·66	N.—Gray, and white frost; very fine day; clear, and *cumuli*.
25	31 35	29·98	N.—Gray morn; very fine sunny day; *cumuli*, and clear night.
26	27 31	30·00	N.—Clear; moon and stars at 7 A.M.; very fine day; moon and star-light.
27	37 38	29·88	SW.—Slight rain; very fine sunny day; rain and wind.
28	35 38	29·54	NW—N.—Clear morn; fine day; very cold; clear and *cirrus*; star-light.
29	27 30	30·00	NW—N.—Clear, and windy; very fine day; dark and windy.
30	35 36	30·00	SW.—Clear, and *cumuli*; rain till after 3 P.M.; stars, and mist.
31	23 32	29·98	NW—SE.—Morn clear, moon-light, and white frost; very fine till *after* 2 P.M.; then *thick stratus*,—so thick that people lost their way, but it decreased after 4 P.M.; dark night, and *thin stratus*. Moon last quarter.

January

Date.	Therm.	Barom.	Wind.
January 1818.			
1	24 31	30·00	NE.—Clear, and white frost; fine sunny day; white frost on trees all day, the same as yesterday; night bright star-light.
2	31 35	30·00	NE.—*Cumuli* and clear; set snow; damp; thaw, slight snow frequently; dark and windy.
3	36 36	29·66	E—NE.—Snow on the ground; snowing; then rain frequently all day; dark and damp.
4	40 44	29·44	SE.—Snow nearly gone off; some small rain; fine sunshine; bright star-light.
5	44 44	29·54	SW—S.—Rain; very rainy day till after 4 P.M.; bright star-light; *aurora borealis* at midnight.
6	35 40	29·88	W.—*Cirrostratus*, and clear; white frost; extremely fine day; star-light. New moon.
7	35 39	30·10	SW.—Cloudy and hazy; gleams of sun, and *cumuli;* dark, wind, and small rain.
8	38 43	29·98	NW.—Clear, and windy; very fine day; star-light.
9	39 45	30·80	SW.—Hazy morn; frequent small rain; slight rain and wind.
10	48 53	29·98	SW.—Foggy; gleams of sun; fine day; dark night.
11	48 49	29·78	SW—S.—Foggy; frequent small rain; dark and windy; fine *aurora borealis* in the night.
12	39 42	29·66	W.—Perfect clear sky and wind at 7½ A.M.; very fine; sun and wind; rain, and wind.
13	48 40	29·87	SW—S.—Cloudy, windy, and small rain; damp, and windy; and some rain; cloudy, and windy; moon in a *corona*.
14	48 51	29·86	SW—W.—Small rain, and wind; at 1 P.M. weather-cock agitated, and great storm of rain and wind, and floating *nimbus* eastward; bright sunshine 3 P.M. and clear sky; cloudy night. Moon first quarter.
15	50 51	29·60	SW.—Stormy, wind, and cloudy; showers, and wind; windy and cloudy.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Dec. 16	8	42·	29·88	Rain
17	9	43·	29·70	Fine
18	10	46·	29·81	Ditto
19	11	43·	28·79	Rain
20	12	36·	29·67	Cloudy
21	13	34·	29·67	Fine
22	14	34·5	29·66	Ditto
23	full	33·5	29·66	Very fine
24	16	32·5	29·88	Ditto—snow at night.
25	17	32·	30·15	Ditto
26	18	31·	30·18	Ditto
27	19	35·	29·50	Ditto
28	20	34·	29·85	Stormy
29	21	3 ·	30·21	Very fine
30	22	32·	30·12	Ditto
31	23	33·	30·16	Ditto

Average of the Year 1817.

January	42·31	29·8486
February	46·714	29·96
March	47·07	29·9051
April	50·	30·3253
May	55·03	29·8155
June	66·016	29·94
July	64·63	29·877
August	63·193	29·8064
September	62·41	30·0563
October	49·85	30·0761
November	51·14	30·0341
December	38·4	29·68

Therm. Barom.
53·03 = 29·9437

The greatest height of the Thermom.	20th June	87° at	$3\frac{1}{2}$ P.M.
The least .. .. ditto	21st March	26	7 A.M.
The greatest height of the Barometer	9th Jan. / 7th April	30·65	
The least .. .. ditto	8th Dec.	28·71	

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For January 1818.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec. 27	38	40	38	29·68	7	Fair
28	40	40	32	·70	6	Fair
29	21	33	35	30.10	10	Fair
30	35	43	38	29·92	0	Rain
31	27	29	28	·92	0	Foggy
Jan. 1	27	32	30	·91	6	Fair
2	35	36	32	·90	0	Cloudy
3	28	36	37	·49	0	Snow
4	39	45	37	·42	0	Fair
5	45	45	36	·40	0	Rain
6	35	42	35	30·00	12	Fair
7	42	46	47	29·90	8	Cloudy
8	39	44	39	30·20	16	Fair
9	40	47	47	29·85	0	Rain
10	47	52	50	·75	0	Cloudy
11	49	49	46	·70	0	Rain
12	39	44	47	·95	9	Cloudy
13	47	53	47	·72	0	Rain
14	47	46	48	·75	0	Rain
15	47	54	47	·59	0	Cloudy
16	47	52	50	·75	14	Fair
17	40	43	37	·80	24	Fair
18	37	44	36	·90	27	Fair
19	29	42	36	30·43	21	Fair
20	34	44	36	·15	29	Fair
21	40	46	38	29·95	26	Fair
22	35	47	42	·50	27	Fair
23	36	42	38	·49	0	Stormy
24	35	43	37	·57	0	Rain
25	34	42	45	·92	16	Fair
26	40	49	40	·70	10	Showery

N.B. The Barometer's height is taken at one o'clock.

XIV. *On the Atomic Theory.* *By* William Higgins, *Esq.*

To Mr. Tilloch.

Sir, — You will oblige me by inserting in your Magazine, which is one of the least prejudiced channels of conveyance we have at present, the following paper. The first part is only preliminary to my theory respecting the connexion of light and caloric.

According to our present knowledge of chemical philosophy, the four imponderable elements with which we are acquainted, viz. caloric, the electric fluid, light, and the deoxygenating rays which accompany light, are the only agents in the hands of Nature to effect her operations, and to preserve ponderable matter in the state we constantly find it. They are said to be imponderable, because hitherto they have been found to possess no sensible weight; but as they are capable of uniting to ponderable bodies, they must gravitate towards them, and consequently must have some degree of weight, be it ever so small.

Every one of the imponderable elements has important duties to perform; and although apparently clashing or opposite duties, yet they all tend to uphold the great fabric of the universe, and to promote the grand and mighty objects of the Creator.

Should all the substances of our globe be only influenced by attraction, there could exist in Nature but solid matter; even our atmosphere would become a solid mass;—animal and vegetable life could no longer exist, and the whole face of the earth would exhibit a dreary and inanimate scene.

Caloric from its peculiar properties seems to be the agent employed by Providence to counteract such fatal effects; to modify or check attraction, and in many instances to almost wholly overcome it; and although these powers are constant antagonists, their respective and opposite forces are so balanced as to answer all the purposes of life, or animated nature.

We have every reason to suppose that caloric is a simple elementary matter; it is a fluid universally diffused, and in excess is capable of fusing or converting into the gaseous state all bodies by removing more or less distant their particles, or atoms, or molecules, as it may happen, from each other, at the same time that their temperature is most frequently increased.

Caloric possesses the property of uniting to bodies chemically without raising their temperature, or changing their state; we call it change of state in bodies when they pass from the solid to the fluid state; or from the fluid to the gaseous state. However, substances in the same state and of equal temperature often contain different portions of caloric: this perhaps depends upon their respective attraction being more or less powerful for that element. Philosophers attribute it to their different degrees of capacity for caloric; this is merely expressive of the fact itself, without accounting for it.

We know that the particles of ponderable elements of various kinds will unite to another imponderable element in different proportions; and in most cases, it is true, this does not depend upon a superior force of affinity:—for example, an ultimate particle of azote will unite to more than twice the quantity of oxygen that those of sulphur, phosphorus or carbon will; yet the latter substances have by far a more powerful attraction to oxygen than the azote, for they will readily deprive it of all its oxygen: probably this proceeds from the quantity of caloric or electric matter which the compounds formed are capable of engaging round their respective atoms*. Be the cause whatever it may, it is a fact which I advanced twenty-nine years ago for the first time, that the particles and atoms of ponderable matter unite in various and definite proportions; and the same law holds good respecting the chemical union of caloric to bodies:—it unites to some ponderable bodies in larger quantities than to others†.

Every single ultimate particle of elementary matter, even in solid masses such as metals, is surrounded with an atmosphere of caloric. Caloric thus attached to particles comes under the denomination of specific heat, and is no doubt intimately, if not chemically, attached, as it does not affect our senses or the thermometer.

When two ultimate particles unite chemically, they come into complete contact, and form one atom whose capacity for caloric is less than its constituents in a separate or simple state; hence it is that caloric is liberated by chemical union. These atoms, however, retain a sufficient quantity of caloric to furnish them with atmospheres.

When an atom, which consists of the most simple combination, unites to another atom chemically, the compound molecule governs still less caloric than its constituent atoms; yet that molecule is enveloped with an atmosphere of caloric. Molecules set bounds to chemical union; molecules are as distinct or as insulated from each other, notwithstanding their seemingly close approximation in their liquid or solid state, as particles or atoms; or, comparatively, as the planets in our system in consequence of their calorific atmospheres.

Fluidity, *flexibility*, *malleability*, *elasticity*, and that vibratory property which produces *sound*, depend upon the internal struc-

* All bodies naturally contain more or less electricity, but not in sufficient quantity to disturb their light and caloric.

† See my Comparative View.

ture

ture and arrangement of solid and fluid substances which I have now described; even glass, although hard and very brittle, possesses a considerable degree of elasticity, and is consequently very sonorous.

The ultimate divisions of ponderable matter are exceedingly minute; but those of the imponderable elements, such as caloric, the electric fluid, and light, are so beyond calculation. The utmost stretch of the human mind can no more estimate the size of those particles, than it can measure *infinite space or duration.* However, their divisibility is limited, notwithstanding their amazing minuteness. Probably a single ultimate particle of caloric bears the same proportion, in its size and weight, to a particle of oxygen, as the latter does to our globe: hence arises the impossibility of ascertaining the weight of that element;—it is likely that a still greater proportion, as to bulk and weight, exists between light and caloric.

From the foregoing considerations we can readily conceive the nature and structure of the calorific atmospheres which are influenced by particles, atoms, and molecules of ponderable matter.

The caloric which solid bodies contain may be partly set free by mechanical means:—instance;—when the malleable metals are quickly hammered or rolled between cylinders, their temperature is considerably increased. This is occasioned by the closer approximation of their particles, which diminishes their capacity for caloric. The temperature of gases is exalted by sudden compression on the same principle.

When certain solid bodies are made to act on each other chemically, more or less caloric and light are set free:—for example, when sugar and oxymuriate of potash are pulverized and mixed, a drop of sulphuric acid will produce a very rapid and vivid deflagration, which will pass through the whole mass, be it ever so large. When a spark is thrown into a mixture of nitre and charcoal a similar effect is produced. Although the foregoing facts are strong proofs of the existence of caloric and light united to solid bodies, they are not to be compared with the following; viz. nitre three ounces and a half, crude antimony two ounces, brimstone one ounce, all reduced to fine powder and well mixed, will deflagrate rapidly when a small spark is thrown in; or the ignition may be commenced by means of two or three grains of oxymuriate of potash and sugar laid on the surface, and a drop of sulphuric acid suffered to fall on it. During the decomposition intense heat and dazzling light are set free. In this process the nitrous acid of the nitre is decomposed, its oxygen unites to the antimony and sulphur; the new compounds thus formed demanding less caloric than the nitre, the surplus is set at liberty.

F 2 There

There is very little doubt, if any, but the whole of the caloric of the gases which constitute nitrous acid is retained in this curious substance; for when nitrous air condenses half its bulk of oxygen gas, with more rapidity than most inflammable substances, neither light nor heat is evolved, except indeed some increased temperature occasioned by the action of the newly-formed acid on the moisture suspended in the gases; and when the acid is again united to potash, very little caloric is given out. It is curious, as I first observed in my *Comparative View*, that nitrous gas should be lighter than the gaseous oxide of azote, more especially when the atoms of the former contain twice the quantity of oxygen :—this I attributed at the time to the different size of their respective atmospheres of caloric and *light* conjointly. But the most extraordinary difference in property between those gases is, that the gaseous oxide of azote which contains but half the quantity of oxygen contained in the nitrous air, will not unite to oxygen when both are mixed: perhaps (as I hinted at in my *Comparative View*) this depends upon a superior *density* or *superior force of attraction* of the atoms of the gaseous oxide for their calorific atmospheres, so as to prevent the particle of oxygen from passing through them.

When muriate of ammonia and sulphate of soda, in fine powder, are mixed, they decompose each other, and new compounds which demand less water are formed, and nearly a liquid mixture is produced. This is perfectly illustrative of the liberation of caloric in the foregoing experiments, for the two salts are dry before they are mixed.

The next way of setting free the caloric and light of bodies is by means of the electric fluid; for when a metallic wire or charcoal is placed in the way of that fluid, circulated by a powerful Voltaic battery, the caloric of their respective particles is disengaged, and a prodigious degree of heat and light is produced.

Substances that retain their calorific atmospheres with the greatest force must be the worst conductors; because the electric fluid has to force its way by removing the whole or a certain portion of it, which obstructs its passage; and the quantity of caloric thus dislodged depends upon the density or energy of the electric matter; for when a space is fully occupied by one fluid, it can only receive another by the expulsion of the former; both cannot exist in the same space. The quantity of caloric, I say, thus liberated depends upon *that* of the electric fluid; and the effects it produces as free caloric, depend upon the size of the masses through which it circulates. Instance;—the electricity of a large cloud will melt a mass of metal, whereas that produced by a Voltaic battery will only fuse small wires.

Metals, charcoal, and living animals and living vegetables are the

the best conductors of electricity, as a sufficient quantity of their caloric is easily removed on its passage through them; but a dead animal completely cold becomes a bad conductor. All the dry metallic oxides are non-conductors, as their calorific atmospheres are small and of course strongly attached to their atoms. Glass also, which consists of different oxides fused into one solid mass, is a non-conductor on the same principle. Yet glass becomes a good conductor when sufficiently heated so as to enlarge its calorific atmospheres, like dead animals while they retain a remnant of their vital heat*.

The electric fluid moves with great velocity through metallic bodies and charcoal, while the progress of caloric in those substances is comparatively very slow, and light is completely obstructed by them. Light, on the other hand, passes with ease through glass, mountain crystal and calcareous spar, and other diaphanous bodies; whereas, they in a great measure prevent the passage of caloric and electricity. Hence it should appear that caloric, electricity and light are substances, and simple substances, totally different from each other.

When the electric fluid is made to pass in a perfect vacuum no light or heat is produced, because this element does not possess those properties in its pure or simple state †.

When a strong electric spark is passed in atmospheric air or in any gas, a flash of light is produced. This is occasioned by the liberation of a portion of the specific heat, which resumes its former station again, round the particles of the air, the instant the influence of the electric matter, which is very rapid and instantaneous, terminates. The same effect is produced on the large scale of nature:—instance;—thunder and lightning.

* Dr. Thomson, as usual, misrepresented my meaning on this subject, page 60 of his Annals for July 1814. His remarks are as follow: "If this hypothesis were correct, the metals ought to be non-conductors, for they have little specific heat; and water and hydrogen ought to be the best conductors in nature, as they have the highest specific heats." I do not agree with the Doctor in his positions: metals contain a prodigious quantity of specific heat, and that heat is not so intimately united as that of water, except the small portion which is necessary to the fluidity of the latter; and when it is deprived of this portion and becomes ice, it is no longer a conductor. In this respect it agrees with glass. Hydrogen contains a considerable quantity of specific heat, but much less than chemists suppose; yet it retains it with greater force than the metals, and consequently is not so good a conductor. I now come to the point in which the Doctor attempts to pervert my hypothesis. He insinuates that I meant that those substances which contain the greatest quantity of specific heat are the best conductors; —no person but the Doctor himself could draw such an inference. I founded my hypothesis on the force of union of caloric to bodies, and not on the quantity which they contain; and substances which contain least of it round their particles or atoms, retain it, most frequently, with the greatest force.

† See my Essay on the Atomic Theory, and Electrical Phænomena.

The phænomena of the *aurora borealis* and fiery meteors, in our atmosphere, are occasioned also by electricity in the same way, that is by dislodging combined caloric.

The heat and light discharged by the electric fluid, particularly by means of friction, confounded philosophers, and led them into many errors, and even to question the materiality of caloric; attributing heat, like gravity, to a mere property of common or solid matter, inferring that it is produced by a peculiar vibration of their particles; and advancing many absurd notions respecting the electric fluid itself. This obscure and far-fetched philosophy, so inconsistent with the simplicity of the laws of nature, appeared to me absurd and chimerical. I have therefore advanced a doctrine which will account for all the phænomena attendant on electricity; and which will also tend to establish the materiality of the imponderable elements, and to give them that station among the ponderable bodies to which they are entitled; viz. their property of uniting with those bodies, and of dislodging one another from them, according to their different or respective powers.

Having shown that caloric and the electric fluid are antagonist elements, we shall now consider how far caloric and light agree with each other; and whether or not there exists any degree of attachment between those subtile elements. From the variety of facts which have been adduced respecting the caloric and light disengaged during the chemical action of solid substances on each other, and also those evolved from substances by a powerful Voltaic battery, and even by the common process of combustion which takes place constantly before our eyes,—there can be very little doubt but those two elements existed in a latent state in those bodies thus acted upon; and we may also infer that they are constant and inseparable associates. It is true that solid and fixed bodies may be made very hot without the appearance of light; but as they become luminous when their temperature is much exalted, no doubt but they contain combined light in their most obscure heat. Some facts which will be related presently, show that these elements may exist in a free and separate state, or nearly so.

Whenever caloric in a certain degree of concentration is dislodged from bodies, no matter how, light is constantly produced. This is a fact which philosophers should attend to. Does light, whose particles are more minute beyond calculation than those of caloric, form atmospheres round the particles of the latter? If so, are those atmospheres of light, by the concentration of caloric, so diminished by the condensation as to set a portion of it at liberty sufficient to afford *illumination?* and again, Do the particles of caloric, when they become less concentrated, or are removed to a certain distance from each other, engage the whole

whole of the light which they gave out, so as to prevent illumination?

We must draw the same line of distinction between free light and combined light that we do between free caloric and combined caloric. The light of the sun comes to us partly combined with caloric, but much of it is free. The same may be said with respect to the light given out by burning bodies. Caloric deprives the light, which forms atmospheres round its ultimate divisions, of the power of illuminating, on the same principle that the ultimate divisions of ponderable matter deprive their natural atmospheres of caloric of the power of heating.

As all bodies contain caloric, we have every reason to suppose that they also contain the matter of light; for it is constantly liberated, more or less intensely, during the rapid union of oxygen to most inflammable bodies, or by the union of certain inflammable bodies to each other, such as that of sulphur and copper, sulphur and potassium, regulus of arsenic and potassium, and potassium and tellurium, &c. without the agency of oxygen.

The brilliancy of the light, however, under all circumstances, depends on the intensity of the heat, or, in other words, upon the degree of the concentration of the particles of caloric at the moment of their liberation by the chemical union of ponderable bodies. In proportion as the caloric passes from the focus of its concentration by its particles flying from each other more or less distant, their capacity for absorbing light is proportionally increased; and consequently, as already mentioned, they recover those atmospheres of light which they lost by their concentration.

The following well-known simple facts tend to support this doctrine; viz. If a patent lamp, or any other brilliant burning body, be suffered to go on in a small and well-closed room, the temperature of the surrounding space is gradually exalted, at the same time that the light in the room is not in the smallest sensible degree increased, because it is absorbed by the expanding caloric as fast as it is liberated from the concentrated caloric, on the same principle that caloric is liberated by the compression of gases, and absorbed again upon restoring them to their former degree of expansion. Thus it is that the light of bodies in the act of brilliant combustion ceases the moment they are extinguished, without any marked interval; otherwise there would be a gradual diminution of light in the surrounding space before it totally vanished.

When a bright sun-beam is suffered to pass into a dark room through a small opening, the instant that opening is closed by an opaque substance the light vanishes, because its source is cut

 off,

off, and because the light of the beam in the room unites to the free caloric which accompanies it.

It is probable, as Sir Isaac Newton and other philosophers supposed, that the particles of light, like those of ponderable matter, possess their repulsive and attractive poles, and that it is upon this account that they dart from luminous bodies in straight lines; and it is also likely, while in the act of moving or radiating with velocity almost incalculable, that they are incapable of uniting to the free particles of caloric which accompany them in their flight; and that so soon as this rapid motion ceases, by the interposition of opaque bodies, their particles may assume a new arrangement in relation to each other, which arrangement may enable them to unite the more readily to caloric, and probably to certain ponderable bodies; and that by this union they become incapable of illuminating upon principles already explained. Light diminishes in regular progression through a medium of uniform density as we recede from the luminous body, as Count Rumford has shown in a very ingenious way by means of his photometer, a very simple instrument. This is occasioned by the absorption of the particles of light as they move along. Indeed the feeble light we receive from the fixed stars, which are considered suns as bright as our own, affords a strong illustration of the subject.

When the bright light of the sun, as it radiates immediately to us from that luminary, is collected into a focus by means of a large burning lens, prodigious heat and light are produced. Perhaps the intensity of the illumination is increased by the liberation of a portion of the combined light of the caloric from the concentration of its particles; and it is also more than probable that the intensity of the heat is in some degree increased by a diminution of the combined or atmospheric light of the particles of caloric, so as to set them more at liberty.

When moon-light is treated in the same way by a *lens*, very brilliant light is produced in the focus, but no sensible degree of heat whatever; yet by means of the prism, moon-light is separated into the seven coloured rays. Hence we may infer that the rays of light do not heat; but I am inclined to believe that moon-light contains some small portions of caloric, yet sufficient to absorb all the light when its source is interrupted by opaque bodies. Dr. Herschel the celebrated astronomer has shown that heat and light may be in some degree separated from each other, by placing a thermometer in the different coloured rays produced by the prism. The red ray being the least refrangible has the most heating power, and this power diminishes progressively according to the degree of the refrangibility of the different rays: consequently the violet ray, which is the most refrangible, has the

smallest

smallest heating power; close to the boundary of the red ray was found a colourless ray, which produced the greatest heat, and this is a ray of caloric. The heating power of the red ray and that of the violet are to each other as the following numbers: the violet 16; red 55. These facts prove that the rays of the sun contain free caloric as well as free light: whether the colourless ray of caloric may contain some small portion of combined light, is a question not readily solved; it is however very evident that all the rays contain more or less caloric, notwithstanding the quantity separated by the prism.

When a beam of the sun is reflected from one surface to another, or from different surfaces, its power of heating is much diminished, which proves that caloric is separated from the light. This tends to prove that calorific rays are not so flexible as those of light, and that on the large scale of nature, the surface of the moon reflects most of it back again in straight lines to the source whence it received it, or else absorbs it, so as to withdraw it from the light, which from its amazing divergency illuminates the universe. The first hypothesis seems to be the most probable.

Many facts might be adduced to show that the light which issues from burning bodies consists of radiant heat and the light itself barely mixed, and they can readily be separated from each other to a certain degree by different reflectors.

It was from turning in my mind repeatedly the facts adduced in this paper, that I conceived the idea, that as free light and caloric diverge from luminous bodies in company with each other, sudden darkness is occasioned by the instantaneous union of light to caloric, so as to form atmospheres round its particles; and in my opinion there is no other rational way of accounting for it.

The first part of this paper respecting electrical phænomena and caloric was published four years ago in my *Essay on the Atomic Theory*. I have only introduced that part of it here, and in a different point of view, which relates to my doctrine of the connexion of light and caloric, which appears to me to be perfectly new, according to the extent of my reading. It is founded on the Atomic Theory which I presented to the public twenty-nine years ago. At that distant period I felt that the principles I set out with were well founded, as the preface to the work itself will sufficiently prove: there I had predicted what it has ultimately led to; and yet we are told in the Monthly Review for May 1817, that I was not aware of the importance of my own ideas on the subject: but I refer the learned author of the paper on that subject to my Comparative View, which he may have at Mr. Murray's, bookseller, Albemarle-street, in its old

old garb, but it is the dress of the time in which it first issued from my hands*. This learned writer states also, that I was within a step of the doctrine in question, and wonders I had not gone a little further. I can assure him that, at this present moment, I cannot advance a single step further: as to the doctrine itself, it cannot be improved, although it may be more extensively applied. In another part of his critique he seems surprised that I should suffer it to lie by so long as twenty years without taking any further notice of it †. He should rather be surprised at the want of taste and judgement of scientific men at the time it was written, and in that respect he would agree with my own feelings on the occasion.

I must not pass by unnoticed the following paragraph, taken from the review of my Essay on the Atomic Theory ‡, &c. by M. H. Gaultier de Claubry, a gentleman whom I only know by his high character;—it is as follows: "C'est ce qui rend son idée plus importante, quoiqu'elle soit loin d'être développée comme M. Dalton l'a fait depuis §." I have proved in the above work, and in many papers lately published in the Philosophical Magazine, that Mr. Dalton erred most egregiously in the proportions of the elementary particles in sulphuric and sulphurous acids; but particularly in those of gaseous oxide, nitrous gas, nitrous acids, and nitric acid.

I will now produce a curious specimen of errors still worse than the former:--Here they follow, as taken from the second part of his work, plate V. Water is represented as consisting of a single ultimate particle of hydrogen united to one particle of oxygen;—so far he is right. An atom of fluoric acid consists of one of hydrogen and two of oxygen; muriatic acid of one of hydrogen and three of oxygen; and oxymuriatic acid of one of hydrogen and four of oxygen. Can any thing be more absurd or more wild? Yet he pretends to give the relative weights of the compound atoms, although he is quite ignorant of their constituents. Similar errors run throughout the whole work. How such a writer should be mistaken for a philosopher I cannot conceive; and why he should be supposed to improve my theory is equally inconceivable: were he to attempt to ridicule or rather to caricature it, he could not accomplish his purpose more effectually. How Dr. Thomson and Dr. Henry, &c. could attempt

* I thought until very lately that the work was out of print, there are only a few copies to be had.

† He will find on reading the preface to my Essay on Bleaching, that I have not neglected my *offspring* as he is pleased to call it. It could not be expected that I should be continually puffing it off as quacks do their nostrums; I was confident that it would force its own way sooner or later, and I was not disappointed in my expectations.

‡ This work may be had of Longman and Co.

§ *Journal de Physique.*

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to hand over my Theory to him is unaccountable. Probably it is because they do not perfectly understand it.

The Atomic Theory is such that it could never originate but from deep investigation and from a complete knowledge of chemistry. With those qualifications a man could never be guilty of gross errors; but the man who borrows his ideas and attempts to apply them, is liable to commit mistakes every step as he moves along, like the monkey who, as we are told, in attempting to shave himself, in imitation of his master, cuts his own throat. Imitation and originality are readily distinguished. They can only be confounded by partiality and prejudice.

I am, sir,
Your very obedient humble servant,

Dublin, Jan. 12, 1818. WM. HIGGINS.

XV. *An Analysis of Sea-water; with Observations on the Analysis of Salt-brines.* By JOHN MURRAY, *M.D. F.R.S.E.*

[Concluded from p. 25.]

THE question now remains for consideration, What is the real composition of sea-water? How far are the salts obtained in either mode of analysis, those which exist originally in solution? This question is evidently to be considered under the same point of view as that which I have illustrated in a former paper, with regard to the change in the state of combination, which may be produced in the saline compounds existing in mineral waters, by the analytic operations to which they are subjected. We have no strict evidence that the binary compounds which are obtained are those which existed in solution, admitting even the principle that binary combinations exist. On the contrary, there is every probability that the substances obtained are often products of the operation, arising from changes of combination which it established. And this is even placed in a more striking point of view in the present case, as the ingredients obtained are actually different, when different methods of analysis are employed. It is, therefore, necesssary to inquire further what the real composition is.

With regard to the sulphate of lime, which is the first substance separated by the evaporation, the general views I have already stated, give every probability to the conclusion, that it is a product of the operation formed by the action of sulphate of soda or of magnesia, during the evaporation, on muriate of lime; that this last salt, therefore, is an ingredient in sea-water; the

the proportion of it being the quantity equivalent to that of the sulphate of lime which the analysis affords.

The small portions of carbonates of magnesia and lime which are obtained in the evaporation, I have already stated, are evidently accidental products, from the decomposition of muriate of magnesia and muriate or sulphate of lime.

The large quantity of muriate of soda leaves no doubt, if binary compounds at all exist in a state of solution, that it is the chief ingredient in sea-water, though the quantity of it may be affected by the changes which occur from the actions of some of the other salts.

A similar conclusion is to be drawn with regard to the muriate of magnesia. Though the proportion of it may be affected by the changes which occur in the analytic operations, still, from the quantity in which it is obtained, a considerable part of it must originally exist.

The principal difficulty is with regard to the sulphate of magnesia, and the sulphate of soda. It has always been supposed, that sulphate of magnesia is an ingredient in sea-water, from its being procured by evaporation; and it is possible that it may be so. But it is just as possible, *a priori*, that sulphate of soda may be the original ingredient, and that, during the evaporation, the mutual action between it and muriate of magnesia is favoured by the concentration, whence portions of both are decomposed, and corresponding quantities of sulphate of magnesia and muriate of soda are formed. Nor is there any thing connected with the mere results themselves, which proves which of these views is just.

If the appeal be made to experiment, it is sufficiently established, that sulphate of magnesia may be formed by the action of sulphate of soda on muriate of magnesia. When these two salts are boiled together in solution, a double decomposition takes place at least partially, and portions of sulphate of magnesia and muriate of soda are formed. On the other hand, the reverse combinations may also, acccording to circumstances, be established. We have seen reason to conclude, that they are so from the agency of alcohol; and even in an aqueous solution, when certain proportions of the salts are used, they appear, under some circumstances, particularly that of a low temperature, to take place to a certain extent. But still these facts only show what decompositions may occur from evaporation or other processes; they do not prove what the actual state of combination is in the original solution.

It is obvious, that this is merely a case belonging to the more general question, What is the state of combination in a compound

pound saline solution, and on what principle can it be determined what are the binary compounds that really exist in it?—a question of considerable importance, but one at the same time of very difficult determination.

When it is admitted that this cannot be inferred with certainty from the actual products of analysis, the next most obvious view is, that it may be inferred from a knowledge of the real forces of affinity, as, according to these, certain binary compounds must be formed; and, as the state of the science does not afford any certain estimate of the strength of attraction, the problem, it may be concluded, is at present incapable of being solved.

This conclusion, however, is by no means certain. Attraction is so much modified in its operation by external forces, and combinations are so frequently established from the influence of these, that it is not clear that we should be able to determine what combinations would exist in cases similar to those connected with the present investigation, from a knowledge of the degrees of attraction, were we even in possession of it. It is rather, perhaps, from a knowledge of the influence of these external forces, that an approximation to the solution of the problem is to be attained; and an extension of the principle I have illustrated in the preceding part of this paper, it appears to me may throw some light on the question.

If the force of cohesion has so much power in modifying chemical attraction, as to change its results, and establish combinations independent of the relative degrees of strength with which it is exerted; and if the reverse of cohesion, that is, the power of a solvent, operates in establishing the reverse combinations, as, in considering the agency of alcohol in this analysis, there has appeared sufficient reason to conclude, then it will follow, that as in a concentrated medium the least soluble compounds are formed, so in a dilute medium the more soluble compounds will be established. The power of the solvent is exerted with greatest effect on those which are most soluble; and hence, if the reverse combinations even existed, this power must change them, and establish the others, precisely as the power of cohesion acts with most energy on those which are least soluble, and thus causes their formation, when it is brought to act with sufficient force. Hence will follow the simple rule by which the state of combination may be determined; that, in any fluid containing the elements of compound salts, the binary compounds existing in it will be those which are most soluble in that fluid; and the reverse combinations will only be established by its concentration favouring the influence of cohesion. Thus, if we concentrate a solution containing sulphuric and muriatic acids, soda and lime, we know that, from the influence of cohesion, the binary

binary combinations will be those of sulphuric acid with lime, and of muriatic acid with soda. And on the same principle, we may infer that, in a dilute solution containing these elements, the combinations will, from the influence of the power the reverse of cohesion, that of the solvent action of the liquid, be those of sulphuric acid with soda, and muriatic acid with lime. In a concentrated solution, containing muriatic and sulphuric acids, soda and magnesia, sulphate of magnesia and muriate of soda are formed; and, on the same principle, in a dilute solution, there must exist sulphate of soda and muriate of magnesia.

This principle, if just, is an important one, as enabling us to determine the state of binary combinations in a saline liquor. I add, therefore, one other illustration of the reasoning on which it rests.

Suppose that in a compound saline solution, that is, one containing more than one acid and one base, the acid and the base which have the strongest attraction, are those which are most soluble, or form the most soluble compound; the solvent power of the liquid operating at the same time, will concur with this, and favour their combination; and any other acid and base likewise present, will of course, at the same time, combine. But suppose the more powerful attraction to belong to those which form an insoluble compound, the solvent power counteracts this, and prevents the combination. And the more this power is increased, which is done by increasing the quantity of the solvent, the more will this be counteracted. The reverse combinations will therefore be established by the operation of the opposite affinities. Hence, generally speaking, in a dilute solution, the binary combinations must be those which form the most soluble compounds, and very powerful attractions would be required to counteract this.

Applying this principle to the composition of sea-water, or rather to the question with regard to the sulphate of soda and sulphate of magnesia, it is obvious, that the former is to be considered as the original ingredient, and the latter as a product of the evaporation; for muriate of magnesia and sulphate of soda are, on the whole, more soluble in water than muriate of soda and sulphate of magnesia. On the same principle it follows, still more unequivocally, that the lime exists in the state of muriate of lime, with a portion of sulphate of soda equivalent to the quantity of sulphate of lime which the evaporation affords. The salts, therefore, really existing in sea-water, are muriate of soda, muriate of magnesia, muriate of lime, and sulphate of soda. The quantity of muriate of soda is less than what is obtained by evaporation, for a portion of it is formed by the decompositions which occur; the quantity of muriate of magnesia is larger, as a portion a portion of it is decomposed; the quantity of muriate of lime is inferred from the quantity of sulphate of lime; and the quantity of sulphate of soda is determined from the quantities of sulphate of magnesia and sulphate of lime obtained. The proportions may thus be easily assigned. Referring to the preceding analyses, the proportions in a pint, according to this principle, will be the following:

According to the first analysis,

Muriate of soda,	..	170·2 grains.
——— magnesia,	..	30·6
——— lime,	..	5·8
Sulphate of soda,	..	21·9
		228·5

According to the second analysis:

Muriate of soda,	..	165·2
——— magnesia,	..	31·6
——— lime,	..	5·9
Sulphate of soda,	..	24·9
		227·6

If the opposite view be adopted, that the sulphate existing in sea-water is not sulphate of soda, but sulphate of magnesia; then the ingredients and their proportions will be as follow:

According to the first analysis,

Muriate of soda,	..	188·3 grains.
——— magnesia,	..	16
——— lime,	..	5·8
Sulphate of magnesia,	..	18·4
		228·5

According to the second analysis,

Muriate of soda,	..	185·6
——— magnesia,	..	15·2
——— lime,	..	5·9
Sulphate of magnesia,	..	20·9
		227·6

But this view rests on no principle, and is, as I have stated, less probable than the other*.

The

* There is sometimes obtained in the large way, from the products of the evaporation of sea-water, a triple salt, which has not been noticed by chemists, but which appears to be of definite composition, and is distinguished by peculiar properties,—a sulphate of magnesia and soda. It is formed in purifying the sulphate of magnesia procured by the first evaporation from the bittern of sea-salt. In this process the sulphate, which is impure, both from the

The difficulties attending the perfect separation of compound salts from each other, by crystallization, even with the aid

the intermixture of muriate of soda and muriate of magnesia, and perhaps, also, of sulphate of soda, is dissolved in water, and by evaporation and cooling is obtained crystallized; a fresh quantity of it is added to the residual liquor, and by the necessary evaporation and cooling, a new crystallization is produced; this is repeated for a third or fourth time; and it is in these latter crystallizations that this triple salt is formed, frequently in considerable quantity, and usually at a high temperature, being precipitated even in the boiler. It crystallizes in rhombs, at first irregular and semitransparent; but by solution in water, and a second crystallization, is obtained in more regular rhombs, truncated on the acute angles, on the obtuse angles and edges, and on the terminal edges, considering the rhomb as a four-sided prism, and transparent. The crystals are permanent in the air; they are soluble in little more than three times their weight of water, at the temperature of 60°; they do not undergo the watery fusion from heat, but suffer decrepitation. In these properties, this salt differs entirely from sulphate of soda, or sulphate of magnesia.

To determine its composition, 20 grains reduced to powder were exposed to heat, raised gradually nearly to redness; they lost from the escape of water 5·6 grains. The residual powder was dissolved in water, and muriate of barytes was added as long as any precipitation was produced. The precipitate dried at a red-heat, weighed 23·9 grains, equivalent to sulphuric acid 8·2 grains. To the clear liquor carbonate of ammonia was added, which did not impair the transparency; phosphoric acid was then dropped in, which produced a copious precipitation. The precipitate, calcined at a red-heat, weighed 5·3 grains, equivalent to 2·1 of magnesia, or 6·4 of sulphate of magnesia; the residual liquor being evaporated to dryness, the dry mass was submitted to heat, gradually raised, as long as any vapours exhaled; it afforded, by solution in water and evaporation, muriate of soda in cubes, which, after exposure to a red-heat, weighed 6·4 grains, equivalent to 7·8 grains of sulphate of soda. 100 grains of the salt, therefore, afford of

Sulphate of magnesia, - - -	32 grains.
——— soda, - - - -	39
Water of crystallization, - -	28
Loss, - - - - - - - - -	1
	100

It afforded also a slight trace of muriatic acid; its solution being in a very slight degree rendered turbid by nitrate of silver, probably owing to the intermixture of a little muriate of soda, as an extraneous ingredient. This accounts for the proportion of sulphuric acid, as inferred from the quantities of the bases, being a little larger than that directly obtained by the precipitation by muriate of barytes.

The difference of crystalline form, as well as other differences of properties, in the salt, from those either of sulphate of soda or sulphate of magnesia, sufficiently prove that it is not merely an intermixture of the two, but that it is of definite composition. It deserves to be remarked, too, that it has not the same relation to water that either of these salts has, or any mean between them; the quantity of its water of crystallization being considerably less. Its taste is much less disagreeable than that of sulphate of soda or sulphate of magnesia; it might therefore probably be introduced with advantage as a purgative salt, especially as it could be procured at a low price; and from its composition, it would afford a very good substitute for the aperient mineral waters, which usually owe their activity to sulphate of soda and sulphate of magnesia.

of

of the action of alcohol, either as a solvent or a precipitant, are so great, that analyses executed in this mode can scarcely be perfectly accurate. And as it appears, if the preceding observations are just, that there is no certainty in the conclusion that the products of analysis by evaporation or crystallization are the real ingredients, no peculiar advantage in this respect belongs to this method, and just as much information is obtained by discovering the acids and bases which exist in solution, and then inferring, according to the most probable view, what the states of binary combination are in which they exist. This kind of analysis has the advantage, that it can be executed with much more precision than the other: it is liable to fewer sources of error, and, by finding the quantities, not of the compounds, but of the elements, any error that is introduced is discovered, when the binary compounds are inferred. To ensure accuracy, therefore, it was desirable to apply it to the illustration of the present subject, more especially as the preceding analyses, though they do not differ greatly in the results, still, from these difficulties, do not exactly correspond.

Different methods might be employed. The following is the one I have preferred.

To a pint of sea-water, reduced by evaporation to nearly one-fourth, at which state of concentration no crystallization nor deposition takes place, muriate of barytes was added as long as any precipitation occurred. By a preliminary trial, it was found that the precipitate gives no effervescence with muriatic acid, nor suffers any change. It was therefore sulphate of barytes. Dried by a low red heat, it weighed 43 grains, equivalent to 14·4 *sulphuric acid.*

By this step the whole salts in the sea-water were converted into muriates. It remained to discover and estimate the quantities of their bases.

To the clear liquor, oxalate of ammonia was added as long as any turbid appearance was produced. The precipitate, washed and dried, by a heat of 150° continued for two hours, weighed 8·5 grains. Calcined with a low red-heat, it gave of carbonate of lime 5·2 grains. This, dissolved with strong effervescence in dilute muriatic acid, and the product being heated with sulphuric acid, gave sulphate of lime, which, after exposure to a red-heat, weighed 7 grains, equivalent to 2·9 of pure *lime.*

To the clear liquor warmed, carbonate of ammonia was added, and phosphoric acid was dropped in*; an abundant precipitation took place of phosphate of magnesia and ammonia, and additional portions of the phosphoric acid, with such additions

* I shall have to state in a subsequent paper, the peculiar advantages attending this method of estimating the magnesia.

of the carbonate as were necessary to preserve an excess of ammonia in the liquor, were added, as long as any precipitation was produced. The precipitate was converted, by calcination for an hour at a red-heat, into phosphate of magnesia. This weighed 37 grains, equivalent to 14·8 grains of *magnesia.*

The clear liquor was evapora ed to dryness, and the dry mass was exposed to a heat gradually raised to redness, to expel the muriate of ammonia formed in the preceding operations. Muriate of soda remained, which weighed 180·5 grains, equivalent to 96·3 of soda, and 84·2 of muriatic acid.

This gives the quantity of *soda* contained in the sea-water; but it does not necessarily give the quantity of muriatic acid; for if more of this acid be present than what the soda can neutralize, combined with portions of any of the other bases, (and from the former analysis this appears to be the case,) this quantity will be combined with ammonia in the preceding steps of the analysis, and is of course dissipated in the state of muriate of ammonia.

This will appear, and the quantity be discovered by ascertaining what proportion of these bases the quantity of sulphuric acid obtained by the analysis is capable of neutralizing, thus finding if any excess of them remain; and, from the quantity of this discovering the quantity of muriatic acid, which would be requisite for saturation, which of course is the quantity lost. 2·9 of lime, the quantity of this earth obtained by the analysis, neutralize 4·1 of sulphuric acid; this deducted from 14·4, the quantity obtained, leaves 10 3, to neutralize which, 5·1 of magnesia are required; this deducted from 14·8, the quantity of magnesia, leaves 9·7 of that earth; to neutralize this, 13·5 of muriatic acid are required; and this added to the 84·2 in the muriate of soda, gives 97·7 grains as the total quantity of *muriatic acid.*

The elements, then, of the salts, in a pint of sea-water, are, by this analysis,

Lime, ...	2·9 grains.
Magnesia, ...	14·8
Soda, ...	96·3
Sulphuric acid, ..	14·4
Muriatic acid, ...	97·7
	226·1

The proportions of the compound salts may be assigned from these, according to whatever view may appear most probable, of the state of combination in which they exist in sea-water, and thus the results may be compared with those of the former analyses.

Thus, supposing the elements to be combined in the modes in which

which they are obtained by evaporation, that is, as muriate of soda, muriate of magnesia, sulphate of magnesia, and sulphate of lime; the proportions of these salts in a pint, will be

Muriate of soda,	..	180·5 grains.
——— magnesia,	..	23·
Sulphate of magnesia,	..	15·5
——— lime,	..	7·1
		226·1

Supposing that the lime exists as muriate of lime, (which is the most probable conclusion with regard to it,) and further supposing, that the sulphuric acid exists in the state of sulphate of magnesia, the proportions will be

Muriate of soda,	..	180·5 grains.
——— magnesia,	..	18·3
——— lime,	..	5·7
Sulphate of magnesia,	..	21·6
		226·1

Or, lastly, supposing that the sulphuric acid exists in the state of sulphate of soda, the proportions will be

Muriate of soda,	..	159·3 grains.
——— magnesia,	..	35·5
——— lime,	..	5·7
Sulphate of soda,	..	25·6
		226·1

These proportions differ somewhat, though not very materially, from those found by the other modes of analysis. The principal differences consist in the quantity of magnesia and of sulphuric acid being rather larger. This is evidently to be ascribed to the modes of detecting sulphuric acid by barytes, and magnesia by phosphoric acid and ammonia, being so perfect, that the entire quantities of them are found; while, in the other modes, from the difficulty of effecting the entire separation of salts from each other, a small portion of sulphate of magnesia, or of muriate of magnesia and sulphate of soda, had remained with the muriate of soda; and though subcarbonate of soda was employed to decompose them, this decomposition is not altogether perfect. In the mode of analysis, too, by reagents, the presence of water in the products can be more completely excluded; and to this, probably, is to be ascribed the absolute quantity of saline matter being a little less according to this analysis, than it is in the others *.

Of

* In another analysis of sea-water, in which subcarbonate of ammonia was employed to precipitate the magnesia, a solution of it being added to the

Of the different views which may be taken of the state of combination of the elements, I have already inferred, that the one which supposes the sulphuric acid to exist in the state of sulphate of soda, is the most probable; and as the mode of analysis by reagents is the most accurate, the last table may be considered as that which exhibits the highest approximation to the real composition of sea-water, both with regard to its ingredients, and their proportions.

I had proposed to add a few observations on the analysis of salt-brines; but as they are merely applications of the principles already illustrated, it is sufficient to state them briefly, or to notice those which present rather striking results.

Klaproth has given a laborious investigation of the nature of these brines; in the greater number of them, he states as ingredients, muriate of soda, muriate of magnesia, muriate of lime, and sulphate of lime. It is obvious that there are no just grounds whence this composition can be inferred; it is much more probable, that sulphate of soda is the ingredient, and that, acting on a portion of the muriate of lime, it forms sulphate of lime.

In other analyses,—in those, for example, of the salt-brines of Lorraine, by Nicholas *,—muriate of soda, sulphate of soda, sulphate of lime, muriate of magnesia, and muriate of lime, are enumerated as ingredients. Here it is still more evident, that there is no proof of the previous existence of sulphate of lime; on the contrary, as both muriate of lime and sulphate of soda are present, they must, in the concentration of the liquor by evaporation, form by their mutual action muriate of soda and sulphate of lime; and the quantity therefore of this sulphate which may be obtained, must have this origin.

the water concentrated by evaporation, the clear liquor, after the subsidence of the precipitate being evaporated to dryness, the saline matter being exposed to heat, to dissipate the muriate of ammonia; being redissolved in water the subcarbonate of ammonia again added, and this repeated for a third, and even a fourth time, the results gave the following proportions of the elements:

Lime, ...	2·9 grains.
Magnesia, . . .	13·
Soda, ...	97·6
Sulphuric acid, .	15·2
Muriatic acid, .	96·9
	225·6

The principal difference here, is the proportion of magnesia being somewhat smaller, evidently owing to its precipitation by the carbonate of ammonia, even with the aid of the methods employed to promote it, being imperfect.

* *Ann. de Chimie*, t. xx.

In

In general, when muriate of lime and muriate of magnesia are stated as ingredients with sulphate of lime, no sulphate of soda or sulphate of magnesia is found. The reason is obvious, that if either of these salts existed, it would react on the muriate of lime, and form sulphate of lime. But when muriate of lime is not found, sulphate of magnesia, or sulphate of soda, is often stated as an ingredient, obviously owing to the circumstance, that although a portion of muriate of lime has been present, so as to form sulphate of lime, there has not been a quantity sufficient to decompose the whole sulphate of soda, or sulphate of magnesia.

A striking example of these facts is to be found in Dr. Henry's analysis of the different varieties of sea- and rock-salt *. In four varieties of rock-salt, there were found small quantities of muriates of lime and magnesia, with a portion of sulphate of lime, but no sulphate of magnesia; while in the different varieties of sea-salt, British and foreign, there was no appreciable quantity, and in some of them no trace whatever of muriate of lime, but in all of them with sulphate of lime considerable quantities of sulphate of magnesia. In the latter, therefore, the muriate of lime had been converted entirely into sulphate of lime from the excess of sulphate of magnesia; in the former, from the deficiency of the sulphate, a portion of the muriate of lime had remained undecomposed.

A result somewhat similar, and which affords a very direct application of the same principles, is stated by Mr. Horner, in his analysis of the salt-brine at Droitwich, compared with that of Cheshire †. The latter contains a little muriate of lime; the former contains none. But, then, that of Droitwich contains sulphate of soda and sulphate of lime; there is every probability, therefore, that its muriate of lime has been converted into sulphate of lime by the sulphate of soda, which is in excess; while in the Cheshire brine, as there is no sulphate of soda in excess, that is, none after the evaporation, a portion of muriate of lime remains.

There is a singular fact stated by Dr. Henry with regard to what is called *fishery salt*, prepared from salt-brine, which seems to admit of explanation only on these views. He found the proportion of sulphate of lime mixed with it to be less, as it was collected at a later period of the evaporation; that drawn from the boiler, after two hours application of the heat, contained in 100 parts 16 of sulphate of lime; that after four hours contained only 11; and that after six hours only $3\frac{1}{2}$. Now if the water of this brine held sulphate of lime in solution, the sulphate wou'd begin to be deposited when the quantity of water was diminished

* Philosophical Transactions, 1810. † Geological Transactions, vol. ii.

 to

to that extent that it was unable to retain the whole dissolved; and, in the progress of the evaporation, would continue to be deposited proportional to this, to the end of it, and the muriate of soda would be deposited in the same manner; so that the proportion between the two would continue nearly the same. But if the sulphate of lime did not exist in solution, but derives its origin from the action of sulphate of soda on muriate of lime, which these brines contain, this action would take place, when a certain degree of concentration of the liquor had been attained; the sulphate of lime would then be copiously deposited; but as the evaporation continued to proceed, its quantity would be diminished, as the quantity either of sulphate of soda, or of muriate of lime, became less; and its deposition would cease when either of these salts was exhausted.

This is placed in a still clearer light, by an analysis of these brines, after evaporation, to a certain extent, compared with their original composition. A brine from Northwich was found by Dr. Henry to afford, by evaporation, saline matter, which, he inferred, contained in 1000 parts, muriate of lime and muriate of magnesia in nearly equal proportions 5 parts, sulphate of lime 19 parts, muriate of soda 974 parts. But the brine remaining after the separation of all the common salt, which it is thought worth while to extract, afforded saline matter by evaporation, which he found to contain, in 1000 parts, muriate of magnesia 35, muriate of lime 32, sulphate of lime 6, muriate of soda 927. Here, in the progress of the evaporation, the quantity of sulphate of soda, which may be considered as an original ingredient of the brine, had been diminished by the decomposition arising from its action on the muriate of lime. The liquor, therefore, after this, afforded by further evaporation, along with a large quantity of muriate of lime, a small quantity only of sulphate of lime; while, if this sulphate had been an original ingredient, it would have continued to be afforded at least in equal proportion.

Something similar to this occurs in the evaporation of sea-water. It is after a certain extent of evaporation, but while a large portion of liquor still remains, that the precipitation of sulphate of lime takes place; that is, after the concentration is sufficient to favour the mutual action of the sulphate of soda, or sulphate of magnesia, and muriate of lime. After this, the quantity diminishes as the evaporation proceeds, till at last not a trace of it, or of sulphate of lime, remains in the bittern, which consists of muriate of soda, muriate of magnesia, and sulphate of magnesia alone. This curious fact has not been particularly noticed, though it is in consequence of it that magnesia is prepared from bittern on the large scale, perfectly pure.

All

All these facts seem scarcely to admit of any explanation, but on the view that has been stated, and they afford a strong confirmation of it.

XVI. *On the Identity of Water-Spouts and Whirlwinds* *.

SIR, — IF you think the following remarks relative to whirlwinds, or water-spouts, worthy of a place in your Journal, you will oblige me by their insertion; as the opinions of travellers, and also of philosophers, differ greatly concerning this natural phænomenon, and any information afforded, by attentive observation, may therefore be interesting, if not useful.

AN old stager, in the last number of the Naval Chronicle, seems to be of the opinion of Theophilus Lindsay, and some other philosophers; viz. that in the phænomenon called a water-spout, the water *descends* in columns from the clouds upon the earth or sea, and does not *ascend* from the sea upward to the clouds, which I believe to be the common opinion.

To corroborate his opinion, this writer gives an extract from a Scotch newspaper, stating, that a water-spout had descended and done considerable damage in a part of that country.

In stormy weather, when the barometer is low and the atmosphere light, if clouds which contain much moisture happen to impinge against any of the hills of an alpine country, they are certainly liable in such case to discharge their contents in *heavy rains*, which descending rapidly from the summits of the hills, rush with irresistible force down the valleys, carrying every thing before them; and these local discharges of heavy rain are commonly called *water-spouts* by the neighbouring inhabitants. The Hawkesbury river in New South Wales is sometimes subject to a rise of from twenty to thirty feet above the natural level, by the sudden rupture of clouds on the summits of the Blue Mountains. About thirteen years ago a phænomenon of this kind happened at St. Helena, when a cloud suddenly broke upon the hill that forms the head of Rupert's valley; and although the bed of this valley is generally dry, the immense body of water that rushed through it at this time, bore down the strong line of stone ramparts, and carried some heavy pieces of artillery into the sea.

I think (although the last number of the Naval Chronicle is not now before me) his correspondent considers the water-spout seen at sea to be a similar, if not the same phænomenon as this last mentioned, except that the white column in the centre of the spout he considers to be a congregated mass, or body of

* From the Asiatic Journal, No. 23.

of water, descending from the clouds to the sea. Now, as many water-spouts are of great diameter, I am decidedly of opinion, that if the central white column were a *body of falling water* upon the surface of the sea, its noise would be heard many miles, if not many leagues, like the falls of Morency and Niagara, and would sink, or destroy, any unfortunate ship which happened to come in contact with its vortex; but, my experience compels me to think otherwise, as I never heard the noise of any water-spout until very close to it, and then, the noise resembled that of steam issuing through a small aperture of a boiler, occasioned by the whirlwind's rapid motion in disengaging water in the gaseous form from the surface of the sea: besides, if the central white column were a mass of falling water, its diameter ought to increase by the resistance of the atmosphere in descending, and consequently be greater near the sea than higher up towards the cloud; but this probably never happens, as the diameter of a water-spout, as well as the interior column, is greatest near the impending cloud, and converges towards the sea. That whirlwinds, or water-spouts, may often differ much in formation and appearance, I believe there can be little doubt; but I have certainly more than once, both by ocular and tangible observation, been convinced, that a whirlwind and water-spout are sometimes one and the same phænomenon. At one time, when dense clouds, charged with electric matter, approached the ships in Canton river, a regular water-spout was formed by a tube descending from the cloud in the usual manner, and the whirlwind turned one of the ships round at her moorings. As this whirlwind passed over the island, close to the village of Whampoa it unroofed several thatched houses, and tore the leaves from the trees, which were carried up a considerable way into the atmosphere by the whirlwind, and at this time it had a dense appearance; but as soon as it drifted over the land and came in contact with the water of the river, the white tube became very conspicuous in the centre of the whirlwind, and the water seemed to be torn from the surface of the river and carried upwards in small particles by the whirlwind. Had any light terrene bodies been floating in the river at this time, in the path of the whirlwind, they certainly would have been drawn upward like those which came into its vortex when it passed over the land. This was certainly an example of the unity of a whirlwind and water-spout. At another time a regular-formed water-spout was driven along by the wind till its exterior surface nearly touched the quarter of our ship, when I plainly saw the water disengaged from the surface of the sea with a hissing noise, and carried upward in the gaseous form by the ascending whirlwind, while the vacuum, or cavity, in its centre, was very distinct, with heavy drops of rain falling down

both

both from the interior and exterior sides of the ascending spiral, where it was evident the power of the whirlwind was not capable of carrying all the gaseous particles up into the cloud. When we were close to this water-spout the white tube in the centre was not visible, but only a vacant column, as mentioned above; which column, had we been a quarter or half a mile off, would probably, by an optical illusion, have appeared, as usual, like a white column of water.

In the straits of Malacca I have sometimes seen upwards of a dozen water-spouts at the same time, and have been near to several. Once I passed through the vortex of a whirlpool produced by a water-spout beginning to form; it was directly under a dense cloud, from which an inverted conical tube was descending when we passed through the whirlpool in the ship: this was about twenty or twenty-five yards in diameter, and the water was carried round by the force of the whirlwind over it, with a velocity of about from three to four miles an hour, breaking in little waves with a hissing noise, by a portion of those waves being torn away in the form of white vapour. I felt a pleasing sensation at the time, expecting when passing through the vortex of an incipient water-spout, to be a close observer of it completely formed; but whether the communicating force was destroyed by the ship passing through the vortex, or from a deficiency of strength in the whirlwind, or from some other cause, a dispersion of the phænomenon soon followed.

It would be needless to adduce more examples to exhibit the affinity of the common water-spout, as observed at sea, and the whirlwind; but I fully agree with the assertion, that there are various kinds of whirlwinds, and, perhaps, also of water-spouts; both the former and the latter, as has been observed, happen sometimes in this country. On the 27th June last, a remarkable case of the affinity of the water-spout and whirlwind was observed by many persons in the vicinity of London, among whom was the editor of the Monthly Magazine, and a description of this phænomenon is recorded in the Philosophical Magazine, No. 232, vol. 50. When it happened, very dark clouds had collected over the adjoining country, and some stormy rain accompanied by several strokes of lightning followed this hurricane of wind.

The correspondent of the Naval Chronicle says, whirlwinds occur very frequently when the clouds are high, the sun shining, and the wind light; but, although whirlwinds do certainly happen at these times, yet they seem more dangerous and terrific in their appearance when accompanied by dense and stormy clouds. I once observed a whirlwind upon the coast of Coromandel during a warm day, when there was little wind and no clouds, which

carried

carried up a column of sand a great way into the atmosphere; and if it had passed from the land to the surface of the sea, it no doubt would have carried the water upward in the gaseous form, and probably a cloud would have appeared over it.

Whirlwinds of a minor kind may be perceived almost daily; but these are only *eddies* of wind produced from obstructions of hills, cliffs, buildings, &c. to its regular course, and similar to whirlpools or eddies, in a river or strait, occasioned by the prominent parts of the land.

Another kind of whirlwind like those last mentioned, is sometimes experienced to blow from valleys or over high cliffs, down upon the sea. Although this, as he remarks, may not happen in Gibraltar Bay, or in Table Bay at the Cape of Good Hope, yet in sailing close to high cliffs among the Eastern Islands, I have several times seen whirling gusts of wind descend and rebound from the surface of the sea, carrying the water in their vortex several fathoms upward in the form of spray.

Previous to concluding these remarks, it may not be irrelevant to advert to the opinions of some of those who have written in early times on meteorology. Pliny, in his Natural History, describing a sudden blast of wind or typhon, says, 'there riseth also upon the sea a dark mist resembling a monstrous beast, and this is ever a terrible cloud to sailors. Another likewise called *column* or *pillar*, when the vapour and water engendered is so thick and stiff congealed, that it standeth compact of itself. Of the same sort, also, is that *cloud which draweth water to it*, as it were into a long pipe.'

Aristotle, in his third book on meteors, describes some of the causes of whirlwinds or typhon, and mentions that there are both descending and ascending whirlwinds. Olympiodorus, his commentator, in reference to Aristotle's definition of these words, says, 'and thus through continued vibrations, a spiral and involution of the wind is formed, proceeding from the earth as to a cloud, and elevating any body with which it may happen to meet—on the sea indeed ships, but on the earth animals or stones, or anything else which the half blow again suffers to tend downward. This involution Homer calls *thuella*, but Aristotle *typhon*, in consequence of vehemently striking against as it were, and breaking solid bodies. Sailors, however, call it syphon, because like a syphon it *draws upward* the water of the sea.'

If, however, it is produced from a cloud, it originates as follows: the cloud being on all sides condensed and inwardly compressed, fuliginous exhalation becoming inwardly multiplied and evolved in a multiform manner, the cloud, from the violence is suddenly burst, and the inwardly evolved fuliginous exhalation, proceeds out of it, preserving the same form which it had within in, viz. the spiral form. Afterwards the spiral thus tends to the earth like hairs that are curled, not from the imbecility of the secreting power, but from the pores being winding through which it proceeds, and from its being fashioned together with them. And these, indeed, are the causes why the spiral of the typhon at one time proceeds *upward from beneath*, and at another *downward from on high*. But the knowledge of these is two-fold; for we know whether the spiral is moved upward from beneath, or downward from on high, and in the first place indeed from the sight itself. For since the spiral, viz. the typhon, is evident to the sight from the density of its parts, when we see it at one time proceeding downward, and at another upward, we say that the beginning of the spiral is from beneath; but if it is alone moved downward from on high, then it must be said that the beginning of it is from on high. In the next place, we know this from the bodies which are hurried away and elevated by the spiral. For, if the body is first turned from its proper position, and afterwards is moved obliquely and then elevated, we say that the typhon originates from on high.—Your obedient, &c.

October 10, 1817. J. H.

XVII. *On the Origin of Hot Springs. By* Gavin Inglis, *Esq.*

To Mr. Tilloch.

Sir, — In your truly valuable Magazine for November last, page 352, in a note to the paper on the Purification of Mercury, the hot waters of Lucca, Pisa, and Bath, are mentioned with the following remark: "There are still persons who ascribe the hot baths to the influence of volcanoes; but there is not the smallest trace of any thing like volcanic matter, or even any combustible substance, to be discovered within many miles of them. Nothing that, either chemically or geologically speaking, could sanction the belief, that they owe their warmth to exhausted subterraneous volcanoes."

It is certainly a matter of no small importance to science, to ascertain the true cause of such phænomena, and a great source of contemplative amusement to minds inclined to study the wonderful operations of Nature, in the production of hot and even boiling waters from the bowels of the earth, of volcanoes vomiting fire in all its dreadful forms, accompanied with scoria and fused minerals of every description found in the vast cavities or great furnace of the mountains.

I have long amused myself with ideas regarding the cause, intermission and prolongation of volcanoes; but whether these correspond

respond with, or differ from, the opinions of others, I know not; as I do not remember to have even seen any cognate views or sentiments published upon this subject. I therefore beg leave to submit the outlines of my cogitations to your judgement; and if you consider them worthy of a place, your inserting them will confer a favour.

I do not conceive an effort of Nature equal to the production of volcanic matter necessary to bring forth hot or boiling water from the interior of the earth; the temperature and properties of *all* springs must depend on the arrangement and quality of the superior and intervening strata, the porosity or scissures of the various layers, and the nature and quantity of the metals contained in these strata. I hold all mineral stratification, whether perpendicular, inclined or horizontal, on the mountain or in the valley, as so many natural Galvanic piles, whose powers of decomposition and deflagration must correspond with the number and composition of the natural plates:—hence the graduation of temperature and properties in hot, boiling or mineral waters issuing from these stratifications. Where the stratification presents a pile of diminutive powers, a partial decomposition only, without deflagration, takes place. The oxidable minerals take up the oxygen, leave the hydrogen at liberty to act on the iron, the sulphur, the carbon, or whatever comes in its way: hence the sulphuretted and carburetted hydrogen gases that accompany these waters, and the iron in chalybeates;—or the hydrogen appears in the horrifying shape of inflammable gas in coal-mines or other underground workings; or escapes through the loose superior strata into the upper regions of the atmosphere, loaded with its mineral spoils, where, exploding, it produces thunder and rain, and the elements, simultaneously collapsing to fill up the vacuum, force into the centre the mineral particles carried thither by the hydrogen, in a state of extreme ignition, and produce the meteoric stones, &c. that fall from the clouds.

While the diminutive piles are only producing hydrogen, mineral waters, hot-baths and boiling springs; the great strata or major piles are at work in the interior of the different regions of the globe, and their effects brought to light by mounts Hecla, Etna, Vesuvius, Strombolo, &c. And the deflagration of metals and minerals produced by the artificial pile on the small experimental scale of the laboratory, are here produced in all the awful majesty and glory of nature, infinitely beyond the reach of human effort. The intermitting of volcanic mountains must depend on the quantity of water contained in, or connected with, the mountain, the component parts of which I conceive to be the grand reservoir of combustion. When the volcano ceases to flame, it is when its anterior struggles have exhausted and dried

up

up all its waters; it must then cease to act, until the mountain is cooled down and receives a fresh surcharge of water, when it may again be brought into action by its own Galvanic electricity.

The eruptions of Mount Hecla are always most tremendous after long frosts: when the mountain is, by the casement of ice and frozen snow, hermetically sealed against the escape of hydrogen through the sides and pores of the mountain, thereby adding to the ordinary stock of combustible materials, the unavoidable consequence of increased ignition takes place.

That the prolongation of volcanoes depends on the support of combustion thus supplied, must appear evident when we consider the immensity of any other inflammable substance that would have been necessary.

Had the island of Sicily consisted of coal, Etna would have consumed the whole long before this.

Had Strombolo been a compound of the most œconomical combustible materials, it could not have existed even at the time it is first noticed by the writers of antiquity: the continued uniformity of that volcano must be caused by its connexion with the waters of the Mediterranean. Its flame I consider a mere *gas-light*, from a grand laboratory of nature at work for ages, filling up the immense cavities from whence the materials composing the mountains themselves have been drawn, with perhaps new and previous combinations, or with the soda of the waters thus deflagrated. It is not impossible that the prodigious salt-rocks and mines found in different places of the earth, may owe their origin to such a cause; and may not these cavities now filling up, be to some remote successors of the human race, sources of emolument and wealth? Earthquakes I also conceive to be the consequence of decomposition and deflagration by these Galvanic stratifications: but to what distance their influence may extend it is impossible to conceive. The agitation of some lakes in Scotland during the great earthquake at Lisbon must have been occasioned by connected strata. The troubling of the waters of Loch-Leven during and at the precise time of the shock at Inverness, must have been connected with a similar cause.

I remain, yours truly,

Jan. 3, 1818. GAVIN INGLIS.

XVIII. *On the Probability of Meteorolites being projected from the Moon. By Mr.* J. ACTON, *and* CAPEL LOFFT, *Esq.—Galvanic and Electric Experiments by Mr.* ACTON.

To Mr. Tilloch.

Ipswich, Jan. 8, 1818.

SIR, — IN consequence of a communication I made to my respected friend Mr. Capel Lofft relating to the meteor I observed

on

on the 8th ultimo, (an account of which you have kindly inserted in your valuable Magazine of last month,) an interesting conversation took place between us upon our meeting soon afterwards, in which the probability of meteorolites being projected from the moon was amongst other things discussed; from which subsequently resulted the inclosed interesting letter, affording me much gratification; and, as being replete with intelligence and ingenuity, will, I trust, be considered as deserving a place in your next number. Notwithstanding the subject has frequently arrested the attention of several eminent philosophers, who have deemed it no stretch of belief to account for the fall of stones to the earth, by their being projected from the volcanoes in the moon,—I acknowledge my total want of faith in so unlikely an occurrence; nor have any arguments I have heard or read, at all lessened my conviction of their fallacy: and if I had needed any further confirmations of my opinion, the recent discoveries in chemistry respecting the decomposition of the alkalies and earths by electrical agency (as well as the circumstance of our having volcanoes upon our globe), would have been all-sufficient for such purpose. It appears to me quite as wonderful that ammonia should be capable of being reduced to a metal, as that meteoric stones should be formed in the atmosphere, one of the grand laboratories of nature. The mind is lost in astonishment at contemplating the extraordinary powers of galvanism and electricity in the combustion of metals, the decomposition of water, air, alkalies, earths, salts, and most other bodies submitted to their influence; and it scarcely can be doubted, but the establishment of these facts must appear as extraordinary to such of our present chemical philosophers, who were in existence thirty years ago, as the phænomenon of stones being formed in our atmosphere, or thrown from our volcanoes: and thus judging from analogy of the efficient power of nature to produce such effects, they would unnecessarily, and I should think reluctantly, carry their imagination so far as the moon for an explanation of the causes.

Having been lately engaged in some Galvanic and electric experiments, I cannot resist this opportunity of informing you that I succeeded in decomposing a small quantity of potash by a single battery of fifty double four-inch plates of zinc and copper with glass partitions, and a mixture of one part of the common muriatic acid of commerce and nine parts of water. I made use of the apparatus invented by Mr. Pepys, some time since described in your Magazine, without the naphtha: the metal, consisting of three or four small globules, was found imbedded in the alkali, and being placed under naphtha in a watch-glass was extricated with a silver knife. Solutions of Glauber salts and sulphate of silver were afterwards submitted to its action, and de-

composed

composed with facility,—the latter result having a most beautiful and brilliant appearance of metallic curls. Gold-leaf, tinfoil, and small watch-pendulum wire were also deflagrated to a considerable extent—the wire gradually assuming first a red, then a white heat, and lastly falling into globules.

The cylinder of my electrifying machine is only eight inches diameter, and ten inches long: but by applying an excellent amalgam and using every obvious precaution of having the whole apparatus dry and in good condition, I have with the assistance of Cuthbertson's balance electrometer, and a single jar containing about four square feet and a half of metallic coating, fused eight inches of the smallest watch-pendulum wire I could procure, with a very luminous and brilliant effect;—with a modification of Lane's electrometer of my own adaptation I have drawn pretty large sparks at a distance of four inches.

I am induced to mention these circumstances, in consequence of having often made the want of a larger apparatus a subject of regret, not having then any idea these experiments would succeed with one so small; and well knowing many persons are in possession of apparatus of about the same size as mine, without being aware of the effects they are capable of producing, I am anxious some hint should be given to induce their owners to bring them into useful action. I have found that by breathing into the jar, its capacity for receiving and retaining the electric fluid is very much increased.

I beg you will excuse my troubling you with these trifling desultory observations; the only apology I can offer is the experience I have had of their utility in my own pursuits.

One more remark, and I have done.—Being in possession of your excellent Magazine from its commencement, and observing it has now reached and completed fifty volumes, I do hope and entreat you will favour us with a general index to them.—I need not expatiate upon the great convenience of such an addition; it is too obvious; and whatever the expense may be, I am certain it would be met with alacrity by those who take your work. To be compelled to look through fifty volumes for an article, you must allow, is a trial upon the time and patience which a general index can alone remove.

I am, sir,

Very respectfully yours,

J. Acton.

P. S.—A few evenings ago, Mr. Lofft and I with some other friends were much delighted with looking through an excellent Cassagrain telescope, of a power of about 230, at Mars and some other of the heavenly bodies, which we observed to very great advantage, the objects being well defined, the field of view considerable,

siderable, with a concentration of light greatly exceeding any thing of the kind we had ever before witnessed. It is impossible for words to do adequate justice to the excellence of this instrument. It was constructed by Mr. Crickman, of this town, a self-taught optician, who has for some time past been highly reported of, for the perfect manner in which he makes his specula —particularly the finishing part of giving an exquisite polish, and forming the most desirable and difficult to be obtained figure;—as he has also been for making very superior microscopes. He has manufactured for me one of each sort at a reasonable rate, with which I am entirely satisfied. His rare abilities are well known to Major Kater, and several other scientific gentlemen, who have employed and approved him.—I shall be much obliged by your naming him in your Magazine, as soon as a convenient opportunity offers.

The dark dense cloud near the Sword of Orion was seen by us to great advantage by the above telescope, as also a group of spots near the centre of the sun's disk by Mr. Lofft.

Mr. Capel Lofft *to Mr.* Acton.

Ipswich, Dec. 30, 1817.

Dear Sir,—I have been thinking of our conversation last night respecting meteoric projectiles.

Yours of the 8th at any rate we may safely say could not be from the moon, it being then new moon (within less than twelve hours after) and the moon consequently near one-third of our earth's circumferential rotation below our horizon, and the visible path of the meteor from west to east; in order to which it must have traversed a vast portion of the lower sky beneath our earth, corresponding to near 16000 miles of the earth's circumference, before it would emerge above our horizon in the westward.

But we may consider the supposition generally.

The moon is about thirty of our diameters distant from us, and we must appear at the moon a diameter of about two degrees. Now at the radius of 240,000, what probability is there of a projectile being thrown from the moon, to a point of a circumference at this distance from her sufficiently near our position at the time to bring it to the earth, and prevent its falling back to the moon again?

It has an equal probability of being thrown to the side further from the earth, or at right angles to the earth, or at some other angle less than a right, but too great to admit of its falling to the earth:—the circumference on such a radius being about 1,500,000.

If projected from the moon so as to approach the earth at an angle

angle of 9° from the earth's place; this would amount to the distance of an arc of 36500 miles and $4\frac{1}{2}°=18250$, and $2\frac{1}{4}°=9125$. And this distance is far too great, and the obliquity of the line of direction too much to admit of such projectile being attracted to the earth, and falling on it.

Beside this, it must be projected with a force which would carry it 234,000 miles in about 8″, to carry it to a point where the attraction of the moon would no more than equal that of the earth. This is a velocity of very near 30,000 miles in a second; while that of the comet of 1680, for some few hours at its perihelion was only about 800,000 miles an hour $=8000$ miles in $36''=1000$ in $4\frac{1}{2}°$.

A body projected with such force would appear to have less than a momentary duration, if it even passed us vertically from east to west, allowing it to be much larger than any projectile from the moon can be supposed to be. Its density and power of cohesion must be inconceivably great to prevent its being fused and vanishing in vapour, or dispersed to atoms long before it would reach the earth. And with such density, to what astonishing depth must it not bury itself? And must not the force and depth of such an explosion be sufficient to rend the moon itself into fragments? Nothing comparable happens in our volcanoes on earth. And supposing it projected even twenty diameters of the moon from her, or 43,000 miles, when it had reached that distance or long before; the probability is, that it would revolve round the moon as a sub-satellite.

It costs little to make suppositions as to any phænomena, till the circumstances attending that supposition come to be examined.

I agree with you, that it is infinitely more probable that they should be found in our atmosphere, or perhaps in some rare instances projected from mountains of our own.

I remain, dear sir, Yours, &c.

Ipswich, Dec. 30, 1817. CAPEL LOFFT.

The density of the moon according to Newton is as 21 to 17, or as 7 to $5\frac{2}{3}$ to that of the earth. This is so small an excess that there is no reason for supposing a density of lunar much superior to terrestrial projectile masses. Their very slow rotation and her nearly equal sphericity may account considerably for this very superiority of density. If it be supposed that a lunar projectile may be invisible on account of its smallness till it has approached near to the earth, the velocity which enables it to overcome the moon's attraction must be vast indeed, and further increased by its gravitation as it approached the earth, so that nearly all the difficulties would remain.

The sketch underneath will illustrate this subject.

E represents the earth; M the moon; OO part of the moon's orbit.

a b c the passage of the projectile in a parabolic curve toward the earth.

Its greatest altitude is marked in its course. Its extent of arc in a parabolic curve of very moderate eccentricity; the proportional size of the earth and moon to each other, and to their distance nearly. The whole I think will prove how utterly void of apparent foundation the idea is of these projectiles from the moon reaching us. The semicircle round the earth includes a radius of about 6000 miles.

M M radius at which the projectile might revolve round the moon.

XIX. *Description of Mr.* Edmund Turrell's *improved Drawing Board and* T *Square**.

Sir,—The invention of ruling machines in the art of copper-plate engraving, produced such a degree of perfection in the tints ruled by them, that a corresponding degree of accuracy was immediately required in all the other departments of the art, wherever the use of the machine was introduced.

But unfortunately for the credit of this department of the art, a most general opinion was directly formed, that engraving done with a machine required but little exertion in the artist to attain perfection, and that nearly the whole secret lay in the possession of a ruling machine:—Experience has most indubitably proved, that in general practice, the invention of a ruling machine, at the same time that it produced a portion of perfection in the tints ruled by it, never before seen, presented a degree of difficulty in the drawing and finishing department, such as had never been experienced. Indeed, such an excess of difficulty as might have prevented its adoption in the hands of any other person than that of its most ingenious inventor.

One of the most formidable difficulties which presents itself to an engraver of machinery and scientific subjects, is that of getting a correct outline tracing upon the copper-plate. To effect this, transparent oiled paper (commonly called tracing paper) is used, for subjects of the free picturesque kind. But in plans, elevations, and sections of buildings, machinery, &c. such means are of lit-

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* vol. xxxiv.—For these improvements Mr. Turrell was voted the Society's silver medal, and five guineas.

tle

tle or no use; because, when such tracings are passed through the rolling-press to transmit the outline to the copper-plate, its unequal expansion while under the process of wetting, and likewise in its passage through the press, produces such a degree of error (especially when the ruling machine is to be employed) as to render such tracing completely useless.

To obviate this evil, the engraver has no other resource than to make a very correct outline reversed from his original drawing upon thin bank post paper, and having smeared the back with red chalk, carefully trace over each line, when the outline is laid down upon the varnished copper-plate.

I trust that it will appear evident from what has been said, that much will depend upon the nicety of the outline thus made; and as a means of insuring accuracy of form, and delicacy of execution, I have found it necessary to improve the drawing boards for such purpose.

Whenever very thin paper is used for making outlines upon, (and which is absolutely requisite in the case I have alluded to,) considerable difficulty is found by the surface of the board giving way, wherever compasses are used for taking dimensions; but more particularly in all cases where a number of concentric circles are to be described from the same point, the large holes produced in the drawing rendering it very unsightly, at the same time that all accuracy is destroyed by the centre hole frequently shifting into a hollow produced by the grain of the wood.

To avoid errors of this kind, I some years ago had recourse to a drawing-board covered with a plate of copper, which answered tolerably well; but when a point was pricked through the paper, it was scarcely visible, owing to the colour of the copper, which being reddish, rendered the dot, or puncture, very indistinct, while the surface was likely to have a very unpleasant oxide produced upon it, owing to the necessity of stretching the paper while in a wetted state.

The drawing-board which I have the honour to present, is covered with a plate of rolled zinc, which may be had of almost any dimensions, at Mr. Knight's, in Foster-lane, Cheapside.

As I have made use of drawing-boards of this kind for nearly two years, I can speak with confidence of their advantages; and I can assure the Society, that those artists who may be inclined to sacrifice a small additional expense, to obtain the means of making their drawings with neatness and accuracy, will not be disappointed if they possess a board of this description.

The advantages of zinc for this purpose I have found to be, that it is soft enough to admit of the insertion of a point sufficiently deep to be plainly seen, and yet hard enough to prevent the point from going to any considerable depth, so as to permit large holes to be made in the paper; and the drawing being complete, whatever holes are made in the zinc may be burnished down by rubbing the thumb nail over them, which will sufficiently close them.

When oxidation of the metal takes place, it should be suffered to remain, because being white, it helps to render the thin paper opaque, and consequently the lines drawn upon it will be the more plainly seen.

When the sides of the frame of a drawing-board are straight lines and nicely perpendicular to each other, parallel and perpendicular lines may be very correctly drawn with a good T square; but as wood is continually warping with every change of whether, accuracy cannot be expected from such imperfect means.

To prevent any error arising from this circumstance, I screw a solid rim of brass upon the upper surface of my frame, permitting it to project a small distance beyond the outer edges of the wooden frame, so that the stock of T square may slide against either edge. This brass rim being dressed very true and at right angles, wlll remain so for any length of time, as nothing but extraordinary violence can injure it.

I can assure the Society, that the present invention has sprung from necessity, originating in the very minute size of the engravings of the present day, added to which, a degree of accuracy is required, that can only be attained by a corresponding improvement in the apparatus; and for effecting such purposes, the zinc plate may be applied to a common drawing-board, and the brass rim to the frame; therefore those persons who possess a set of drawing-boards may have them improved at a small expense.

The same objection that applies to the frame of a drawing-board, applies equally to a T square made with a wooden blade and fixed immovably to its stock; for if ever any injury happens to the fiducial or drawing edges, by a blow causing an indention, or by the wood warping through a change of weather, it must remain, because the blade being glued fast to the stock, does not admit of being corrected without considerable difficulty.

The square which I have the honour to present, is so constructed as to permit the blade to be withdrawn from the stock, for the purpose of correction, should any accident occur to it; and the same means which permit the blade to be removed for the purpose of correction and adjustment, enable the draughtsman to use it as a bevil at any angle, where it may be fixed by a clamp and thumb-screw.

Should objection be formed to the weight of this invention, I beg leave to state that the same may be made in ebony or box wood,

wood, which would render it as light as a common square. The blade may be very easily set to a right angle by making it coincide with a line drawn on the arc for that purpose, where it may be fixed by the clamp.

Should any doubt arise as to the use of this improvement, I shall be happy to attend a Committee of the Society, to explain the difficulties that have given birth to it.

I am, sir,

Your most obedient servant,

No. 11, Gee-street, Clarendon-square. EDMUND TURRELL.

To C. Taylor, M.D. Sec.

Reference to the Engraving of Mr. TURRELL's T *Square.*

Fig. 2, (Plate I.) shows a view of a T square with an adjusting blade; consequently it can be set to any angle and fixed to the arc G, by the thumb-screw D.

Fig. 3, represents a section of fig. 2, in which the action of the thumb-screw D, and the clamp E, may be more distinctly seen, as likewise the screw F, upon which the blade turns as a centre.

The screw F being withdrawn, permits the blade to be taken out, and the edges repaired, if they should receive any injury from wear or accident. The arc G, being divided into degrees, permits the blade to be placed at any angle where it may be fixed by the thumb-screw D.

XX. *On Chemical Philosophy. By Mr.* MATTHEW ALLEN*.

Appendix to Essay I.

THE reason why I have included the views noticed in Essay I. under the title CHEMICAL PHILOSOPHY, is not alone that the consideration of electricity, galvanism, caloric, light, &c. are at present more particularly investigated by the chemical philosopher, but also because I believe the word *chemistry* had originally the most extensive meaning. The ancients considered heat and flame as the most prominent features in the operations of a power, on which every motion, change, and phænomenon depended; and hence it received innumerable names, all expressive of

* Mr. Allen's First Essay appeared in our number for Feb. last (vol. xlix. p. 81), and we expected before this time to have laid the continuation before our readers. With the present communication we have received a letter from the author, which gives but too distressing a reason (a family bereavement) for the delay in sending, the promised Essays—Essays which, from the one already before the public, and that introduced in our present Number, we have every reason to think, will not only amuse but afford solid instruction.

 heat

heat or flame, separately or combined. In this way all descriptive names are given; and the reason this power has had such an infinite number applied to it, is, that as one grand cause produces all the infinity of effects in nature, the mind of man has been directed to one part of its operations at one period of the world, and to another part at some other time. It was in this way that the word *attraction* was introduced, and has been more particularly adopted in modern times, because the mind was directed to Newton's theory of planetary motions, as well as to that of chemical affinity:—it is from these causes there appears, on a superficial view, so much contradiction in the different systems that have prevailed in the world. This contradiction, however, arises not so much from direct and absolute error, as from the divisions and separations of that which is true:—it is from dividing that which ought to be joined together; from some men at one period directing their attention to one part of the operations of this power in particular, while others at another time have had their thoughts directed to an opposite point. This has tended not merely to the exclusion of a comprehensive view of the whole, but each of these parts has been and still is carried by both parties to the utmost extreme. Such must for ever remain the fate of science and of every other subject, so long as terms which were originally descriptive of some partial and particular effect or peculiarity continue to be applied to a whole of which they form only a part: for though these terms may at first be received as descriptive, they are soon considered as expressive of the nature and essence of the power, &c. of which they are thus arbitrarily adopted as signs. Newton saw this in adopting the word *attraction*, and which has, notwithstanding his anxiety to guard others against it, become its present meaning in the common acceptation of the word. It is from considerations of this nature that I have preferred the terms "The GRAND AGENT," "The GRAND POWER,&c." The proofs why I conceive the word Chemistry had originally been applied in the same way, are not merely that such words as *chamiah, shaniah, lama, flamma,* and an immense number of other words,—all expressive of heat and flame, singly or jointly, throughout Asia and Europe,—have such a striking resemblance to each other, and in fact identity, considering the different shades of pronunciation produced by substituting the letters *s, c, g, k, ch, sh, w, v, p, f, b,* &c. for each other; but several other considerations are connected with their views of the subject, and their notions of the general principles of science. I am aware of this view differing in some measure from that given in Dr. Thomson's History of Chemistry (last edition of his System), and any hitherto adopted. I have not ventured to state this, however, without grounds, and grounds so extensive

extensive that it would be utterly impossible to state them in this place. To trace the connexion of words with each other, their relation to science and mythology, and all this connected with the men of ancient times, is too alluring, too important, and too mighty a subject to enter upon lightly—at any moment and in any place. I say this for the sake of asserting, that what I have said about others, or about names and definitions, has arisen from feelings and motives very different from that of the vain, flippant, carping, fastidious and wanton spirit of criticism, than which nothing can be more contemptible; but from a conviction that no man has a right to control or withstand that of truth. In the mean time I shall only remark on this subject, that I conceive the Arabians, gathering their notions from Eastern parts of the world, were led to believe that, as the power expressed by the word *chamiah* (to burn) was that which pervaded Nature and produced all its transformations, they could, by discovering its secret principles of operation, perform any transmutation they chose; and hence they called it, by way of distinction, *alchamiah*.

Indeed, in the present day, notwithstanding the partial definition given of this science, and its affected separation from other branches of natural knowledge, every description we have of its powers, its objects, and its applications, describes it not as an insulated portion of human knowledge, but as that, the professed object of which is to remove the veil from the face of Nature; to exhibit the nature, properties, and changes of matter; and to make us acquainted with that wonderful power which produces them. Here are the elements of science! Here is the centre and circumference of a mighty circle wherein all science is included.

Essay II.

I come now to the second division of our subject, that of ATTRACTIVE AGENCIES; a name which I have chosen, and under which I class ELECTRICITY, GALVANISM, MAGNETISM, CALORIC and LIGHT; because, should it in the mean time be objected, that they are not mere effects and phænomena, as I conceive they are, arising from the operations of one power on matter variously modified, and placed in all possible circumstances; it will nevertheless be admitted, that they are powers so different to those things commonly called substances; so highly attenuated, so universally diffused, of such essential importance in nature, and so invariably and intimately connected with all the phænomena of that power we have hitherto called Attraction, that I think the name ATTRACTIVE AGENCIES will apply better than any one yet given them, not only as being more apposite to my views of their powers and science, but even considered as secondary causes altogether unconnected with that power. Besides, in either light, the present state of science, and our acquaintance with these agencies in particular, render it necessary to give them a separate and distinct consideration; and in fact, the consideration of these wonderful phænomena,—of electricity, —of galvanism,—of magnetism,—of caloric, and of light, are incomparably the most interesting, entertaining and brilliant parts of the subject.

I need not defend the propriety of this generic term, which I have for the present adopted*:—the reader will be convinced it is the best I could have chosen, in connexion with attraction; the one cannot be altered without changing the other. Nor is it necessary to halt, in order to point out the defective and ill-assorted terms of imponderable substances, or immaterial substances, or impalpable substances, or ethereal matter, or radiant matter, and a variety of other terms which dissatisfaction and difficulty have given them.

Innumerable facts prove that these ATTRACTIVE AGENCIES are connected with attraction in the operations of nature, and so closely that they cannot be discerned to precede or follow each other; and I deny (and shall afterwards prove) that they can be procured separately and isolated. It is from a complete conviction of these facts, that I shall, to avoid repetition, and when speaking in a more general manner, class them together under the terms, *The grand agent,—Attractive agencies,—The principle* or *power of nature*, &c.

In my opinion, there are no changes in nature or art, no action, movement or operation, however slight or however produced, in which they may not be detected, in whatever light the reader may view them. Whether as primary effects of distinct powers, or as agents, obeying and observing the laws of that power on which every thing depends, the name ATTRACTIVE AGENCIES will apply. By considerations which will be gradually unfolded as I proceed in these Essays (which I hope to do monthly), I have been led to a decided conviction—a conviction so strong that I know not what further proof could increase it—that electricity, caloric and light are merely effects and phænomena arising from the nature of the substances on which, and the circumstances in which, the properties and energetic actions of one power are exerted and applied;—that changes and phænomena differ not in kind, but in degree; not in the power producing them, but in the nature and circumstances of its actions:—But of this in its proper place.

In every system of chemistry, the intimate connexion of ca-

* See Appendix to Essay I. above, from whence it will be evident that I conceive this term objectionable, &c.

loric

loric and light is constantly remarked. Even since some apparent differences have been more distinctly marked, not only has this union been remarked, but also of both with electricity, "in a way," says a celebrated author, "that indicates there is more in this than we yet understand, or shall perhaps be ever able to comprehend." If then the essence of attraction—of attraction in the abstract—is placed beyond the reach of human sagacity, so perhaps the most accurate conception we shall ever obtain of the phænomena of electricity, caloric, and light, &c. may be incomplete; yet I conceive that we may approach very near to that of accurate conceptions upon them. I say so; not without being aware of this grand and dangerous error; that whenever we attempt, on any subject, more than we ought, it only leads us into fruitless controversy and confusion, so dangerous and so fatal to man on every subject. Though there remains much of science to be discovered, there are limits we cannot pass; and nothing is more important than to know the limits which are assigned to each of us.

The name *attractive agencies* appears to me, however, more applicable to those changes of which, all must allow, they are either the cause which produces them, or inseparably connected with that which does produce them, and therefore more descriptive of the nature of their powers than any thing that we can say upon them.

Our acquaintance with the gases, and our means of exciting the grand power of nature in the varied forms of electricity and galvanism, may be considered as the most astonishing proof of the highly advanced stage and state of the world. To become familiar and conversant with things not palpable to any of our senses in their ordinary state of existence, could never have entered into the cold calculations of reason; nor can reason, proud as she is, calculate the number or measure the extent of those discoveries and improvements that may yet meet us as we advance in our future progress—it is more than her province to say, Here you may hope, but there you shall despair.

We come now to the third division of our subject, to which I have given the name of PASSIVE SUBSTANCES, which are those things or constituents of matter acted upon by ATTRACTION or ATTRACTIVE AGENCIES; and I divide them into SOLIDS, FLUIDS, and GASES. I place PASSIVE SUBSTANCES in this situation in this arrangement, and give them this appellation, because I conceive they owe their subdivisions solely to this power, and the effects which it produces. Without such a power and the agencies it exerts in the creation, where would be our world, or any other? Where would be the grand scenes that creation now presents

presents to our view? or rather, To whom would they be presented? Where would be the order or harmony, the beauty and life of this magnificent display? Where the powers of contemplation and enjoyment?

I scarcely think I have any need to take up our time with proving the superiority of this term over such as—"material substances," "ponderable substances," "inert substances," "*vis inertiæ**, &c. &c."

Passive substances, with their subdivisions of SOLIDS, LIQUIDS, and GASES included, have this general definition, "*Every thing which occupies space.*" Now we can form no conception of any thing which does not occupy space:—to occupy no space, is to have no existence. This definition therefore is improper; because it either intimates an impossibility, or must include every thing, even infinitude itself, and of course electricity, galvanism, caloric and light. I propose this slight addition, which removes these objections and qualifies it, "*Every thing which occupies a definable space.*" This is the general definition for passive substances, including solids, liquids, and gases. I shall now offer particular definitions to mark and distinguish them from each other; which has not hitherto been done, as far as I know, with that correctness or peculiarly appropriate description which definition requires.

SOLIDS have little or no tenuity and freedom of motion in their parts; and they differ from each other in weight, in hardness, in transparency, in form, and in colour.

FLUIDS possess more tenuity and freedom of motion in their parts; and they differ from each other in density, in colour, opacity, and transparency.

GASES possess most tenuity and freedom of motion in their parts; they are also particularly distinguished by compressibility and expansibility; and they differ from each other in weight, colour, and transparency.

XXI. *Chemical Examination of some Substances used in Ceylon as Remedies against the Bites of venomous Serpents.* By JOHN DAVY, *M.D. F.R.S.*†

IT is well known that certain substances or preparations are used in India by the native empirics or snake charmers, for curing the bites of venomous snakes. As some confidence has been placed in these nostrums, not only by the Indians but by some of the European settlers, it was interesting to ascertain whether any real virtue belonged to them. This object has been undertaken

* See first Essay on *Vindiciæ Antiquæ.*—Classical Journal.

† Communicated by Sir H. Davy.

taken by Dr. Davy, who at present resides in Ceylon; and he has communicated the result of his researches (of which an abstract is here given) to the Asiatic Society of Calcutta.

The snake stones as they are called, which Dr. Davy examined, were of three kinds. One by minute analysis was found to be merely *calcined bone*; another was *carbonate of lime coloured by vegetable matter*; the third was a bezoar stone. Of these, the two first had some adhesive powers when applied to the tongue; but the last had no such power.

Dr. Davy decides that these stones are of no use whatever as applied to the wounds produced by the bites of serpents; and he refers the pretended cures produced by them to Nature, or to the circumstance that they have been applied to wounds produced by snakes which are not venomous.

Of eleven different species of snakes which the author has examined, all of which were believed by the natives to be poisonous, Dr. Davy found only three to be really so; and the bites of two of these (the *Cobra di capello* and the *Polonga*) only are mortal, and that under very peculiar circumstances.

Dr. Davy concludes by stating, that the sooner the belief in snake stones is exploded the better, as much time is lost in applying imaginary remedies, and some lives are lost which might have been saved, and much suffering occasioned which might have been prevented by efficient means of cure.

XXII. *Remarks on the Case of Miss* M'Avoy. *By Mr.* Egerton Smith *.

Mr. E. Smith having been referred to by Dr. Renwick, the advocate of Miss M'Avoy's pretensions, as one of the host of testimony in their favour, that gentleman has deemed it necessary, through the medium of the public press, to enter into some explanation on the subject. The following remarks extracted from his last letter will be found not undeserving of attention.

"Under the supposition that Miss M'Avoy is not blind, the simple question will be—whether her sight may not be so much more acute than that of others, as to enable her to read a book of small print, when there is so little light that ordinary eyes can barely discern the leaves of the book?

"Experiments, which I have recently witnessed, have proved, that one person, when stationed at the furthest corner of a very long room, could read a book, of moderately small print, by the light of a single candle placed in the opposite corner of the room; whilst some others of the company could barely make out the

* From the Liverpool Mercury.

print

print at only one-sixth part of that distance from the light. Here, then, in a small party, indiscriminately assembled, it is shown, that one individual could discern an object by means of one-thirty-sixth part of the light which was necessary to enable another to see the same object.—This experiment was necessarily limited, by the dimensions of the apartment: but, I am inclined to believe, that the same gentleman would have succeeded in reading at ten times the distance from the candle at which I found it necessary to station myself for the same purpose: or, in other words, he could have read the print alluded to, when it was illumined by the one-hundredth part only of that light which I required to enable me to peruse the same print.

"I have now before me a very extensive and most singular work, called *Wonders of the Little World*, which contains about 700 pages, and abounds with cases as marvellous as that of Miss M'Avoy. As I have little faith in this apocryphal folio of Wanley ,it would be unfair, in me, to adduce instances from his work; or I could tell of one sharp-sighted fellow, who could 'see and discern out right 135 miles;'—of another, named Julianus, a monk, that, 'for the space of seventy years, never lighted nor had a candle; who nevertheless was used to read books throughout in the darkness of the night;'—and of a third, who, when he was young, used, in the night-time, to 'compose very elegant verses, and write them down exactly by the light which issued out of his own eyes.' I know nothing on record resembling the last feat, except it be in the memoirs of Munchausen, where we read, that the hero having lost his flint, placed his head immediately above the pan of his fowling-piece, and, giving himself a smart blow, made his 'eye strike fire,' and thus ignited the powder.

"To return to the subject, however, and to be serious: It is of some consequence, in the investigation of the case of Miss M'Avoy, to pay attention to the wonderful diversity which prevails in the powers of the visual organs; and I cannot refrain from transcribing the following curious circumstance, which has recently appeared in the public journals.

"'In the conversation between some English gentleman and Bonaparte, at St. Helena; the former speaking of the Cossacks, —Napoleon said, that 'they resembled the Bedouin Arabs in the gift of vision: so great in this respect was the faculty of the Bedouins, that, when in Egypt, upon an occasion when he wished, by means of his telescope, to observe a body of men that appeared in the horizon, he had scarcely levelled his glass, when a Bedouin, near him, recognised with his naked eye another Bedouin, and described his dress, &c. so as to distinguish the tribe to which he belonged.'

"Now,

"Now, is it not much more rational, as well as philosophical, to conclude that Miss M'Avoy possesses visual powers of an extraordinary description, than that she is gifted with a new or sixth sense, unknown in the animal œconomy; particularly, when so many notorious facts militate against the latter supposition? Some of the prominent suspicious circumstances, connected with this case, were first stated in my letter to Dr. Renwick: and, together with many more stubborn facts, were afterwards treated at considerable length, and with much perspicuity, by Mr. Sandars in his pamphlet on the subject. In consequence of these awkward disclosures, many persons, who had previously yielded the most implicit assent to all the wonders related of the young lady, have wholly, or partially, recanted; and, if I may be allowed a homely phrase, have been anxious to '*back out*' with the best possible grace. For my own part, my faith, even at its crisis, was so very limited in degree, and of such transient duration, that I should not feel the slightest hesitation in making a full confession of its rise, progress, and decline.

"I was first introduced to Miss M'Avoy by Dr. Renwick, a gentleman for whom I entertained a respect and esteem, which has not suffered the slightest abatement by any subsequent event: for I must confess, that I am wholly at a loss to understand, why a mere difference of opinion, upon a speculative point, should be allowed to disturb the social feeling that previously existed between individuals whose motives are undoubtedly above all suspicion, and can only be referred to a regard for the truth; however they may differ as to the best means of attaining their common object,—or however opposed may be the conclusions they may draw from a different view of the same case.

"I was assured by Dr. Renwick that Miss M'Avoy was actually blind; a circumstance, which, as it would have been presumptuous in me to have denied it, from my entire ignorance of such a subject, obtained my ready belief. So long, therefore, as she confined the display of her talents to discovering the words of a printed book, or the shades of cloths, I experienced less hesitation than surprise in the belief that it was possible that she did possess the faculty of touch *to that degree*. I then thought, and I am still of the same opinion, that there is nothing revolting to analogy or common sense in the recognition of such a faculty, however rare its possession may be.

"It is most certain, for instance, that there is a most *material* difference between the black ink with which I now write, and the white paper upon which I am writing. The printing ink used for books differs still more from the paper upon which such book is printed; and, from its lubricous nature, must present less

asperity

asperity to the touch than the paper. The simple question, therefore, is, whether this essential and substantial difference, the existence of which none will pretend to deny, is palpable to, or cognizable by, the human touch?

"Implicitly relying upon the assurance of her professional attendants, that Miss M'Avoy was blind; and having seen her exhibit her surprising feats, when she was so completely bandaged, that her eyes, had they been of the most perfect nature, could not, *as I then thought*, have been of any avail;—I found it impossible to withhold my belief, that she enjoyed some superior organs of touch, by which she was enabled to decipher the words of a printed book, and to ascertain various shades of cloth; particularly as the latter power has been admitted in the cases of many persons who have been unquestionably blind. The subsequent pretensions of the young lady, when she began to tell the time of the day by feeling at the watch-glass, and to identify passengers in the street by applying her fingers to the window-glass, were so extremely preposterous—so much at variance with the attributes of touch—that I could not afterwards regard the *whole* performance in any other light than that of a clever deception, in which, as I expressed myself in my first letter, 'the eye was, in some way or other, concerned.'

"Those persons who have been, or still continue to be, thorough believers in the faculty of Miss M'Avoy, either refer the phænomenon to the class of miracles, or rest their faith upon one of two data, neither of which has been proved, and both of which are strongly presumed to be false. They take it for granted, in the first instance, that she is actually blind; and, in the next place, they assume, that, if this was not the case, she has, during the experiments, been so completely blind-folded, by the application of bandages, gogglers, gold-beaters' skin, and other means, as to render all access of light to her eyes absolutely impossible.—Mr. Dale, who is a most complete ultra-M'Avite, and who has designated her power the '*cuticular faculty*,' boldly maintains both these positions; and as he is the latest as well as the most devoted champion of the transparent theory, I shall quote the following passages from a letter of his:—'Upon inspecting her eyes, with that attention which her case demanded, I felt assured, that the visual power was completely gone.' Soon after which, speaking of the gogglers used, he adds, 'these I examined, prior to their being applied; and, after they were firmly fixed, I was thoroughly satisfied, that even although she had actually been in possession of the full and proper use of her eyes, those organs could not possibly avail her, in attempting, whils thus hood-winked, to ascertain colours.'

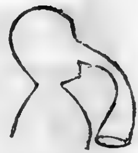
"I have

"I have said, that neither of these data, so boldly adopted by Mr. Dale and many others, have been established by such proofs as a reasonable man has a right to demand, before he yields his assent to a phænomenon which violates all the known laws of nature and philosophy. It is notorious that many eminent professional men have declared that she is not blind; and there are very strong grounds for believing that, whenever the bandages, gogglers, &c. have been properly applied, the young lady has not been able to exhibit her '*cuticular powers*,' and has been 'deserted by the faculty.' We have the testimony of Mr. Sandars corroborated by most respectable witnesses, to show that other persons have been able to read, ascertain colours, and the hour by the watch, with precisely the same apparatus applied to their eyes as had been used by Miss M'Avoy: and I have found to my great surprise, that it is scarcely possible by means of a silk handkerchief, or shawl, to blindfold some persons; probably owing to the prominence of the nose, the flatness of the cheeks, or some peculiar conformation of the face,—a circumstance which I do not pretend to explain, although it has certainly astonished me; because a simple bandage formed by a silk handkerchief places *me* in a state of utter darkness.

"As the blindness of Miss M'Avoy is denied by medical men, who ought to be the most competent judges of the fact,—and as all the ordinary modes of blindfolding her have been pronounced to be imperfect, or liable to suspicion,—it would naturally tend to set the matter completely at rest, if some test could be proposed, which, whilst it would impose no restraint upon the lady incompatible with her own theory, if I may so call it, would either silence her pretensions at once, or for ever put an end to the objections of the most querulous scepticism. Such a test I conceive to be extremely simple; and I shall evince the sincerity of my own convictions upon the subject, by committing myself entirely to the issue of the trial I am about to propose.

"It has been repeatedly observed, that when any obstacle has been interposed between the subject of her examination and her face, she has uniformly failed of success: and she accounts for this, by stating, that it is necessary that there should be an 'uninterrupted communication between her hands and the breath of her mouth, or nostrils.' Now, although this is considered by many as a mere subterfuge, I will, for the sake of argument, admit that it is really indispensable to her performance; and shall proceed to propose the test to which I have alluded. A MASK shall be provided, to cover the whole face, and pass UNDER THE CHIN,—which latter circumstance is essential, in my view of the subject, as it will prevent the possibility of any light passing in the direction suspected by many persons.

"The



"The mask, of which a rough outline is annexed, contains no aperture for the eyes; and the only passage for breathing is through the CURVED TUBE, which, whilst it freely admits the *breath* of the *mouth* and *nostrils*, prevents the possibility of the access of light to the eyes, as light proceeds in straight lines only. The object of her examination may be stationed near the bell-mouth of this *curved tube*, where, whilst she is exercising the '*cuticular faculty*,' she may *breathe freely upon her hands*; so that she will here have every condition for which she herself stipulates.

"If she will consent to abide by the test of such a mask as this, it will remove all suspicion that she accomplishes her purpose by means of a small portion of light, which finds its way under the ordinary bandages, as I have seen most unquestionably proved to be practicable, in the persons of others, who have succeeded in reading, &c. when their eyes appeared to have been most completely blindfolded; although they were mere novices, unpractised in such manœuvres, and made no pretensions to the '*cuticular faculty*.'

"I feel so confident of the efficacy of the test I have proposed, that I hereby pledge myself to pay TWENTY GUINEAS to the Treasurer of the Blind Asylum, if Miss M'Avoy, with such a mask on, can read a single line of moderately sized print, or writing, —and ascertain, correctly, the shades of half a dozen pieces of coloured glass: the experiments to be conducted by a committee of three, of whom Dr. Renwick may be one; the others to be appointed by myself.

"In order that the funds of the excellent charity just named may be benefited by the experiment *at all events*, it is to be hoped that some of the advocates for the '*cuticular faculty*' will have sufficient confidence in their cause, to enter into an engagement to forfeit a similar sum, should the experiment fail on the part of the lady.

"If this challenge should be accepted, I will provide the mask; and should my conjecture prove unfounded, I shall consent to do penance, if required, in a white sheet, with a lighted taper in my hand, and, in that situation, to read a formal recantation of my heresy, as humble and humiliating as the most sanguine enthusiast in the cause can dictate.

"Yours respectfully,

"EGERTON SMITH."

XXIII. *Case*

XXIII. *Case of an American Girl—deaf, dumb, and blind*.*

If proofs of vigorous intellect in the deaf and dumb are admirable, how much more wonderful are evidences of a similar nature in persons who suffer the additional misfortune of being blind!

I heard a benevolent lady mention the name of Julia Brace, a girl about eleven years old, living in the vicinity of Hartford, who is afflicted with the triple calamity of blindness, deafness, and dumbness, having lost the senses of sight and hearing, by the violence of a typhus fever, at the age of four years. On visiting her myself, I learned that the extreme poverty and the obscurity of her parents have prevented her from being known or particularly noticed, except by the charitable ladies of the town, and a few gentlemen, who have been induced by motives of curiosity to examine her conduct. The following facts and little anecdotes I relate for your amusement.

Her form and features are regular and well proportioned. Her temper is mild and affectionate. She is much attached to her infant sister, often passes her hand over the mouth and eyes† of the child, in order to ascertain whether it is crying, and soothes its little distresses with all the assiduity and success of a talkative or musical nurse. All objects which she can readily handle, she applies to her lips, and rarely fails of determining their character. If any thing is too large for examination in this way, she makes her fingers the interpreters of the texture and properties, and is seldom mistaken. She will beat apples or other fruit from the tree, and select the best with as much judgement as if she possessed the faculty of sight. She often wanders in the field, and gathers flowers, to which she is directed by the pleasantness of their odour. Her sense of smelling is remarkably exquisite, and appears to be an assistant guide with her fingers and lips.

A gentleman one day gave her a small fan. She inquired of her lips what it was; and on being informed, returned it to the gentleman's pocket. The mother observed, that Julia already possessed one fan: she probably thought that another would be superfluous. The gentleman gave the same fan to a neighbouring girl, whom Julia was in the habit of visiting. She went a few days after, to visit her companion, whose toys she passed under the review of her fingers and lips; and among other things, the fan, the identity of which she instantly discovered, and again restored to the pocket of the gentleman, who happened to be present.

* From the Boston Intelligencer.

† Probably to discover whether the mouth be distorted, or there are any tears on the cheek.

She feels and admires mantle-piece ornaments, and never breaks or injures the most brittle furniture, even in a strange room.

She is obedient as other children in general. The jar of her mother's foot upon the floor effectually prevents the commission of a fault; but she easily distinguishes the stamping of one of the children from that of her mother, and obeys or not, as she pleases.

Her parents, as you may well suppose, have not been able to indulge her in dress; but when she receives articles of clothing or ornaments as presents, she is highly gratified to find that they resemble in form and fashion those of her playmate. She has, as you perceive, a spice of female vanity! At a tea-table, she behaves with more gentility than many a miss who has the benefit of eyes, by which to adjust her motions and attitudes.

A gentleman once made several experiments with a view to satisfy himself whether she really had the discernment which she was reported to possess. Among other arts for effecting his object, he pretended to carry away her infant sister. She immediately detected the cheat, by ascertaining that his umbrella remained upon the table. She then went out of the door, and picked the head of a large thistle in full bloom, brought it in, smelling of it as she came, and offered it to the gentleman, apparently as a nosegay. He reached out his hand; but, instead of giving it, she archly pricked his hand, by way of retort for his freedom in testing her sagacity.

XXIV. *On a Plan for properly observing Fire-Ball Meteors.* *By* H. Clarke, *LL.D.*

To Mr. Tilloch.

Sir, — As your correspondent J. A. (Dec. 1817, p. 469) seems very justly desirous of a plan for properly observing the fire-ball meteors, it is presumed that this gentleman may not be apprised of that which was recommended and transmitted, as a circular, to the scientific public by the late Astronomer Royal Dr. N. Maskelyne, now upwards of thirty years ago. Having a copy of this paper still in my possession, and which is at this time, I believe, rarely to be met with, I have no doubt but its insertion (whenever convenient) in your valuable Magazine, will meet with general approbation.

As a remark, however, on the Rev. Doctor's plan, it may not be superfluous to add, that whoever compares it with the accounts we find of the general appearances of these phænomena, which are copiously given in the French Philosophical Memoirs, and

and in the Transactions of our own Royal Society, (or, see Dr. Charles Hutton's Dict. *Fire Ball*.) will immediately perceive that a very material article is omitted therein; and, indeed, one of the most essential for obtaining by induction a satisfactory theory of these meteors; which is, at the time of observation to connect therewith—the known local peculiarities of the station or point of view respecting atmospherical pressure, refractions, &c.—the thermometric and hygrometric affections, and particularly the magnetic and electric indications at the same time. From this combination it may be then possible to form a general deduction upon true geological principles, and obviously resulting from well-known natural causes.

I am, sir, yours, &c.

Islington, Jan. 7, 1818. H. CLARKE.

A Plan for observing the Meteors called Fire-balls. By NEVIL MASKELYNE, *D.D.F.R.S. and Astronomer Royal.*

Five meteors, of the kind which from their appearance are generally called fire-balls, have been seen of late, in the space of a few weeks, viz. on August 18, Sept. 26, October 4th, 19th, and 29th, which seems to indicate that they appear more frequently than is commonly imagined. The curious and extraordinary appearances which they exhibited, show them to be deserving more attention than has been hitherto given them. For want of a series of proper observations, little progress has been made towards accounting for their phænomena. The greater part of those who have seen them, not being previously acquainted with the circumstances they ought to attend to, have made observations too imperfect to answer that purpose. It is therefore to be wished that all persons who may happen to see a meteor, would attend to the following particulars, and set down their remarks as soon as they can after they see it, while the impression made by the meteor is full and fresh in their memory, before it is vitiated by their own after-thoughts, or the accounts received from other observers. Such after-thoughts may be of great use: but their own genuine *original* observations are chiefly to be wished for by any one who is to calculate the tract of the meteor.

The particulars to be attended to are these:—

1st. The precise time of its appearance.

2d. Its apparent altitudes and bearings at its first appearance, at its greatest elevation, at its bursting, and at its disappearance.

3d. Its figure, and the diameter of the body when at the greatest apparent altitude, compared with that of the sun or moon at the same altitude; the brightness and colours of its light, and the degree of illumination which it gave; and to make a sketch or drawing of the appearances before and after it burst, or any other of its appearances.

4th. Whether both the body and the tail burst; and how many parts this bursting produced; and whether this happened before or after it arrived at its greatest apparent altitude; the length of the tail before the meteor burst; and indeed every alteration of its length they observe; whether the meteor appeared very faint at first, and gradually grew brighter, or appeared very bright at once; and whether it was extinguished suddenly, or by degrees.

5th. How long the appearance lasted.

6th. Whether a sound or sounds (as of an explosion) was heard some minutes after its disappearance, and how long, and from what point of the compass they thought it came.

7th. The bearing and distance of the place of observation from the nearest market-town should be put down.

N.B. As sound moves only at the rate of thirteen miles in a minute, the observer should patiently wait for at least eight or ten minutes, listening for the sound, for all meteors appear to be very many miles indeed nearer to the observer than they really are.

Remarks.

Curious persons may avail themselves of observations made even by the most illiterate, by causing them to trace with a stick the path which the meteor described in the heavens, according to the best of their recollection. The observations would be better made, if you accompany the person to the very spot where he saw the meteor, for there the neighbouring objects, such as roads, houses, or trees, will much assist his memory.

The apparent altitudes of the meteor are best found by a quadrant (a common wooden one of three inches radius divided into degrees will suffice) which the person should direct to the points in the heavens where the meteor appeared to him, if he saw it, or even to such points where the illiterate person above mentioned pointed. In like manner its bearings should be found by a compass.

To ascertain how long the appearance lasted, he should trace over its path in the heavens with its proper velocity, while another person observes the time by a watch or clock that shows seconds; or by the number of swings of a temporary pendulum made by a musket ball or any small weight suspended by a string of 39 inches long from the centre of the ball or weight, which will swing seconds. Without some such method as this, they will be apt to estimate the time much longer than it is.

It would be well if those persons who happen to see a meteor would put down the time by their watch when it first appeared,

or

or was at its greatest altitude, or burst, or disappeared, and again when they hear the sound; and, as common watches are liable to vary much in a few hours, that they would, as soon after as may be, find the error of their watch by comparing it with a good regulator; for, if the exact times could be had at different places, the absolute velocity of the meteor, the velocity of the sound propagated to us from the higher regions of the atmosphere, and the longitudes of places might be determined.

Even in cloudy weather it might be useful to note the times of accidental explosions, or any unusual sounds heard, with the points of the compass from which they are thought to come, whether in the day or night, and of sudden illuminations of the sky in the night, as they may prove afterwards to have been owing to meteors, and will serve some of the purposes above mentioned.

These meteors generally leave a visible tract of faint light behind them, which gives time to observers to ascertain the path, either by the stars near it, or the observations of altitudes and bearings. Meteors are sometimes seen in the day-light.

It may not be amiss to apprize observers, that estimations of altitudes made without an instrument are very uncertain, owing to the apparent figure of the sky being the segment of a sphere, whose centre is greatly below the surface of the earth; so that persons will be apt to judge an object which is near the horizon to be much higher than it is; at 23° of altitude, they may think it at 45°; and to be in or near the zenith, when with an instrument it would be found 10 or 20 degrees from it. This points out the necessity for observers to mention, whether they estimated their altitudes, or observed them with an instrument.

Greenwich, Nov. 6, 1783. N. M.

XXV. *Notices respecting New Books.*

A Voyage to Spitzbergen, containing an Account of that Country, of the Zoology of the North, of the Shetland Isles, and of the Whale Fishery. By John Laing, *Surgeon.* 2d Edition, 12mo, corrected and enlarged. pp. 165.

In the little volume before us, we have an interesting addition made to the natural history of regions of which our knowledge is as yet, comparatively speaking, but imperfect. Mr. Laing has been evidently a diligent and acute observer, and communicates the fruits of his observation in a simple and perspicuous manner. The description which we subjoin of the *Phocæ* class of amphibious animals, all the accounts hitherto given of which by voyagers and naturalists are extremely confused, may be taken as a favourable specimen of the work.

"The *Phocæ* * are the most numerous class of animals which frequent Spitzbergen, where they are found in vast numbers. Though the specific characters of each particular tribe are distinctly marked, their general resemblance is, upon the whole, so very striking, that the following observations may be applied to them all indiscriminately. In the scale of nature, the Phocæ hold an intermediate station between amphibia and perfect fish; but nearer the latter than the former. The organization of other amphibious animals, such as the beaver, castor, otter, &c. fits them better for living on the land than the water. In this genus the contrary takes place. The arms and legs of the Phocæ (if we may employ these terms) are wholly enveloped in the flesh of the animal, the hands and feet being alone protruded; these too are webbed, and are instruments evidently more calculated for swimming than moving on land.

"This unaptness of organization is strongly displayed in the painful motion of the animal, which, from the shortness of its legs, has to rest at every step, on its belly, until it prepares for a new advance. Its agility, considering these defects, is indeed astonishing, and is certainly the effect of great exertion.

"The eloquent and ingenious Buffon was of opinion, that the Phocæ approached to fish by a still more decisive criterion. 'They are the only animals,' says he, 'who have the *foramen ovale* open, and who can therefore live without respiring, and to whom water is as proper and suitable an element as air.' Theoretic views appear to have here led this excellent writer into an error, as it is now well known that the Phocæ cannot remain long in the water without coming to the surface to breathe.

"The *Phoca vitulina*, by the English termed *seal*, and by the French *phoque*, is the most common species of those animals in the North, and is dispersed with some variety throughout the rest of the Ocean. Its head is large and flat; the teeth strong, and so sharp, that I have seen it bite in two the handspikes with which the men were attempting to kill it; the tongue is forked; and it is well furnished with whiskers around the mouth; has almost no external appearance of ears, but merely an aperture to convey the sound to the *sensorium*; the eyes are small, and have a haggard appearance; the neck thickens as it approaches the shoulder, the thickest part of the animal; from whence the body gradually tapers in a cylindrical form, to the extremity, where the hind legs are placed, between which is a very short tail; the fore paws consist of five fingers, joined together by a membrane, and furnished with very strong cylindrical nails; the hind paws are formed in the same way, except that the fingers are longer than

* Under this general appellation I include the seal, walrus, or morse, dugon, &c.

than in the fore paws, and that the shortest of them are in the middle, and the longest on the outside of the paw. The length of an ordinary full grown seal is about seven or eight feet; and its thickness at the shoulder four or five. It is covered with short coarse thick hair, which varies in its colour with the different ages of the animal.

"The flesh of the seal is of a reddish colour, and is, by the Greenlanders, accounted excellent food. Our sailors esteemed the entrails of a young one which they dressed, as equal to those of a hog. A seal will yield about twelve or fourteen gallons of good oil; their skins are very valuable, serving for covers to trunks, vests, &c. and are now used to a very considerable extent in the manufacture of shoes. The Greenlanders, who depend almost entirely for subsistence on this animal, make their boots and other articles of dress, as well as the inside of their huts, of its skin.

"The seal is a gregarious and polygamous animal. It is never met with at a great distance from land, but frequents the bays and seas adjacent to the shore. It feeds promiscuously on most sorts of small fish, but chiefly on the spawn of the salmon.

"Fabricius differs both from Buffon and Pennant in asserting, that the seal brings forth but one at a time, while they maintain that it brings forth two*. At the time of parturition, it comes on shore, and suckles its young there for about six weeks before it takes them to the water, where it instructs them in swimming. Though naturally timid, the female defends her young with great boldness and spirit; on other occasions they generally place their safety in flight; but I have sometimes seen them throw back stones and pieces of ice on the sailors who pursued them.

"Seals delight to lie upon the ice, or on the shore, exposed to the sun †; they there sleep very profoundly, and fall an easy prey to the sailors, who dispatch them by a blow on the nose.

"Their voice has been not unaptly compared by Buffon to the barking of a hoarse dog; when attacked, they make a more doleful kind of noise.

"Pliny expressly states this animal to be of a docile and tractable nature, and in this he is supported by the more enlarged experience of modern times. The seal described by Dr. Parsons ‡ was taught to come out of his tub, and return to the water, at the command of its keeper, to stretch out its neck to kiss him, and to perform several other motions.

"Seals have a very delicate sense of hearing, and are very much

* Perhaps Pliny has hit the truth, "*Parit nunquam geminis plures.*" Nat. Hist. lib. 9. § 13.

† *Sternunt se somno diversæ in littore Phocæ.* Georg. lib. 4.

‡ Pennant's *Quadrupeds*, vol. ii. p. 272.

 delighted

delighted with music. The captain's son, who was a good performer on the violin, never failed to have a numerous auditory, when we were in the seas frequented by those animals; and I have seen them follow the ship for miles when any person was playing on deck. This fact was observed by the ancient poets*; and is thus alluded to by Mr. Scott, in his recent poem:

'Rude Heiskar's seals, through surges dark,
Will long pursue the minstrel's bark.'

"These animals, in swimming, *constantly keep the head,* and often the whole body, as far as the shoulder, above the surface of the water. The first I saw was at a considerable distance, and might easily have been mistaken for a man, though it was much liker a dog.

"Buffon has already remarked, that this animal had given a foundation to the poetic fiction of the Nereids in antiquity; and perhaps we may add, to the no less fictitious mermaids of modern times.

"The Arctic walrus, or *Trichechus Rosmarus* of Linnæus, the other great variety of the Phocæ, frequents the bays and shores of Spitzbergen in vast numbers, though they are not now found in such quantities as when the Europeans first navigated these seas. The walrus is considerably larger than the seal, being sometimes found eighteen feet long, and twelve round, where thickest †. Their characteristic difference, however, consists in the walrus having two very large tusks, or horns, like the elephants, projecting from his upper jaw. These are sometimes found of an extraordinary size, from two to three feet in length, and weighing twenty pounds. The tusks of the Spitzbergen walrus seldom attain this size, because there the animal is generally killed before attaining its full growth. It is only on the northern coast of Asiatic Russia, or where they are not molested by hunters, that such tusks are found.

"With the exception of the tusks, the form of the walrus does not differ materially from that of the seal. Head round, with a short nose; mouth small, with strong bristles; small red eyes; short neck; colour variable; rest of the body similar to the seal; but its toes, especially in the hind feet, are much stronger.

"The walrus is monogamous, but in other respects its habits are nearly the same with those of the seal. It brings forth its young in the same manner, preys on the same kinds of fish, and, like the seal, ascends the ice, (more rarely the land,) to bask in the sun.

"The walrus is a very valuable animal, yielding frequently half a ton of oil, equally valuable with that of the whale. The tusks

* *Apol. Rhod.* lib. 1. *Val. Flac.* lib. 5. lin. 440. *Gaudebant carmine Phocæ.*

† The largest we caught, was only thirteen feet long and seven round.

are

are said to be more valuable than those of the elephant, as being more compact and hard, and consequently taking a finer polish: the skin, which is nearly an inch thick, is used to cover the masts or yards of ships, where they cross each other, to prevent their being injured by the friction. It was formerly cut into ropes; and Buffon mentions its being used at Paris in the springs of carriages.

"The walrus becomes very furious when attacked, and the whole herd join to revenge any injury an individual may have received. If wounded in the water, they will sometimes surround the boat, and attempt to sink her, by striking their tusks against her sides and bottom."

The first Centenary of a Series of concise and useful Tables of complete Decimal Quotients: To which is added, a Tabular Series, with the Equivalent Vulgar Fractions prefixed. By HENRY GOODWYN. pp. 31.

The author of this elaborate work, the ingenuity and accuracy of which cannot be too highly praised, remarks in illustration of the *conciseness* with which the Tables have been made up, that if the quotients arising from *each* divisor and dividend had been exhibited, they would, as ascertained by a well known arithmetical process, have amounted for this centenary to

$$\left.\begin{array}{c} 100 \times 101 \div 2 \\ \text{or} \\ 101 \times 50 \end{array}\right\} = 5050;$$

whereas, the whole number of entered quotients is only 1522, so that there is an *apparent* deficiency of 3528. This very considerable diminution arises, in the first place, from expunging under each particular divisor such of its appropriate dividends as are *not prime* to it;—and, in the second place, from the mode in which the dividends are disposed in the tables. "The usefulness of such tables," adds the author, "will be obvious to all who are in the habit of applying decimals to the purposes of the arts, sciences, or mercantile concerns."

Lest it should be inferred, from the appearance of this work at a time when a bill is under the consideration of Parliament for the equalization of the weights and measures of the kingdom, that the author is an advocate for a decimal division, he states his opinion to be—that for common use the standards of both should be derived from 2 and its powers; and where any intervening weights or measures are necessary, that they should be expressed by *products* of that number. It is shown in various parts of the work, that when the divisor is 2, or a power of 2, the decimal quotients uniformly *terminate*, or are *perfect*. The same is the case also with 5 and its powers—and when the divisor is a product of 2 or of a *power* of 2, by 5, or by a power of 5. While, therefore, the even number 2 and its *powers* seem preferable

preferable as the basis of the distribution; 5 and some of its *powers*, as well as some of the *common multiples* of these numbers, may be conveniently employed wherever their introduction may seem expedient.

The division of weights and measures here submitted is, we believe, nearly the same as the one which has been recommended to the Committee of the House of Commons by Professor Playfair and Dr. Wollaston.

The History of Whitby and Streoneskath Abbey. By the Rev. GEORGE YOUNG, *with the Assistance of some Papers left by the late Mr.* R. WINTER, *and some Materials furnished by Mr.* J. BIRD. Two volumes 8vo.

A considerable portion of this work, as might be expected from its title, is devoted to antiquities, statistics, and local history; but some interesting remarks also occur on the mineralogy of this interesting part of Yorkshire, which we shall probably lay before our readers in our next number.

The above mentioned gentlemen Messrs. Young and Bird are, we are happy to say, preparing for publication by subscription, a more extensive work in a quarto volume—A Geological Survey of the Yorkshire Coast, illustrated with a Mineralogical Map of the district, and numerous engraved sections of the strata.

Mr. William Phillips will shortly publish the Third Edition of his little work, entitled "Outlines of Mineralogy and Geology;" with some additions. From this edition however will be excluded the few pages annexed to the last, as "An Outline of the Geology of England and Wales;" which, together with the map accompanying it, will shortly be published separately, but with large additions, under the title of "A Selection of Facts from the best Authorities, arranged so as to form an Outline of the Geology of England and Wales, with a Map and Sections of the Strata, designed for the use of the Student."

The First Part of a much-improved System of Arithmetic has just been published by Mr. Dowling, of Highgate, author of the Key to Hutton's Course of Mathematics, and Master of the Mansion-House Academy.

This little volume exhibits a great deal of novelty combined with a degree of neatness and precision deserving of praise.

The Stockholm papers announce the publication there of Travels in England, by M. Broling, Counsellor of Mines. The work, which consists of three volumes octavo, and is embellished with 37 copper-plates, is represented as peculiarly interesting in whatever relates to our industry, manufactures, and mines. This work has gained the premium of the Swedish Academy for the best work published during the year; and the proprietors of Forges subscribed for 300 copies.

XXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Thursday, Jan. 8. PART of a paper by Dr. Brewster, On the Polarization and Refraction of regular Crystals, was read. The observations of Dr. B. are founded on experiments made upon upwards of 160 different crystals, in the course of which Dr. B. has discovered various new properties of light, which he promises to communicate in a series of papers to the Society.

Jan. 15. The remainder of Dr. Brewster's paper presented at the last meeting was read.

Jan. 22. A paper was read by Sir Everard Home, containing some further Observations on the Fossil Remains of an Animal, on which two papers by Sir Everard Home have already appeared in the Transactions of the Society.

A part of a communication was also read from Captain Henry Kater, of a Series of Experiments made by him for determining the length of the seconds pendulum in the latitude of London.

Jan. 29. The reading of the Account of Captain Kater's Experiments concluded.

Feb. 5. A paper on the Fallacy of the Experiments in which Water is supposed to be produced by the Decomposition of Chlorine, was communicated by Sir H. Davy.

In this paper Sir H. Davy shows, that when water appears during the action of muriate of ammonia or muriatic acid gas on metals, it either pre-exists in the substances used in its known form, or is produced by the action of the hydrogen of the muriatic acid or of the ammonia in the metallic oxides of the glass; or by the action of hydrogen in common air accidentally existing in the apparatus.

Pure chlorine freed from vapour by muriate of lime yields no water when acted on by metals, and muriatic acid yields no water which cannot be accurately accounted for by the circumstances of the experiment. So that the author's theory of chlorine *is supported* and not overturned by experiments of this kind carefully made.

Feb. 12. Another section of Dr. Brewster's communications On the Laws of Polarization and Double Refraction was read.

Feb. 19. A paper by Mr. George Rennie jun., communicated by Dr. Thomas Young, was read, containing an account of a variety of experiments made for determining the relative strength of materials.

Feb. 26. A paper by The Rev. J. Brinkley, Professor of Astronomy in the University of Dublin, On the Parallax of the Fixed Stars, was read in part—to be concluded at next meeting.

ROYAL

ROYAL ACADEMY.

Monday, 16th February, Mr. Fuseli delivered his first Lecture upon Painting. The Professor observed that as history, by deviating from its proper object, degenerates into biography, and swells to a huge catalogue of individual men; so art, by a similar perversion, has its general process and advancement absorbed in the contemplation of particular genius, the nurselings of different schools. The same process that distinguished the schools of Greece, marked the Italian; it was the universal process of nature;—preparation, establishment, decline.

We owe the restoration of style to *bas-relievo.* It first pended between a copy of barbarity and an impotent imitation of the antique, till it found a basis in Massaccio, though it continued to totter till the appearance of Leonardo da Vinci. The powers of this artist were never consistent. Sometimes he attained the summit of excellence in character--sometimes was content with mediocrity—sometimes degenerated to caricature: his province was confined to the expression of male forms: the female was not within the scope of his genius. The ever exact similarity of all his women makes them to appear daughters of one common mother. It was in Michael Angelo that art attained its full establishment. Whether his encomiasts or his critics were most capable of judging of his powers, is not determined; yet both date from him the full perfection of art. His were the stamina of nature; his the real feelings of humanity:—as sculptor, as architect, he attempted, and, above every other man, succeeded in uniting, magnificence of plan and endless variety of subordinate parts with the utmost simplicity and breadth. The child, the female, meanness, deformity, were by him indiscriminately stamped with grandeur; a beggar rose from his hand, the patriarch of poverty; the hump of his dwarf is impressed with dignity; his women are moulds of generation; his infants teem with man; his men are a race of giants. Sublimity of conception, grandeur of form, breadth of manner, are the elements of Michael Angelo's style.

The milder genius of Raphael succeeded to the inspiration of Michael Angelo: the latter had no infancy, or, if he had, we are not acquainted with it; like an oriental sun he burst upon us, in all the splendours of perfection. Raphael we view in infancy; Propriety rocked his cradle, Character ennobled his limbs; by profound, precise, acute observations upon the antique, and by subsequently as admirable application, he erected his fabric of excellence. Raphael's was the unrivalled province to embody effulgent goodness, in the person of our Saviour. Roundness, mildness, and insipidity characterize his Madonas; transcripts of the nursery, or servile copies of a face he liked. When Raphael attempts

tempts pure character, he stands unrivalled. Design and style were the bases of the plans of these painters; but the principle of the Venetian was very different—colouring. Titian, its master, copied nature at first in a juvenile manner, till he learned breadth of form and colour from Giorge. The Professor gave the character of Titian with great energy and acuteness, summing up by the observation, that he was the father of portrait-painting, and as yet in that branch has stood unrivalled; he was the first that united character and resemblance with dignity.—A national character pervaded the brightest æra of the Venetian school.

The soft transitions from convex to concave, and the coalition of light and darkness by imperceptible blendings are the elements of Correggio's style. This inspires his figures with grace, and to these their grace is subordinate: his was an incomparable hue of colour, and suavity of form. Correggio was emulated by Parmigiano, though not always with equal consistency and propriety. While Michael Angelo was doomed to live and brood over the perversion of his style by the Venetian painters, death prevented Raphael from witnessing the gradual decay of his.—The compact style of Polydoro, formed upon the antique, was the principal meteor of merit, before the appearance of the Carracci, who founded at Bologna that electric school, which, by selecting the beauties, correcting the faults, supplying the defects, and avoiding the extremes of the different styles, attempted to form a perfect system. Among the various schools of the Carracci, called the school of Bologna, the art gradually declined from mediocrity to evanescence, from evanescence to oblivion.

Thursday, 30th Feb. Mr. Fuseli delivered his second Lecture in continuation. It was observed, that the superior claim which the effects of design have upon our attention, would seem to be powerfully implied by their duration;—words become obsolete, sounds expire, colours fade; in forms alone, can idea be rendered permanent.

Mr. Fuseli said, that in the language of the art, the words *copy* and *imitation* were generally confounded, though essentially different in their meaning; precision of eye and obedience of hand being, in truth, the requisities of the former: while choice, directed by judgement and taste, constitutes the essence of the latter. It had been said within the Academy, by the highest authority, that colour should be used as soon as possible by the student; the Professor wished it particularly to be understood, that this was by no means intended to encourage incorrectness of drawing; it mattered little with what materials correctness of drawing was attained; the accomplishment of it was indispensably necessary. It had been said by the same authority, that if we mean to attain excellence of form, we must scrutinize the

principles

principles upon which the ancients worked. Their ruling principle was correctness. The name of Apelles, in Pliny, is the synonym of unrivalled excellence; but the enumeration of his works neither comprises exclusive sublimity of invention, the most acute discrimination of character, the widest sphere of comprehension, nor the best balanced composition;—correctness was his ruling principle. That his excellence was built upon this firm basis is well known, from his famous and well-authenticated contest with Protogenes; from which we may deduce that the schools of Greece recognised colour, grace and taste, as ornaments, not substitutes of form, expression, and character; and that when they usurp that title, they degenerate into splendid faults.

The Professor spoke of imitation under two heads: the first an imitation of nature as it is; this class particularly applied to portraits, and to history, whose excellence was truth:—the second, an imitation of nature in a more extensive view; not distorted by passion, nor modified by disease; imitation of a superior idea of beauty culled from nature; for we are no more able to form an idea beyond nature than we can create a sixth sense.

Mr. Fuseli lamented the frequency of the unsuccessful use of the opportunities afforded by the Academy; and disapproved of the opinion largely entertained and countenanced by high authority, that the students when they entered theLife Academy should copy servilely from the model.

The Professor conceived it was then, that the students were to apply their taste and judgement, acquired in the Antique Academy; he particularly enforced the study of the skeleton, and the science of physiognomy. With respect to the latter, some had ridiculously admitted its application in the parts, but not in the whole: take, added he, one quarter of an inch from beneath the nose of the Apollo, and the *god* is degraded into a common man!

Mr. Fuseli then entered upon the great error of design—*mannerism.* To follow him in the subdivision of this subject, would be too long for our space. Those mannerists who never consult nature but at second hand, and the paltry epitomists of nature's immense volume, were acutely reprobated;—but the least pardonable of all mannerists were said to be those who stoop to meanness for the attainment of grandeur. The wretched taste of Albert Durer, in this respect, was inferior to the infamous caricatures of the great style by Goltzius and Spranger: Raphael himself, in his former days, was under the influence of this base mannerism. The lecture concluded with advice to the students; during which the Professor observed, that the Arts were not children of necessity, nor the nurselings of fashion; but that

they

they are the offspring of fancy, the ornaments of society, the glory of men; what none absolutely want, what all wish for, what few are able to attain: that a trusty labourer, an honest tradesman, an industrious hireling, &c. are more useful members of society, of more service to the state, than an artist of mediocrity.

ROYAL SOCIETY OF EDINBURGH.

January 14. Dr. Brewster communicated a very interesting paper, consisting of extracts of letters from Mr. Boog to his father, the Rev. Dr. Boog of Paisley, giving an account of the recent discoveries respecting the Sphinx and the principal Pyramid of Egypt, which have been made by Capt. C. and Mr. Salt. By very laborious excavations, which were made in vain by the French Savans, these gentlemen have discovered that the Sphinx is cut out of the solid rock on which it was supposed merely to rest. They found that the short descending passage at the entrance to the pyramid, which afterwards ascends to the two chambers, was continued in a straight line through the base of the pyramid, into the rock upon which the pyramid stands. This new passage, after joining what was formerly called the well, is continued forward in a horizontal line, and terminates in a well ten feet deep, exactly beneath the apex of the pyramid, and at the depth of 100 feet below its base. Captain C. has likewise discovered an apartment immediately above the King's chamber, and exactly of the same size and the same fine workmanship, but only four feet in height.

It is a curious fact that this very room over the chamber contains the *Sarcophagus*, which is fully described in the Journal of Mr. Davison (who was formerly British consul at Algiers), inserted in Walpole's Memoirs relating to European and British Turkey, p. 354–357. So far back as the year 1765, this gentleman discovered this chamber, on the 9th of July, of which he has given an accurate description. Maillet, who was forty times in the Pyramid, had not seen it; Niebuhr did not observe it, but afterwards heard of its existence from Mr. Maynard, the person who accompanied Mr. Davison in his visit to this pyramid; nor has it ever been explored by any person since, till Capt. C—— and Mr. Salt's visit: its existence has even been doubted by Dr. Hales (*Chronol.* i. 384). Bruce alludes to Mr. D.'s discovery.

LINNÆAN SOCIETY.

Tuesday, Dec. 20, was read before the Society a Description of the island of Tristan da Cunha, with some account of its productions, by Dugald Carmichael, Esq. Captain in the 72d regiment.

regiment. Captain C., from motives of laudable curiosity, solicited leave to accompany the expedition from the Cape, which sailed in the ship Falmouth to take possession of this very interesting island, and which was provided with the necessaries for establishing a settlement there. He gives a copious account of its natural history. It is a solid rock rising abruptly from the sea in the form of a truncated cone, at an angle of 45 degrees, 3000 feet, surmounted by a dome 5000 feet high, on which is a volcanic crater. The lower part of the mountain is generally surrounded by clouds, and scarcely a day passes without rain. The whole is in a great measure covered with shrubs and with long grass to a considerable height. The animals which inhabit the island are two or three species of seal, various kinds of sea-fowl, and wild hogs and goats, which were probably left by some early navigators. These as well as the vegetable productions are fully described by Capt. C., who also gives an account of a difficult and hazardous journey to the summit. The animals are described as so tame that it was necessary to clear a path through the birds, which were reposing on the rocks, by kicking them aside; and one species of seal did not move when struck or pelted, and at length some of the company amused themselves by mounting them, and riding them into the sea.

Also an account of a new kind of wheat discovered in the United States, and peculiarly adapted to the climate, from its enduring the frosts without injury, in a letter from Governor H. de Witt Clinton.

A letter was read from the Rev. Revett Sheppard, on a powder secreted by birds, of the same colour with their plumage. An account of this kind by Bruce has been ridiculed and discredited, as to certain birds in the mountainous districts of Abyssinia; but his assertion is remarkably confirmed by Mr. Sheppard's observing the same circumstance in a heron which he shot a few weeks since.

Feb. 3. Observations by Mr. E. Burton on the Natural History and Anatomy of the Frigate Bird.—This bird is the *Pelicanus Aquila*, Linn.—called by seamen Frigate-bird, Man-of-war-bird, Sea-eagle, and Halcyon. In vast numbers on the Island of Ascension. Mr. Burton premises that the specimens procured by him differ materially from Linnæus's. It does not swim, but takes its prey skimming the surface of the water. It can only begin its flight by climbing to the edge of a precipice. Flies immense heights, and for hundreds of miles without a resting place.—The paper concluded by a minute description of its anatomy, compared with its habits, &c.

SOCIETY

SOCIETY OF NATURAL KNOWLEDGE OF ZURICH.

The Society of Natural Knowledge at Zurich met during the 6th, 7th, and 8th of October 1817. The first prize offered by the Society presents a question of the highest interest. Several men of learning have asserted, and several others have repeated after them, that the climate is become insensibly more rough and cold in the elevated parts of Switzerland. In the absence of direct proof from thermometric observation, the four following circumstances have been urged as facts in support of the opinion alluded to:

1. Historical evidence of many parts of the Alps having been once pasturage, which are now wholly unproductive. 2. Historical evidence and still remaining traces of the existence of forests above what now is the boundary of the vegetation of trees. 3. The progressive lowering of the line of permanent snow. 4. The progress made by the *Glaciers* in many parts of Switzerland.— The following is the question proposed upon this interesting subject:—"Is it true that the Upper Alps of Switzerland are become more rough and cold during a series of years?" This question requiring an answer by facts only, the Society requires of the candidates—"1. That they shall collect the testimonies, ancient and modern, of the deterioration and decay of pasturage in the Upper Alps. 2. That they shall submit the authenticity of these testimonies to a critical examination. 3. That they shall distinguish the instances of ancient pasturage become sterile by other causes than cold, such as the decomposition of impending rocks, avalanches, &c. 4. That they shall examine the historical evidence of the vegetation of trees existing above the present boundary. 5. That they shall collect the greatest possible number of observations relative to the height of the boundary line of snow, and the epoch at which in different years cattle have descended from the Upper Alps. 6. That they shall bring together the observations of a series of years on the increase and partial diminution of the *Glaciers* in the transverse valleys, and their formation and disappearance in the higher regions. Lastly, that they shall investigate the ancient limits of certain *Glaciers* marked out by the fragments of rocks which they drove before them."

If to all these researches it were possible to add authentic details respecting the neighbouring mountains of Savoy and the Tyrol, it would be a great advantage towards the solution of the general question. The essays are to be written in German, Latin, or French, and sent in to the President of the Society before the 1st of January 1820. The first and second prize of 600 francs and 300 francs are to be decided in the same year.

XXVII. *Intelligence and Miscellaneous Articles.*

DEFENCE OF NAUTICAL ALMANAC.

Feb. 9, 1818.

Sir,— Looking over your valuable Magazine for December last, I was struck with the severe critique on the Commissioners of the Board of Longitude and the Conductors of the Nautical Almanac, in a letter of the 20th of that month, addressed to you from the pen of *Astronomicus*. I immediately, at my leisure hours, began computations for the purpose of ascertaining the truth or falsehood of that part of his letter (and to that part only I shall confine my observations) which contains what he *supposes* corrections of astronomical errors; and after a very minute investigation, it affords me no small degree of pleasure and satisfaction in being able to refute and contradict his assertions with the utmost confidence.

He *first* asks why the occultation of Mars in January (1820) is omitted? Now, by very accurate calculations, I find there will be *no* occultation; but there will be, as the Nautical Almanac states, a near appulse, and the moon and planet *may* apparently be in contact on the northern limb of the moon. He observes, secondly, also, that the true conjunction of Mars will take place *twenty minutes* later than is there stated. I should have readily allowed the propriety of this affirmation, provided Mars had at the time, a *progressive* motion in longitude as well as the moon: this not being the case, however, but, on the contrary, assuming an *apparent retrograde* one, the two celestial bodies will be in one and the same minute of a degree of the ecliptic, or in conjunction *precisely* at the time stated, viz. at *twenty minutes* past seven o'clock in the morning of the 2d January; and I would advise *Astronomicus* by all means to make himself better acquainted with the subject in general, and, in the last instance, to get a knowledge of the difference between a *progressive* and *apparent retrograde* motion of a planet, before he again *attempts* to bring the Nautical Almanac and its conductors into disrepute. He again notices with an equal degree of positiveness, that the commencement of the solar eclipse in September is set down full *one minute* later than it ought to be. But there is no more veracity in this assertion than his former ones, its beginning being September 7, at $0^h\ 24^m\ 11^s$: and as to the omission of the point *where* (not *when*) the moon makes the first impression on the sun's disc, 49° 16′ (not $48\frac{1}{4}°$) from the vertex; it has not been usual to insert it, nor is it, I conceive, of any consequence whatever, otherwise the late or the *present* Astronomer Royal would, no doubt, have ordered its insertion. Lastly, he observes that on casting his eye over the configurations of Jupiter's satellites

tellites for the month of January, he finds the position of almost all of them erroneous. This assertion is couched in terms of so broad a latitude, that it requires no other answer than that (I sincerely believe) it proceeds *entirely* from his unskilfulness and his inattention to Mr. Vince's directions relative to the computation of the configurations in the months of January and February in years called *Bissextile*.

I am, sir, your obedient servant,

To Mr. Tilloch. MANCHESTRIENSIS.

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for January 1818, it appears that during that month the following was the work performed by the engines reported, with each bushel of coals.

		Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
22 common engines averaged		22,188,313	various.
Woolf's at Wheal Vor	..	30,834,438	17·2 lib.
Ditto Wh. Abraham	..	41,847,961	16·8
Ditto ditto ..	..	27,942,875	4·76
Ditto Wheal Unity	..	31,900,613	13·1
Dalcouth engine ..	..	42,622,141	11·2
Wheal Abraham ditto	..	32,239,445	10·9
United Mines engine	..	36,396,841	16·9
Treskirby ditto ..	..	38,733,954	10·6
Wheal Chance ditto	..	28,496,996	8·9

OXALIC ACID, FERMENTATION, CELESTINE, &c.

Extract of a Letter from Professor WURZER, *of Marburg, to Professor* VAN MONS.

I have lately found the oxide of iron and a trace of the oxide of manganese in a human calculus. I have met with both the same oxides in pulmonary concretions, which a patient afflicted with pneumonia, and of which he was cured, had ejected.

Our friend M. Dobereiner has found that the oxalic acid as well crystallized as effloresced (this last combined with a proportion of water); so that the oxalates even to that of lime are decomposed by concentrated sulphuric acid, with which they bear a great affinity. The oxalic acid resolves itself into equal volumes of carbonic acid and gaseous oxide of carbon. As these proportions of volumes correspond exactly with those of the weights furnished by analysis of the acid, the oxalic acid may be considered as carbonate of oxide of carbon—at all times as a combination which in the free acid is maintained in composition by water, and in saline combinations by oxide bases, and which of course ceases to exist

 the

the moment these combinations are broken. Oil of vitriol is also a good reactive, not only to discover the oxalic acid, but also to determine the affinity; for the citric, tartaric, &c. acids, being put in contact with the sulphuric acid at the ordinary temperature, do not give any gas.

New experiments on fermentation have convinced M. Dobereiner that the smallest parcels of sugar concealed in any liquid may be discovered, and their affinity determined, by adding to such liquid some grains of yeast and inclosing the mixture in a vessel sealed with mercury. The fermentation, which at the temperature of from 15 to 20 degrees R. begins to manifest itself, and continues as long as any sugar remains, occasions the disengagement of a quantity of bubbles of carbonic acid gas, from which the quantity of sugar which the yeast has decomposed may easily be calculated. M. D. has found that five grains of sugar, dissolved in a half cubic inch or in a whole cubic inch of water, and put in contact with some grains of yeast, uniformly resolve themselves into 4·7 cubic inches of carbonic acid gas and 2·57 grains of alcohol. He considers the sugar to be a saline compound formed of three affinities, or $3 \times 7{\cdot}7$ of deutero-hydroid of carbon, and three proportions, or $3 \times 20{\cdot}7$ of carbonic acid. Alcohol according to him is composed of three proportions of deutero-hydroid of carbon and a proportion of carbonic acid.

You know that in the vicinity of Jena, celestine is met with in great quantities. M. Doberenier has had occasion to ascertain by a long series of experiments the *stacchiometrique* value of strontian. He has found that the number representing that earth is 50, the hydrogen being 1 and the oxygen 7·5; but this number is exactly the arithmetic mean of those which represent the *stacchiometrique* value of lime (27·5) and of barytes (72·5), *i. e.* $\frac{27{\cdot}5 \times 72{\cdot}5}{2} = 50$. M. Dobereiner was doubtful for a time as to the existence of strontian; but this has satisfied him as well by synthetic as analytical experiments. A circumstance still more remarkable is, that the specific weight of sulphate of barytes (celestine) is also the arithmetic mean of that of pure sulphate of lime and of spath, viz. $\frac{2{\cdot}9 \times 4{\cdot}40}{2} = 3{\cdot}65$; which is still a further proof that celestine is a mixture of equal *stacchiometrique* parts of anhydrite and spath.

I ought not to omit that M. Dobereiner has discovered a considerable quantity of sugar in the aqueous extract of the root of the *calamus*, obtained by the aid of a filter-press of Reäl. Fromsdorff, in his analysis of the same root, speaks only of a sweetish extract, which he describes as insusceptible of fermentation.

TEST

TEST FOR ARSENIC.

Mr. Hume of Long Acre, who more than twenty-eight years ago directed the public attention to the excellence of silver as a test for arsenic, and has ever since continued improving the discovery by a variety of relative experiments, has been again called forth in defence of the validity of the test, in consequence of an opinion thrown out by several professional men, on the occasion of a recent trial at Launceston for murder by poison—that nothing short of the revivification of the arsenic into its *metallic* form can or ought to be accepted as evidence in cases of medical jurisprudence. In a paper which Mr. Hume has just published on the subject, he combats this theory in a very able manner, and refers in his support to some of the principal parts of his former communications to the public. The first public communication in which he announced *silver* to be the most effectual test for detecting arsenic, appeared in the Philosophical Magazine for May 1809. As it is evident that the operation must be by *double* chemical affinity or elective attraction, Mr. H. then advised subcarbonate of *soda* to be joined to the arsenic, and nitric *acid* to the silver. In another communication to the Phil. Mag. of August 1812, Mr. Hume detailed a variety of experiments which he had made with *ammonia*, from which he was led to form a *triple* salt, either with silver or copper and that alkali, which he considered to be a test of most valuable acquisition to analytical chemistry, since by its assistance we are now able to ascertain the precise quantum of alkali required, which could not otherwise be readily obtained. This test possesses also another property;—it does not affect phosphate of *soda*: other phosphates it is true exist in the human system, especially in the urine; but there seems little to fear from the presence of any soluble phosphate whatever in fluids mixed with extraneous matters ejected from the human stomach. The last paper on the subject by Mr. Hume, appeared in the Phil. Mag. of October 1812, in which he added an account of a variety of other experiments which he had made, in order to ascertain the comparative eligibility of magnesia, barytes, lime, and other earths, as tests for arsenic.

NORTHERN EXPEDITION.

The arrangements for the vessels about to explore the Arctic Regions are now nearly completed, and it is expected they will leave the river about the 24th of March. Every precaution has been taken for the general comfort of the crews; fixed bed places are fitted, with sliding doors, for the men to sleep in, housings to form roofs over the ships in the event of being frozen in, a liberal supply of vegetables, and a proportion of six months beef, slightly corned, with some preserved meat, will be supplied.

The Isabella and Alexander are intended to proceed in a N.W. direction to Davis's Straits, and explore there for a passage through into the great Pacific Ocean, by the American Continent.

The Dorothea and Trent, proceeding to the eastward of Greenland, will take a northerly direction, in the hopes of reaching the Pole, and from thence to Behring's Straits.

The Isabella is of 382 tons, and has a complement of 47 men: Captain John Ross, commander.

The Alexander is of 250 tons, complement 33 men: Lieutenant W. Edw. Parry, commander.

The Dorothea is of 369 tons, complement 47 men: Captain David Buchan, commander.

The Trent is of 250 tons, complement 33 men: Lieutenant J. Franklin, commander.

An ample supply of warm clothing will be provided, and three months advance of pay given to the men. The officers will have their pay doubled, and six months in advance.—A compensation will be granted the purser in lieu of balance bills; indeed, the whole arrangements appear on a scale of liberality that will do justice to the projectors of the expedition.

If unsuccessful, it is expected to terminate about September 1819. If it be successful, and the navigators return by the Indian Seas, a reward of 20,000*l.* will be distributed amongst the crews. Notwithstanding this, and an allowance of 3*l.* per month, a difficulty is found in obtaining suitable hands for the voyage, and the vessels are to complete their crews at the Orkneys, the great rendezvous of seamen for the Greenland service.

"If an open navigation should be discovered across the Polar Basin, the passage over the Pole, or close to it, will be one of the most interesting events to science that ever occurred. It will be the first time that the problem was practically solved, with which the learners of geography are sometimes puzzled—that of going the shortest way between two places lying east and west of each other, by taking a direction of north and south. The passage of the Pole will require the undivided attention of the navigator. On approaching this point, from which the northern coasts of Europe, Asia, and America, and every part of them, will bear *south* of him, nothing can possibly assist him in determining his course, and keeping on the right meridian of his destined place, but a correct knowledge of the *time*, and yet no means of ascertaining that time will be afforded him. The only *time* he can have, with any degree of certainty, as long as he remains on or near the Pole, must be that of Greenwich, and this he can know only from good chronometers; for from the general hazy state of the atmosphere, and particularly about the horizon, and the sameness in the altitude of the sun, at every hour in the four and twenty, he must not expect

expect to obtain an approximation even of the apparent time, by observation, and he will have no stars to assist him. All his ideas respecting the heavens, and the reckoning of his time, will be reversed, and the change not gradual, as in proceeding from the east to the west, or the contrary, but instantaneous. The magnetic needle will point to its unknown magnetic Pole, or fly round from the point of the bowl from which it is suspended, and that which indicated north will now be south; the east will become the west, and the hour of noon will be that of midnight."—*The Quarterly Review.*

AFRICAN EXPEDITION.

A letter from Sierra Leone mentions the return to that place of the scientific expedition for exploring the interior of Africa. They were completely unsuccessful, having advanced only about 150 miles into the interior, from Rio Nunez. Their progress was there stopped by a chief of the country; and after unavailing endeavours, for the space of four months, to obtain liberty to proceed, they abandoned the enterprise, and returned. Nearly all the animals perished. Several officers died, and, what is remarkable, but one private, besides one drowned, of about 200. Capt. Campbell died two days after their return to Rio Nunez, and was buried, with another officer, in the same spot where Major Peddie and one of his officers were buried on their advance.

RUSSIAN VOYAGE OF DISCOVERY.

Captain Krusenstern in a letter to Captain Burney*, dated Revel, Oct. 1, 1817, informs him that letters had been received a few days before from Lieut. Kotzebue. On leaving Kamschatka in July 1816, he sailed through Behring's Straits, and succeeded in ranging the coast of America to latitude 67°, when he discovered a large inlet extending far to the eastward. He was obliged to quit it without exploring the whole, but intends to resume the labour this year. Captain Krusenstern does not himself believe that a communication exists between the North Pacific and the Atlantic, but remarks that the discovery of this inlet does hold out some hope that one may be yet found.

NEW NAUTICAL INSTRUMENT.

"Mr. Hunter, of Edinburgh, has invented an instrument which is of great importance to the navigator. From two altitudes of the sun, and the interval of time between the observations, he can determine, within five minutes after the second observation, the latitude of the place, the hour from noon, and the variation of the compass. According to the common form of

* Published in the last number of the Quarterly Journal.

calculation for double altitudes, the latitude, by account, is supposed to be known, which, in the use of this instrument, is not necessary. I have tried it in several examples, and always found the results very near the truth. If a vessel were driven from her course by storms or currents—if the reckoning was altogether lost, and the mariner could not get a meridian observation—with this instrument and a chronometer, he could, in a few minutes after the second observation, ascertain his position on the ocean with accuracy. An invention of so much utility in navigation is worthy of encouragement from those concerned in the commerce of the country.

"Glasgow Observatory, Dec. 3, 1817. "J. Cross."

MOVEABLE AXLE FOR CARRIAGES.

A useful and ingenious improvement applicable to all four-wheeled carriages has just been announced, possessing the following advantages: A carriage with this moveable axle will turn in much less space than with the old axle; may be built from 15 to 18 inches shorter than on the old principle, and affords complete security against upsetting: the fore wheels may be made higher, while the body may be hung lower, which facilitates the progress of the carriage where impediments present themselves arising from inequalities in the road. This invention has been brought forward by Mr. Ackermann of the Strand, well known as the publisher of many ingenious and useful works. Already many of our first coach-makers are busy in applying this improvement to use.

ANIMAL FLOWER.

The inhabitants of St. Lucia have discovered a most singular plant. In a cavern of that isle, near the sea, is a large bason, from twelve to fifteen feet deep, the water of which is very brackish, and the bottom composed of rocks. From these, at all times, proceed certain substances, which present, at first sight, beautiful flowers, of a bright shining colour, and pretty nearly resembling our marigolds—only that their tint is more lively. These seeming flowers, on the approach of a hand or instrument, retire, like a snail, out of sight. On examining their substance closely, there appear, in the middle of the disk, four brown filaments, resembling spiders' legs, which move round a kind of petals with a pretty brisk and spontaneous motion. These legs have pincers to seize their prey; and, upon seizing it, the yellow petals immediately close, so that it cannot escape. Under this exterior of a flower is a brown stalk, of the bigness of a raven's quill, and which appears to be the body of some animal. It is probable that this strange creature lives on the spawn of fish, and the marine insects thrown by the sea into the bason.

QUERIES.

QUERIES.

To Mr. Tilloch.

Sir,—An answer to the following queries from some of your learned correspondents, will much oblige one of your constant readers.

1. Has mercury in the state of vapour ever been combined with hydrogen gas?

Supposing a quantity of mercury to be heated in a retort, and in another, hydrogen gas to be produced, from a solution of zinc, could the fumes arising from the two retorts be safely attempted to pass into a heated tube of iron or porcelain?

2. Do the precipitates of lime, magnesia, or other pure earths, from watery solutions, assume regular crystallized forms? Have they been subjected to microscopical observation?

YELLOW DYE.

A chemist of Copenhagen has discovered a brilliant yellow matter for dyeing, in potatoe tops. The mode of obtaining it is, by cutting the top when in flower, and bruising and pressing it to extract the juice. Linen or woollen soaked in this liquor during forty-eight hours takes a fine, solid and permanent yellow colour. If the cloth be afterwards plunged in a blue dye, it then acquires a beautiful permanent green colour.

PLUMBAGO.

A few months ago a new mine of this valuable substance was discovered in Glenstrathfarar, county of Inverness, in a schistose rock close to the Farar. It crops out to an extent of not less than fifty feet in five different seams, some of them from twelve to fifteen inches in thickness. Several tons of it were raised last summer. It seemed to improve much as the miners penetrated deeper, and the seams to thicken and run into one. Only two other mines of this substance are worked in Britain; one near Cumnock in Aryshire, the other at Borradale in Cumberland. The produce of the latter is so valuable that the finer pieces sell for two or three guineas a pound weight.

LITHOVASA.

This name is given to a new but useful article, made of a peculiar kind of stone, in the form of vessels adapted to cool wine, preserve butter, &c. They owe their properties to the power of absorption and evaporation possessed by the stone; and are superior to earthenware articles applied to the same purposes, being entirely free from that clayey smell which belongs to unglazed pottery.

The wine coolers require only to be steeped for ten minutes in cold water, when they are fit to receive a decanter of wine.—The butter preservers steeped in the same manner are ready to receive a vessel a vessel containing the butter, and will keep it cool in the hottest weather, and retain their moisture for a day or two.

Elegant stone pyramids for growing excellent anti-scorbutic salads, require only to be saturated with water. The seed equally distributed in the external grooves, the central hole filled with water, (and the waste daily supplied,) will in eight or ten days produce a fine green crop of very superior quality, which may be eaten clean and fresh from the pyramids placed on the table.—When the crop is plucked from any number of grooves, and the loose seeds brushed off, new may be sown and successive crops obtained.

To persons on board ship and in warm climates these articles must prove highly useful. They may be had at 448, Strand.

LECTURES.

Mr. Clarke will commence his next Course of Lectures on Midwifery, and the Diseases of Women and Children, on Friday, March 20th. The Lectures are read every Morning from a Quarter past Ten to a Quarter past Eleven, for the convenience of Students attending the Hospitals. For particulars apply to Mr. Clarke, 10, Saville-row, Burlington Gardens.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Scott, of Pengo-place, in the county of Surrey, esq., for improvements in steam-boats, and in the machinery for propelling the same.—23d Jan. 1818.—Allowed 2 months to enroll.

To James Ikin, of William-street, in the parish of Christ-church, Surrey, machinist, for his improved method or methods of constructing or manufacturing fire or furnace bars or gratings. —27th Jan. —2 months.

To George Frederick Hagner, late of Philadelphia in the United States, but now of the Adelphi, Middlesex, for certain improvements in the art of manufacturing pigments commonly known by the names of white-lead and verdigris.—27th Jan.— months.

To Rudolph Ackermann, of the Strand, print-seller, for certain improvements in axletrees applicable to four-wheeled carriages, communicated to him by George Lenkensperger, of Munich in the kingdom of Bavaria.—27th Jan.—6 months.

To William Horner, of Howick, Northumberland, for his machine or apparatus for the purpose of acquiring a very high mechanical power in a small compass and with little friction, and without the possibility of running amain if employed in raising or lowering weights.—27th Jan.—6 months.

To

To George Prior, of Leeds, for his invention for perfectly detaching the escape-wheel of chronometers from the influence of the friction and inaccuracies arising from the main-spring, the pivots, and the teeth of all the other wheels and pinions in the machine during the time of its giving impulses to the balance, whereby its vibration will be more accurately and uniformly supported than by any other invention heretofore made public.—29th Jan.—6 months.

To John Penwarne, of Stafford-street, St. Marylebone, for certain improvements, being an improvement on the cock for drawing beer, cider, and other liquors from casks and other vessels, without the interruption of a vent plug, or any opening whatever in the upper part of the cask or vessel, either for the purpose of admitting air, or for affixing the said instrument or cock or any apparatus or appendage belonging to the same.—31st Jan.—2 months.

To Benjamin Taylor, of Mile-End, near Glasgow, for his loom to work by the power from a steam-engine, which will weave figures or flowers upon either twilled or plain cloth, in either silk, cotton, linen, or worsted, or any of them intermixed.—31st Jan.—2 months.

To Sir Thomas Cochrane, knight, commonly called Lord Cochrane, for his improvement or improvements in the process or processes of purifying a certain spirit or essential oil which is known by the name of spirit of tar or oil of tar, and which is obtained from different ligneous, carbonaceous, or bituminous substances; by means of which improvement or improvements the said oil or spirit will be separated from certain impurities which have hitherto prevented the application of such oil of spirit to divers useful purposes.—3d Feb.—6 months.

To Matthew Cotes Wyatt, of Henrietta-street, Cavendish-square, (in consequence of a communication made to him by a certain foreigner residing abroad,) for a safeguard to prevent the accidental movement of the lock of a gun, pistol, or other fire-arm, forward towards the hammer.—3d Feb.—6 months.

To Jeremiah Chubb, of Portsea, in the county of Southampton, for certain improvements in the construction of locks.—3d Feb.—6 months.

To Daniel Wilson, of Earl-street, for certain improvements in the process of boiling and refining sugar.—3d Feb.—6 months.

To Edmund Naish, of Bristol, hosier, for certain improvements on the machines or machinery used for winding cotton.—3d Feb.—4 months.

To Grant Preston, of Burr-street, Aldgate, for his improvement in the feck glass rim, and on the safety-gate.—3d Feb.—2 months.

To

To Nathaniel Smith, of Kettering, Northamptonshire, for certain improvements on winnowing-machines.—5th Feb.—6 mo.

To Mary Sedgwick, of Bishopsgate Within, starch manufacturer, for her discovery of a valuable product or valuable products from that part of the refuse, slime, or wash of starch that will not of itself subside.—10th Feb.—2 months.

To John Munro, of Finsbury-square, (in consequence of a communication made to him by Barnabas Langdon, of New-York.) for certain improvements on steam-engines.—12th Feb. —4 months.

To John Simpson, of Birmingham, for his improved method of constructing and making spring hooks, or woodcock eyes, and for coach harness, which principle of spring is intended also to be applied to harness and spring swivels.—16th Feb.—2 months.

To Zachariah Barrat, of No. 27, Windmill-street, Tottenham-court-road, for his machine for curing, cleansing, sweeping, and ventilating chimneys, and when chimneys are on fire, for extinguishing the same.—10th Feb.—2 months.

To Thomas Allingham, of Smith-street, Chelsea, for his lamp intended to be called "The œconomical and universal lamp," constructed by means of the flame of the wick being kept in a constant and equal degree of contiguity to the oil, so as to consume (in proportion to the light it gives) a less quantity of oil than other lamps; also giving a continual light of almost unvaried brilliancy.—19th Feb.—6 months.

To John Jones, of the city of Gloucester, for his improvements in certain parts of the machinery or instruments used for dressing of woollen and other cloths.—19th Feb.—2 months.

To James Collier, of Frocester, in the county of Gloucester, for various improvements on a machine now in use for the dressing and gigging of woollen cloths called a gigg.—19th Feb.—2 months.

ASTRONOMICAL PHÆNOMENA, MARCH 1818.

D. H. M.		D. H. M.	
1.21.27	☾ τ ♐	18. .	♃ ν^2 ♐ nearly in contact.
2. .	♀ ♄, ♀ 13′ N.	18. .	♐ 138 ♉ * 15′ N.
4. .	♃ 28 ♐ * 26′ N.	19.11.12	☾ η ♌
4.10.17	☾ ε ♑	20.16.44	☉ enters ♈
7. .	☿ ι ♒ * 6′ S.	22.12.36	☾ γ ♍
9. .	♃ 125 ♉ * 11′ N.	23. .	☾ in perigee
9.15. 5	☾ ο ♓	24. 9. 9	☾ λ ♍
13. .	☾ in apogee	26. 3.38	☾ δ ♏
14.15.10	☾ ♂	28.18.54	☾ φ ♍
15. .	♃ ν^1 ♐ * 4′ S.	29. 2.56	☾ τ ♐
16. .	☿ ♄, ☿ 34′ N.		

Meteoro-

Meteorological Journal kept at Walthamstow, Essex, from January 15, *to February* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between *Noon* and Two P.M.]

Date.	Therm.	Barom.	Wind,
January			
15	50 51	29·60	SW.—Stormy, windy, and cloudy; showers, and wind, stormy night.
16	47 52	29·70	W.—Windy, cloudy, and dry; after 3 P.M. showers and wind; stormy; wind, and rain at night.
17	39 41	29·76	W.—Clear, wind, and some *cirrostratus;* very fine day; sun and wind; moon and star-light, and windy.
18	37 42	29·88	W.—Clear, and windy; very fine day; shower and wind at 3 P.M.; moon-light.
19	29 37	30·33	W.—Clear, and *cirrostratus*; afterwards very fine day; moon and stars at 7 P.M.; then *cumuli;* neither moon nor stars visible.
20	37 42	30·32	S.—Clouds, and white frost; very red sun-rise; fine day; sun, and hazy; very red sun-set; moon-light.
21	36 42	29·90	S.—Clear, and some small *cumuli*; red sun-rise; at 9 A.M. a great storm; very fine sunny day; moon-light bright.
22	33 45	30·00	S.—Clear at 7 A.M.; at 9, hazy; fine day; showers and wind. Full moon.
23	33 43	29·76	SW.—Clear moon-light morning; at 12½ great shower; showers, and wind; bright moon-light night.
24	41 42	29·54	SW—NW.—Rain, and foggy; rain about 1 P.M. fine day after; moon-light.
25	33 40	29·88	W-NW-SW.—Clear, sun, and wind; very fine day; hazy towards evening; cloudy night.
26	47 52	29·76	SW.—Damp, and windy; cloudy day; rain and wind; at 11 P.M. remarkable *cumulostratus,* the moon shining between.
27	34 43	29·87	W.—Clear, and *cirrostratus;* hazy day; windy and dark.
28	37 41	29·54	SW—W.—Cloudy and windy; fine sunny day; a hail shower at 3 P.M.; clear star-light.
29	32 42	29·66	SW.—Clear moon-light morning; fine day; some small rain about 2 P.M.; showers and wind. Moon last quarter.
30	41 45	29·00	SW.—Great rain, and wind which continued till after *noon;* windy and cloudy; star-light.

January

Date.	Therm.	Barom.	Wind.
January			
31	36 38	29·21	NW—Clear, *cumuli*, and windy; very fine day; clear, star-light.
February			
1	38 40	29·10	S.W.—Rain and wind; at 9 A.M. great shower of rain and large flakes of snow; fine day; sunshine; clear star-light.
2	27 34	29·00	S—N.—Clear, and white frost; cloudy pleasant day; star-light.
3	24 39	29·31	S—NW.—Clear and *very* white frost; fine day; hail-storm at 2 P.M. and some snow; cloudy night.
4	28 33	29·11	E—N.—Cloudy; some snow on ground; gleams of sun; a *raw* cold day; bright orange sun-set; dark, but some stars.
5	39 39	29·44	S.—Hazy; fine day, but hazy, and sun throughout; dark night. New moon.
6	27 34	29·88	Clear red sun-rise at 7; at 9 A.M. *thick* fog; sun through the fog; stars early; dark afterwards.
7	28 32	30·00	SE—NW.—Foggy morn; at 10 A. M. *very thick* fog; very foggy day; at night fog so thick that people took torches to light them in the roads,
8	26 32	30·00	SW—W.—Fog, and very white frost; sun through fog all day; very dark and foggy at night, but some stars after 10 P.M.
9	24 34	30·00	W—NW.—Fog, and very white frost; fine winter day; sun through mist; dark night.
10	24 53		SW—E.—Foggy; trees beautifully white; still foggy; thawing and icy drops fall from trees; moon and stars early; dark at 10 P.M.
11	32 37	30·11	NE—E.—No fog; gray morn; very fine day; cloudy, and windy night.
12	31 36	30·20	SE—SW.—Gray morn: about 9 A.M. a fog came on; fog decreased; night cloudy.
13	33 37	30·11	SE.—Gray morn; very fine day; moon and star-light. Moon first quarter.
14	27 37	29·87	E—SE.—Fine pale orange sun-rise, and white frost; very fine day; moon- and star-light.
15	29 41	29·87	E.—Fine clear sunshine, and white frost; fine day; sunny; cloudy after 8 P.M.; and cloudy night.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1818.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Jan. 15	9	55·5	29·55	Stormy
16	10	47·5	29·90	Cloudy
17	11	41·	29·90	Fine
18	12	42·	29·97	Stormy
19	13	38·5	30·53	Very fine
20	14	43·	30·29	Ditto
21	15	44·	30·01	Ditto
22	full	44·5	29·81	Cloudy
23	17	39·	29·60	Ditto
24	18	41·	29·55	Very fine
25	19	40·	30·05	Cloudy—rain in the evening.
26	20	51·	29·75	Ditto
27	21	40·	29·93	Very fine
28	22	40·	29·59	Ditto
29	23	41·	29·71	Ditto
30	24	44·	29·37	Rain
31	25	38·	29·33	Cloudy
Feb. 1	26	40·	29·21	Ditto
2	27	32·	29·04	Ditto—snow P.M.
3	28	33·5	29·25	Ditto ditto
4	29	28·	29·28	Ditto frost
5	new	40·	29·60	Ditto ditto
6	1	37·	30·06	Very fine ditto
7	2	38·5	30·14	Ditto ditto
8	3	37·	30·10	Ditto ditto
9	4	40·5	30·08	Cloudy ditto
10	5	38·5	30·15	Very fine ditto
11	6	32·	30·30	Cloudy ditto
12	7	33·	30·27	Ditto ditto
13	8	30·5	30·14	Ditto ditto
14	9	36·	29·91	Very fine ditto
15	10	41·	29·87	Ditto—rain in the evening.

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For February 1818.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan. 27	37	41	37	29·80	27	Fair
28	40	46	35	·50	26	Fair
29	32	45	45	·42	24	Fair weather, storm at night
30	45	45	40	28·90	0	Stormy
31	36	42	34	29·30	28	Fair
Feb. 1	35	41	32	·10	0	Snow and Rain
2	27	35	32	28·82	0	Foggy
3	27	40	32	29·02	0	Fair
4	28	36	30	·03	0	Cloudy
5	28	45	32	·52	24	Fair
6	29	37	32	·90	0	Foggy
7	27	30	29	30·00	0	Foggy
8	21	28	29	29·97	0	Foggy
9	27	33	28	·92	0	Foggy
10	26	32	32	·50	0	Foggy
11	33	37	33	30·05	0	Foggy
12	33	36	32	·04	0	Cloudy
13	32	40	33	·02	18	Fair
14	33	42	32	29·80	21	Fair
15	32	40	37	·82	16	Fair
16	37	47	46	·90	27	Fair
17	44	54	45	·90	30	Fair
18	44	52	50	·80	0	Rain
19	43	52	44	·82	0	Showery
20	37	49	40	·92	0	Showery
21	44	45	40	·93	0	Rain
22	40	35	32	29·00	0	Snow
23	35	40	40	·72	0	Fair
24	38	46	45	·80	18	Fair
25	47	55	47	·50	27	Fair
26	40	45	35	·56	0	Snow and showery

N.B. The Barometer's height is taken at one o'clock.

XXVIII. *On the Atomic Theory.* By William Higgins, *Esq.*

To Mr. Tilloch.

Sir, — I beg leave to transmit to you the following observations, which I request you will insert in your Magazine. The first part relates to the article *Atomic Theory*, published in Dr. Rees's new Cyclopædia, vol. xxxv. part 2. The author I know, for it is impossible to mistake his prejudiced style of writing.

The writer begins by giving a definition of the *Atomic Theory of Chemistry*. "It is the means of explaining the composition and decomposition of chemical bodies, by considering their ultimate atoms* or particles as peculiar and distinct elementary solids, never changing in their figure, weight or volume, under any circumstance."

The definition is very fair: and I will say with confidence that I was the inventor of this theory, and the first that applied it in the manner above described; and I defy any person to produce any thing to the contrary,—I mean any person that will step forward without confounding dates and facts, as has been the case repeatedly on the subject of the Atomic Theory.

The writer proceeds in continuation, on the supposition of the infinite and definite proportions in which elementary matter might, or might not, combine. This I must pass over, as it has nothing to do with the present discussion.

"Philosophers," says he, "were always satisfied to consider this fact of the limitation of the proportions of bodies, as one of the hidden secrets of nature, as difficult to conceive as the nature of the attraction by which their elements were held together. Berthollet appears to have been the first to attempt this arduous task, in his ingenious work entitled *Chemical Statics*."

As Count Berthollet's work does not materially relate to my Atomic Theory, and as he had written some years after me, I shall make no comments on this part of the subject: I will only say that my *Comparative View* should have, in the history, a precedency of the *Chemical Statics*, where this arduous task, as he calls it, was first attempted with perfect success.

"Chemists have from the earliest times been acquainted with those points which we call mutual saturation, and have been long familiar with those limited augmentations of their proportions called by some doses, and by others particles."

The ancient chemists were well aware that one body required a given portion of another body to saturate it so as to form a neutral compound; but their knowledge went no further,—they

* He should leave out the word *atom*, as being a compound.

had no conception of the laws that regulated the limitation, because they were not aware that bodies united particle to particle and atom to atom in certain but limited proportions. In short, the cause of this law was unknown until I published my *Comparative View.*

The writer tells us that the true nature of metallic oxides was not known until Lavoisier's time :—How could it be known, when their oxidation was solely attributed to a loss of their phlogiston? But the first idea of metals uniting to different doses of oxygen, like sulphur, charcoal and azote, will be found in my *Comparative View.*

"Although chemists have frequently used a language which appeared to show their acquaintance with the real cause of the definite proportions; such as one compound being formed by one proportion, dose or particle, of one of its elements; and another with two proportions, doses or particles *: on the other hand, we find expressions which would favour the idea of indefinite proportions; such as bodies losing a small portion of their oxygen, or absorbing a little oxygen from the atmosphere." The drift of the latter part of this passage will appear presently.

"The most decided language used in any chemical work before the discoveries of Mr. John Dalton †, giving any idea that the doses are limited by distinct atoms, will be found in a work by Mr. Higgins, entitled 'A Comparative View of the Phlogistic and Antiphlogistic Theories.' This work was written for the express purpose of combating the phlogistic theory, and principally in answer to Kirwan's Treatise of Phlogiston.

"In order to show the contradictions and absurdities of the phlogistic doctrine, which under the name of phlogiston confounded a number of bodies which were very different, he exhibited by diagrams a number of chemical operations, in which he supposed the elementary bodies concerned to be ultimate particles, and their immediate compounds molecules. He in the same diagrams also used numbers, which he supposed to be estimates of the strength of affinity of the combining particles. By this means he very successfully showed many of the inconsistencies which must be admitted to explain the phænomena on the phlogistic theory. In this mode of proceeding, however, the numbers expressing the relative attractions served his purpose much more than the consideration of the proportions being caused by distinct atoms; and the language which would induce the belief that he had such a conception of the nature of elementary matter occurs only in a very few parts of his work."

* No such language was used until I had written.

† It would puzzle the first philosophers of Europe to discover any thing new in Mr. Dalton's work, except his errors which I have repeatedly pointed out.

The

The numbers alone would avail nothing, if they had not been coupled with the proportions of particles which constituted different compounds, and *vice versâ* ; and the language expressive of those ideas runs uniformly and conspicuously throughout my whole work.

"After concluding that it is unnecessary to admit the existence of the imaginary substance, phlogiston, in sulphur, he concludes, in page 36, that sulphurous acid is compounded of one ultimate particle of sulphur with one of oxygen, and that sulphuric acid consists of one of sulphur and two of oxygen.

"In the same page he also observes, that water is formed of one ultimate particle of water* united to one of oxygen."

The author next quotes my statement of the constituents of sulphuretted hydrogen, and the porportions which their respective particles bear to each other, and then passes to my estimate of the proportions of the particles of azote and oxygen in nitrous oxide, nitrous air, red nitrous acid, straw-coloured nitrous acid, and in the nitric acid.

"These facts," continues he, "are certainly very remarkable, as they agree with the conclusions in the present time, and give a strong proof of Mr. Higgins's genius at the time he wrote.

"He does not, however, lay any stress upon these remarks, and was not probably aware that they would be confirmed by future research." I was perfectly satisfied that I was right, and that my demonstrations would bear the test of time and investigation †; and the best stress I could set upon them was, to lay them before the public. But he goes on: "We are induced to think so from the manner in which he expresses himself in other parts of his work, in which he frequently speaks of the absorption of small portions of oxygen, and of bodies having a small portion of oxygen more than they can retain."

These remarks do not in the smallest degree invalidate the principles which I advanced. We know that distilled water absorbs oxygen from the atmosphere, that all the sulphites gradually absorb oxygen from the atmosphere, so as, in time, to become sulphates. And many substances contain more oxygen than they can well retain;—instance, nitric acid, euchlorine, oxymuriate of potash, and the oxides of gold and silver, particularly the latter.

"This vague manner of speaking, and others which we do not immediately recollect, is sufficient to show that Mr. Higgins had no fixed notions of the cause of definite proportions; and the language in which he has used ultimate particles and molecules,

* The author made a mistake ; read a particle of hydrogen instead of a particle o water.

† See preface to my *Comparative View.*

was employed rather with a view to illustrate his examples, than to broach any new theory to explain definite proportions. Indeed it would have been inconsistent to have treated two subjects so very different in their objects, in the same pages."

I will now, before I proceed any further with the author of this article, remark that I made no use of vague or equivocal language, and that I entertained fixed notions of the laws of definite proportions, which are fully demonstrated throughout the whole of my *Comparative View.* It is true I gave no name to the novel mode which I adopted for the purpose of my research, --but what is a name but a mere shadow in comparison to the matter itself? Lavoisier never gave a name to his doctrine. Kirwan was the first that gave it the name of the antiphlogistic theory; and I will say that it was not inconsistent to trace the errors of the phlogistians in the same page, and even in the same paragraph, by means of the laws of definite proportions; and it was in consequence of that close investigation that the Atomic Theory started up in my mind; otherwise, in all probability, it would have still remained unknown.

The author tells us in another part of this article, that the reviewer of this work (the Comparative View) in the Analytical Review soon after it was published, took no notice of my diagrams or particles, although he gives me the highest praise for the able manner in which I refuted the doctrine of phlogiston. This he adduces as a proof that there was nothing striking in what I advanced on the theory of definite proportions. The Reviewer, it is true, only observed that " my facts and mode of reasoning were original and striking." What more could be expected at a time when there was no fixed theory, and when the science was almost in a chaotic state? It was impossible that such novel view should all at once be adopted even in the most advanced state or the science of chemistry.

My diagrams were taken notice of in the *Critical Review*, at the time I had written, and the remarks made on them show the ignorance of those days; for they only observed that they were the same with those of Dr. Black. And Dr. Thomson himself, after I published my Essay on the Atomic Theory, &c. mentioned in one of his Journals, (I forget in what number, for I have it not by me at present,) that there was nothing material in those diagrams of mine, for indeed that Dr. Black's were much more pretty than mine. What a scientific expression from a compiler of philosophy!

I scarcely need to tell the reader that Dr. Black's diagrams and mine bear no relation whatever to each other.

But the writer goes on. " It was not enough to know that compound bodies were formed of particles, to enable us to explain

plain the cause of definite proportions; and we want not greater proof of this than the fact of the true cause not being known till twenty-eight years after Mr. Higgins had told us that one particle of sulphur and one of oxygen formed sulphurous acid, and that one to two formed sulphuric acid. These loose expressions were but a small step indeed towards the discovery of the Atomic Theory in its present form, which has placed chemistry on the same ground with that on which the discovery of the laws of gravity placed the science of astronomy."

The above paragraph is written with a great deal of disingenuity, and evidently could only flow from the pen of a prejudiced man. We could never be acquainted with the cause of definite proportion without first knowing that compounds consisted of elementary particles; and the proportions of those particles, the relative forces with which they unite in different compounds, and their relative weights:—all these constitute the Atomic Theory; and those important circumstances are unequivocally, not loosely, to be met with in my *Comparative View*. It was the pride of my life since I had written that work, to feel that "I placed chemistry on the same ground with that on which the discovery of the laws of attraction placed the science of astronomy."

The following quotation from the preface of my Essay on Bleaching, page 18, will show how confident I was that what I advanced in my Comparative View was perfectly just, viz. "I have connected the whole (the facts and phænomena then known) and reduced them to a system, and made use of demonstrations, which in my opinion are not to be invalidated or contradicted, until the order of natural things assume a different aspect."

The above Essay was published in the year 1799, many years before Dalton's work appeared.—But to return to our writer.

"We are inclined to believe that the first step towards this important discovery was given by Richter. He found in the double decomposition of salts, that the acid of one salt was always just sufficient to saturate the base of the other, and *vice versâ*." So far as the decomposition takes place this holds good, but in other respects there are many exceptions.

"He also ascertained, that when one metal was precipitated by another, the oxygen of the precipitated metal was just what was required by the precipitating metal."

I wrote several years before Richter; and many of the chemists of the time at which I published, as well as myself, were acquainted with what this gentleman attributes to Richter*. The ancients

* The mutual saturation of saline bodies on interchanging acids and bases with each other; that is, double decomposition.

knew as well as Richter, that in the gross, one quantity of alkali required a certain quantity of acid to saturate each other,—and what more can he attribute to Richter? It has nothing to do with the atomic theory and definite proportions of particles or atoms.

And as to what relates to metallic precipitations, he is wrong in many respects, as I have shown in my Comparative View, page 263, which the following extract will show: "Should the precipitant be unable to take up the whole of the oxygen of the precipitated metal, it falls down in the state of a semioxide. Thus lead and silver will precipitate gold from its solution of a dull purple colour, while copper and iron throw it down in its metallic state."

I now come to the most singular passage of all, as it exhibits the most glaring prejudice *and ignorance* that could flow from the mind of any man that could have any pretensions to science; it is as follows: "It is the means of drawing these inferences arising from the mutual fitness of those parts of bodies which combine, that constitutes the importance of the Atomic Theory; and it is for the establishment of this new principle that we are indebted to Mr. John Dalton. When Mr. Higgins can show from the data given in his work, that similar inferences could be drawn, he then will be entitled to share in the merit of the discovery of the Atomic Theory. We say share with him; for we are firmly convinced that Mr. Dalton had never read Mr. Higgins's book previous to the publication of his own work."

There is nothing new, as I said before, in these facts, they were known before I wrote my *Comparative View;* and the mutual fitness (which by the by is an odd expression) of some of them, for it does not extend to all saline bodies, was familiar to every experimental chemist, and Mr. Dalton has nothing to do with it; nor does it immediately relate to the Atomic Theory.

"When Higgins can show that similar inferences, &c."

I will adduce some facts which, according to the writer, will "*entitle me to share in the Atomic Theory.*" In the *section* on the *precipitation* of *metals* by each other, page 260 *Comparative View*, will be found a diagram representing the principles on which one metal precipitates another. The precipitation of copper in its metallic state from its solution in sulphuric acid by iron, was adduced as an example. The diagram represents by means of numbers the relative forces of attraction of the different elements in a molecule of sulphate of copper, and also the influence of a particle of iron on each of those elementary principles united to the copper. The play of affinities which enables the particle of iron to strip the particle of copper of the whole of its oxygen and volatile sulphurous acid, so as to leave it in its

pure

pure metallic state, is minutely explained, and is highly interesting*. There are two more diagrams, somewhat different from the former, representing the precipitation of mercury and silver in their metallic state, on the principles of what is also called the Atomic Theory. No such philosophy is to be found in Dalton's work; no, nothing is to be seen there but bombastical and erroneous imitations of my doctrine forced on the public by hirelings. The respectable editor of this useful work will, it is to be hoped, be careful in future who he employs. That the writer of this article should assert that Mr. Dalton never read my work previous to the publication of his own, is rather extraordinary; for no man can know what any other individual reads or does not read. Dalton himself has never denied his having read it—at least publicly. There is nothing else in this article that I had not taken notice of in my observations on the same subject in the *Encyclopædia Britannica*, and which was published in this Magazine. The writer tells us that Mr. Dalton's book was published some time before chemists understood the true spirit of the Atomic Theory. I believe it is not perfectly understood at present, or else I should not have so much trouble to establish my claim. If Mr. Dalton's work was so difficult to be understood in the present day, surely it could not be wondered at, that the original should lie by unnoticed in a more obscure age of chemical philosophy.

On lately casting my eyes over Dr. Wollaston's paper on the Synoptical Scale of Chemical Equivalents, I observed some remarks on my theory, or, as he unjustly calls it, Dalton's theory.

The Doctor, after having given the opinion of different chemists on the relative quantities of acid united to a given quantity of alkaline and earthy bases, observes that, "It could not escape the penetration of M. Berthollet, that there exist numerous deviations from this law of neutralization, and cases of prevailing affinity dependent on a redundance of one or other ingredient in a mixture of salts. But he was not so happy in detecting the definite law, by which many at least of these deviations are governed. It has since been found, that when a base unites with a larger portion of acid than is sufficient to saturate it, the quantity combined is then an exact simple multiple of the former, thus exhibiting a new modification of the law of definite proportions rather than any exception to it.

"The first instance in which the same body was supposed to unite with different doses of another, in such proportions that one of these doses is a simple multiple of the other, was noticed by

* The diagram and explanation may be seen also in my Essay on the Atomic Theory, page 158.

by Mr. Higgins, who conceived rather than actually observed to occur, certain successive degrees of oxidation of azote, and represented the series of its combinations with oxygen to be azote, one with two of oxygen making nitrous gas."

He continues to the end of the series of the combinations of those elements to nitric acid which limits their combination, and marks their definite proportions. But what the Doctor means by the expressions "*conceived, rather than actually observed to occur,*" I do not perfectly understand. It is too ambiguous. If he means that it was not founded on facts, I cannot agree with him; for I have adduced a great many to confirm my positions, which may be found in different parts of my *Comparative View*, but particularly under the section *Nitrous acid.*

If he means that I accidentally stumbled on the idea, I have had a great many such stumbles throughout 280 miles (280 pages), and yet I have not once tumbled. Perhaps he means that I dreamed of the thing; if so, it must be a very happy and a very long and well-connected kind of a dream, such as seldom occurs.

"But," continues the Doctor, "though Mr. Higgins, in the instance of the union of hydrogen and oxygen, anticipated the law of bulks observed by M. Gay-Lussac, with respect to the union of gases, and in his conception of union, by ultimate particles, clearly preceded Mr. Dalton in his atomic views of chemical combination, he appears not to have taken much pains to ascertain the actual prevalence of that law of multiple proportions by which the atomic theory is best supported; and it is in fact to Mr. Dalton that we are indebted for the first correct observation of such an instance of a simple multiple in the union of nitrous gas with oxygen."

I have also shown the proportions in which carburetted hydrogen and oxygen united so as to produce water and carbonic acid gas, and that this gas contained two-thirds of oxygen and one-third charcoal *. In short, I was acquainted with the proportions in which all the gases united in volumes:—and it evidently appears throughout most parts of my work, that I have taken great pains to ascertain the *actual prevalence of that law of multiple proportions by which the Atomic Theory is best supported*; and that it is not in *fact* to Mr. Dalton that we are indebted for the first correct observation of such an instance of a simple multiple in the union of nitrous gas with oxygen. The Doctor could not bring forward a more unfortunate instance than the latter to support his friend, as I have fully proved upon a former occasion in the number of this Magazine for May last. I will now suffer the Doctor to go on.

"Chemists in general," says he, "however, appear to have

* See pages 252—53 Comp. View.

been

been by no means duly impressed with the importance of this observation of Mr. Dalton, till they were in possession of other facts observed by Dr. Thomson and myself, in a more tangible form, with regard to neutral and super-acid or sub-acid salts, &c." He here refers the reader to Phil. Trans. 1808, p. 74 and 96, to which we will now pass.

The paper now under our consideration is entitled, On Super-acid and Sub-acid Salts. By William Hyde Wollaston, M.D. Sec. R.S. Read January 28, 1808.

"Dr. Thomson," says he, "has remarked, that oxalic acid unites to strontian as well as to potash in two different proportions; and the quantity of acid combined with each of these bases in their super-oxalates, is just double of that which is saturated by the same quantity of base in their neutral compounds."

The Doctor tells us that he observed the same law to prevail in various other instances of super-acid and sub-acid salts; and as he considered it general, it was his intention to pursue the subject, "with the hope of discovering the cause to which so regular a relation might be ascribed."

"But since the publication of Mr. Dalton's Theory of Chemical Combinations as explained and illustrated by Dr. Thomson, the inquiry which I had designed appears to be superfluous, as all the facts that I had observed, are but particular instances of the more general observations of Mr. Dalton—that in all cases the simple elements of bodies are disposed to unite atom to atom singly*; or, if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms."

In the foregoing paragraphs the Doctor to my great surprise, and indeed to the surprise of every honest and liberal minded man, transfers over to Mr. Dalton those principles which are so clearly developed in my Comparative View, and which he himself was obliged to allow five years afterwards, as I have already shown in this paper.

But as the Doctor supposes that his intended experiments might throw additional light on the theory of Dalton, he is determined to go on with them. He commences with the sub-carbonate of potash.

"*Experiment* 1. Sub-carbonate of potash recently prepared is one instance of an alkali having one-half the quantity of acid necessary for its saturation, as may thus be satisfactorily proved.

"Let two grains of fully saturated and well crystallized carbonate of potash be wrapped in a piece of thin paper, and passed up into an inverted tube filled with mercury, and let the gas be

* It would have been more correct to have said particle to particle singly.

extricated

extricated from it by a sufficient quantity of muriatic acid, so that the space it occupies may be marked upon the tube.

"Next let four grains of the same carbonate be exposed for a short time to a red heat, and it will be found to have parted with exactly half its gas; for the gas extricated from it in the same apparatus will be found to occupy exactly the same space, as the quantity before obtained from two grains of fully saturated carbonate*.

"A similar experiment may be made with a saturated carbonate of soda, and with the same result; for this also becomes a true semi-carbonate by being exposed for a short time to a red heat."

There can be nothing novel in those observations of Dr. Wollaston. The same may be seen in my Comparative View, pages 40 and 41. In explaining a diagram representing an atom of sulphuric acid with its two particles of oxygen united to one particle of sulphur, with numbers expressive of the force of their union, I observed that if one of the particles of oxygen were removed, the other would become more strongly united; and when the second particle was again restored, the force of union would be diminished as the quantum of attraction of the particle of sulphur would be divided equally between them.—Here follows an extract in continuation of the above explanation†.

"This seems to be a general law: all bodies unite with greater force to half the quantity of those substances to which they have an affinity than to the entire. Instance; carbonate of potash will part with a portion of its carbonic acid in a moderate degree of heat, yet it requires a very strong heat to expel the whole. In like manner crystallized sulphate of potash will part with most of its water in a heat below ignition, but it requires a strong red heat to drive away the entire of its water. Thus we find in proportion as the potash is deprived of one part of its carbonic acid, its power of retaining the remainder is increased: and the same holds good as to the expulsion of water from the salt. I shall forbear mentioning several other circumstances of the like nature."

The Doctor should at least glance at the work in which those important ideas first originated, and not attribute the principles on which they are founded to an author who cannot have the smallest pretensions to them.

It is very well known that I have done much for the antiphlogistic theory, that I have fixed it upon a more solid foundation

* It would be very difficult to hit upon the degree of heat to ascertain the products so accurately as the Doctor describes.

† See Essay on my Atomic Theory and Electrical Phænomena, page 64, or Comp. View, pages 40-41.

than

than Lavoisier himself had done; yet, as it originated with him, it belongs to him of right, and to him alone.

No person can prove that Mr. Dalton has made any novel or original addition to my Theory, except extending fancifully and hypothetically my relative weights of the ultimate particles of elementary matter, without sufficient proof to support his conjectures; at the same time that it is within the reach of accurate experimental knowledge to confirm the principles which I broached. As to the relative weights or relative quantities of matter in elementary particles, I cautiously confined myself to few instances, and those few will be found correct. They were deduced from the relative weights of simple and compound gases; and I have pointed out exceptions, even to this mode of procedure: Instance,—nitrous air is lighter than the gaseous oxide of azote, and yet the atoms of the former are heavier than those of the latter; and I have lately pointed out that the particles of azote are nearly twice the weight of those of oxygen, although an equal volume of the latter gas is specifically heavier than that of azotic gas. I attributed these differences to the distances to which their particles or atoms are removed from each other by their respective atmospheres of caloric.

The relative proportions of ultimate particles in atoms and molecules were illustrated by many examples in my *Comp. View*, which constitutes another essential part of my system. The next and the most important part of my doctrine relates to the relative forces with which ultimate particles and atoms unite to each other singly, and the modification of this law when they unite 1 and 2, or 1 and 3, or 1 and 4, &c. Were I to leave out this part, I could accomplish nothing decisive in my arduous investigation; and it enabled me to account for many phænomena and operations in chemistry which would otherwise be inexplicable.

The foregoing principles aggregately, but particularly the latter part, enabled me "to place chemistry on the same ground with that on which the discovery of the laws of gravity placed the science of astronomy."

This last link of my Theory Dalton overlooked altogether. I suppose he considered it too marked a feature to bring forward. But forward it must come, or else the Atomic Theory must remain a mere *bauble*.

In taking a cursory view, a few days ago, of the last edition (the fifth) of Dr. Thomson's System of Chemistry, I found that he transferred my Atomic Theory to Dalton, without even mentioning my name; and, what is extraordinary, adduces as an example, my proportions of azote and oxygen in the different compounds of those elements*. The Doctor also gives some ex-

* Vol. iii. p. 19.

periments

periments which were first made by me, and which helped very materially to illustrate the atomic theory or definite proportions, without the smallest reference to the author. I will adduce one, viz. the firing of oxygen and sulphuretted hydrogen by means of the electric spark, and the ascertaining of the products, &c.

In giving an history of the progress of the antiphlogistic theory and of the memorable contest which was carried on between the two sects of philosophers, he does not even glance at my *Comparative View*, which according to himself, in one of his Journals, operated so conspicuously and decisively against the arguments of my illustrious friend Kirwan. In giving an account of electrical phænomena, he passes over my hypothesis on that subject, although he adduces less probable ones of many other writers; and in his Account of Meteoric Stones, although I analysed one which fell in this kingdom; and although I advanced a new doctrine agreeable to my hypothesis of electrical phænomena, respecting the cause of their ignition, &c. he never once mentions my views. I could enumerate many more facts; but a sufficient number have been adduced to show a rooted prejudice, and a degree of glaring injustice not to be equalled in the history of any science. But the Doctor having, unfortunately to himself, commenced with his prejudices, he must persist; although we find him contradicting himself on many other occasions. A compiler of a science is an historian in that department, and he should detail his facts faithfully and impartially; he should not attempt to shove aside one experimenter, and to bring forward another of less pretensions; he should not attempt to suppress the labours of one man in order to confer them on his favourites. When a compiler deviates from those principles, he injures his readers, the science, and ultimately himself.

The Doctor, it is true, was generous enough to allow me a few facts; facts so insulated or so detached from the important objects to which they belonged in my system, that they appear singly of little or no consequence. To make use of the expression of a learned acquaintance of mine, "The Doctor extinguished your great lights, and furnished you with the feeble glimmer of a rush-light."

I am, sir,

Your obedient humble servant,

Dublin, Feb. 4, 1818. WILLIAM HIGGINS.

XXIX. *Mr.*

XXIX. *Mr.* Smith's *Geological Claims stated.*

To Mr. Tilloch.

Sir, — In the month of November last, a gentleman to whom the cause of Geological Investigation was long ago under obligation, called on *Mr. William Smith*, in Buckingham-street, and after mentioning, that some differences of opinion seemed to prevail, as to the priority of investigation and discovery, of several of the principal geological facts of England and Wales, requested Mr. Smith to show him Documents, and give him such Memorandums and References, as would enable himself, and some Friends who felt a like interest, to state the case, so fully, as would he hoped, put an end to these differences, by assigning to the earlier English Geologists, and to the followers of M. Werner and others, their respective shares of merit herein; which would leave Mr. Smith, in undisputed possession of what really might appear to belong to him.

To which request and proposal Mr. Smith so far assented, as to promise to consult some of his Friends thereon; which having done, and it appearing, that nearly all the earlier and the most explicit notices which had appeared in print, and could thus be adduced as published testimonies, regarding the principles, practice, localized results, and dates of Mr. S.'s investigations, *had been published by me*, as well as *Mineral Surveying* having been *the most widely and minutely practised by me, of any one*, I was requested by Mr. Smith, to give assistance to the gentleman alluded to, and his Friends, by looking out and arranging my various publications hereon, and in drawing up a Statement; and having assented thereto, the gentleman alluded to, and any of his friends, were requested by Mr. Smith to call on me.

I began immediately to look out the works and passages requested, and to minute down from my original Letters, Papers, Maps and Memorandums, all such particulars as I judged could assist, in preparing the Statement which was wished, as far at least as fell within the knowledge of or concerned myself or Mr. Smith; but no one calling, as was expected, I took the opportunity which presented itself on the evening of the 30th of November, of meeting the gentleman alluded to and several of his friends, and Mr. Smith and several of his friends, at Sir Joseph Banks's *Conversázione;* when by myself and Mr. S., and by more than one of his friends, the gentleman alluded to and his friends were invited and pressed, to attend a meeting next day at my House, for deliberately considering and finally adjusting such a Statement of Mr. Smith's claims, as could be borne out by undeniable testimonies.

This

This meeting accordingly took place at the hour appointed, and continued through several hours, before the Statement (in 16 Items, and with as many Notes of Reference, for which now I beg insertion in your Magazine,) was finally adjusted, as then it was, and unanimously approved by all present; but neither the gentleman who had first moved in this business, or any of his friends who have been alluded to, came, or sent any apology to this meeting; and soon after, he went out of town, and nothing since has, I believe, been heard from him or them on the subject.

Mr. Smith, after having again well considered the Statement mentioned, sent me an authentic copy of it, with Letters expressive of his entire approval and concurrence in it, and his wish for its being published; although, not then immediately, because he had been persuaded, to first ask the signature of some of the oldest, more respectable, or well known of his friends, by way of sanction to it, with the public.

It would be useless for me to occupy your pages in mentioning the various and opposite reasons, some of them, which different individuals assigned to Mr. S., as I have been told, for not complying with the wish last mentioned; although, all expressed their general approval of the Statement, and wish for its being speedily published, just as it stood. This latter being also the feeling, most unequivocally expressed, by several persons who are amongst the best judges on this subject, to whom I have since lent my authentic copy, I do not now hesitate to send you, a faithful transcript therefrom; and the insertion of which will greatly oblige, Sir,

Your obedient servant,

Howland-street, London, JOHN FAREY Sen.
Feb. 26, 1818.

Mr. WILLIAM SMITH'S *Claims (according to the Opinions of his Friends) to* merit and originality, *in regard to the Knowledge of the* British Strata, *may be briefly stated as follow: viz.*

1st. Having, while employed in the under-ground surveys of Collieries, at and near High-Littleton, in 1790, and two or three following years, acquired a more intimate acquaintance with the facts of the Stratification, *beneath the surface*, and drawn more correct inferences therefrom, as to *the necessary connection of the edges* of these subficial Strata, *with the Surface**, than were then current or known to the several coal-agents, over-lookers or working-colliers, in the vicinity; or than are even now (at more

* An explicit notice of which was published by Mr. Farey on the 31st of June 1806, in the Philosophical Magazine, vol. xxv. page 44; and with fuller details on the 7th of June 1811, in his Report to the Board of Agriculture on Derbyshire, vol. i. p. 108, &c. and on numerous other occasions.

than

than a quarter of a century of time distant) known or current, among a very large proportion of the same class of practical men, throughout all the coal districts of Great Britain*.

2d. Having while so engaged, accurately discriminated *the regular and undisturbed Strata*, with the roundish *Nodules* they frequently contain, and trata of *Sand*, from the *really worn* and heterogeneous *alluvial ruins of Strata*, which are *superficially*, and very variously scattered, *on the tops and edges of the Strata*†, but *are in no case found beneath regular Strata*; and having practically established means, *of knowing the Alluvia*, almost at first sight;—at the time, when almost all observers and writers on the subject, were confounding the alluvia with the strata.

3d. Having in the year 1795, applied the aforesaid inferences or deductions *to practice*, in actually *making a Map of the Strata in the vicinity of Bath and Bristol*, and having then freely shown and explained the same, to great numbers of persons ‡, particularly to those assembled at several public meetings of the Bath and West of England Society.

4th. Having, during the progress of making this first *Strata Map*, and in beginning very soon after, to extend this map to other parts of England, discovered a notable difference between certain English strata, as to *the visible boldness* with which *the edges* of certain of them *are presented on the surface*, compared with others; some of them forming almost continued *ranges of hills* §, where they basset; and low *flat districts*, or wide easy

* See Derbyshire Report, i. 163. Note. Mr. Farey being unable still, at this day, to add any other names to the two there mentioned. See note *, in following page.

† Mr. Farey and others, who have now acquired experience *in the practice of mineral surveying*, are ready to testify, that the Smithian rules of *discrimination, as to the Strata and the Alluvia*, were quite *fundamental ones to their progress*, in acquiring a knowledge of the British strata, and without which discrimination, no progress whatever could have been made therein.

‡ The late Rev. Joseph Townsend was among these persons; and who so highly valued what Mr. Smith had done, as to request and press Mr. Smith, for materials and permission, to publish a general account of them, and a List of the Shells and Strata (mentioned in the 8th and 11th items), in some work which he then contemplated; on which request, a correspondence took place in May to July 1801, between the Rev. B. Richardson and Mr. Smith; wherein the former persuaded Mr. Smith, to publish them himself; and to cause a Latin edition to be prepared, for more readily circulating the important novelties of Mr. S.'s discoveries, throughout Europe. In 1812 Mr. Townsend published the 1st volume of his "Character of Moses," and in the preface, handsomely acknowledges Mr. Smith's assistance in tracing the strata, &c.

§ This principle of selecting characteristic Strata, was published by Mr. Farey in February 1810, in the Phil. Mag. vol. xxxv. p. 138; and in June 1811, in Derby Report, i. 112, 113; he having previously *acted on it*, in 1807, in the construction of a great part of his large Mineral Map of Derbyshire, and its environs.

valleys

valleys being found, where several of the others come to the surface. And having then fully adopted *and practised this new principle of selection,* for choosing such of the strata, out of the great number of others, as should first have colours assigned them, and the tracing and depicting of which on his Map, should *be first attempted.*

5th. Having *made use* of certain Strata selected as above (several of which are very *unimportant* in almost any other point of view, but *their visible edges,* and had not even *received a name,* or been mentioned in previous geological writings) as the subsequent *means of mapping or filling in, between the ranges of these characteristic Strata,* as many of the less conspicuous (although perhaps more *useful*) strata*, as the scale of the map would admit; a practice quite new amongst the makers of mineral Maps, of mine or colliery Estates, then, or even now, except by those who have expressly followed Mr. Smith in this practice.

6th. Having in these early parts of his Survey of the strata of England, by that very particular attention to *the nature of the surface Soil,* and its fitness for and appropriation to particular kinds of *vegetable cultivation* or *spontaneous growth,* which his previous and early habits as a Land-surveyor and Valuer, had led and enabled him to pay to these objects, while investigating the strata beneath; succeeded, in ascertaining and establishing numerous helps to the mineral surveyor, from *the visible appearances of the Vegetable productions of a district,* towards tracing out the surfaces of its less conspicuous Strata beneath †.

7th.

* In all the numerous and wide-spread opportunities which Mr. Smith, Mr. Farey, and other of his friends have had, in seeing *the maps* which are in the possession of the mineral Owners, and their Lessees and Agents, and in those of professional Coal-viewers, &c. throughout Great Britain, not an instance has occurred, of any of these maps depicting the thick Rocks and Strata, *whose edges are conspicuous on the surface,* as the means of marking out, almost *parallel strips, within which the coals, ironstones,* thin *limestones, fire-clay,* &c. &c. are to be found ranging; although, in all the minutiæ of surveying mineral estates, these principles and proceedings of Mr. S. are equally, and even more applicable and useful, than they are in kingdom, county or district surveys. As a recent instance may here be quoted, the survey of the Leinster coal district in the South of Ireland, made by Mr. Richard Griffith junior, in 1814, and since published by the Dublin Society; wherein, *the bassets of no rocks* are attempted to be shown, within the coal-field, but merely the basset ring of one *coal-seam,* and broken parts of the bassets of three others; the ascertainment of which coal-bassets, must have been as laborious as the accurately marking out of half, or perhaps the whole of the basset-edges of the principal sandstone rocks, would have been, which interlay the coals of the whole field; and from whence, the internal structure of the same would have been incomparably more visibly and usefully shown, than at present.

† In the minuter details of the coal districts, Mr. Farey used this other principle

7th. Having by the same persevering *attention to the surface in connection with its Strata beneath it*, ascertained *the true source of the supply of all Springs* of Water, to be *the superficial water* (of rains, or streams, pools, &c.) *percolating downwards through porous Strata* or Alluvia, until intercepted by *water-tight strata*, or by Faults or patches of clayey alluvia, or by Water already stagnated, in such porous masses;—and having deduced *and applied in an extensive practice*, then commenced, these investigations and conclusions, as to the Strata and Springs, to the *Draining of Land*, wherein Mr. Smith has been employed, in most of the improving agricultural districts in the kingdom, since about the beginning of this century.

8th. Having while engaged in the earliest of the investigations above mentioned, ascertained the important fact of the *fossil Shells*, Corals and other organic remains, imbedded in the strata, not being accidentally or capriciously distributed therein, but that *each particular species* of these organic remains (when such species are with *the requisite niceties* discriminated) *has its proper and invariable place in some particular stratum or bed*, of the successive laminæ of the earth;—and having then drawn the conclusion, and verified it by an extended research, that some one or an assemblage of two or more of *these species of fossil Shells*, &c. may serve as new and *more distinctive marks, of the identity of most of the Strata* in England *, than were previously known or resorted to, by mineralogists or others.

9th. Having ascertained, *by the actual tracing* and examining of great lengths of most of the upper and middle Strata of England, that the *mineralogical characters*, as well as the more obvious or *useful qualities* of nearly all of them, *vary so considerably*, in different parts of their course and breadth of surface, as to render any *names for such strata*, or descriptions of them founded *on mere mineral characters*, very uncertain and useless †: yet probably, *no instance has occurred*, of any remarkable or useful quality of, or property attending a stratum (including its nodules and organized fossils) having occurred *in one place only*; but more commonly, such occur *in numerous places* on its range; nearly all the properties of a stratum, more or less often and

principle of selecting, mentioned p. 162 of his Report; all which he acknowledges having derived, from the instructions of Mr. Smith, in mineral surveying: see also the Phil. Mag. vol. xxxv. p. 139, and vol. xlii. p. 168.

* On the 5th of August 1807, Mr. Farey published an explicit notification of these discoveries and conclusions by Mr. Smith, as to *fossil Shells*, in the latter part of the article COAL, in Dr. Rees's Cyclopædia.

† A matter on which Mr. Farey very pointedly insisted in 1811, in the preface to, and in various other parts of his Derbyshire Report, vol. i.

suddenly appearing to decrease, and perhaps to disappear locally; to increase again to perfection, and after a greater or less length of range or breadth, again to decrease and disappear; and so on.

10th. Having ascertained, that although the strata of nearly all the south-east and east of England, have *a general and easy dip towards the south-east:* it is not, as one *flat plane* that they so dip or decline, but they are waved in a somewhat parallel manner, almost like the surface of a ploughed field: and on the great scale, are found to form *ridges and troughs**.

11th. Having, by the collection of very numerous Specimens, *actually ascertained the particular species of fossil Shells* and other organic remains, *which distinguish ten or more of the principal Strata of England;* having observed that often, where extraneous alluvia is absent, the organic remains of the Strata, show themselves in the ploughed Soil; and having *extensively used these,* in conjunction with the other new means above mentioned, of tracing and identifying the Strata, *previous to* 1799; and having in that year, made and circulated several manuscript copies of a list of such Shells, &c. † occupying a series of twenty-three of the principal Strata which are enumerated therein, in their order, from chalk downwards to coal:—*which order of the English Strata had not previously been ascertained and published* by any one ‡; or the fact known, that *London is situated* on almost *the highest of the British strata,* which in the following summer Mr. Smith ascertained.

12th. Having in the *prospectus* which Debrett printed and circulated in 1801, (and of which also great numbers were dis-

* In April 1812, Mr. Farey published a notification of this part of Mr. Smith's discoveries, in the Philosophical Magazine, vol. xxxix. p. 271, Note.

† The Rev. Benj. Richardson, of Farley; William James, Esq. of Welsbourn; and the late Rev. Jos. Townsend, of Pewsey, were among those who at first *received copies of the List,* here spoken of, which was drawn up at Mr. Townsend's house in June 1799; concerning which List, Mr. Farey published a notice on the 31st of May 1815, in the Phil. Mag. vol. xlv. p. 334; and in August of the same year, Mr. Smith published a copy of this original List, in the "Memoir," which accompanied his Map, facing p.8.—The late William Reynold, Esq. of Coalbrook Dale, Mr. Thomas Bartley, of Bristol, and others (see Phil. Mag. vol. xxxviii. p. 338, Note) received copies of this List, at second hand, soon after 1799.

‡ In confirmation of which, all the numerous mineralogical and geological works, learned transactions, periodical publications, or other works, prior to 1811 (when Mr. Farey's Derbyshire Report, vol. i. appeared) may in vain be searched, for any such knowledge; or, for the clear and *explicit laying down and acting on, or using for the purposes of geological investigation or theory,* any of the important discoveries and deductions, which are herein claimed for Mr. Smith.

tributed

tributed, while Mr. Smith was soliciting *the names* of subscribers for the publication of his Map and Memoir *), set forth very fully, what were the objects and advantages to various classes of the community, as well as to science, which would result from the diffusion of the knowledge regarding the Strata which had then recently been acquired.

13th. Having, from the first commencement of his tracing and mapping the British strata, in the most free and unreserved manner *communicated*, to all the various *mine*, *colliery*, or *quarry Owners*, *Agents*, *Workmen*, &c. with whom he conversed on the spots, almost throughout England and Wales, and to *scientific Men* and others, in general †, whatever they wished to ask, regarding *the principles and process*, on which his investigations had been commenced and carried on, to the state in which his *Map*, *Sections* and *Collection*, were then *shown to them*; and as to *the general conclusions*, of every kind, which he had drawn therefrom; and to *Mr. John Farey* in particular, the agent at Woburn, for the late Duke of Bedford's estates, at his Grace's particular request (made before Mr. Farey had ever heard the name of Mr. Smith mentioned) a full and particular course of instructions were given, in *Mineral Surveying*, by Mr. Smith ‡; at the time of revising his Map of the adjacent parts of Bedfordshire and Buckinghamshire: such as, joined with his own previous acquirements and industry has enabled Mr. Farey, since the decease of his Grace, extensively *to practise Mineral Surveying* in new situations, almost throughout Great Britain.

14th. Having, at very considerable trouble and expense, brought together and arranged, *a numerous Collection of Specimens of the several English and Welsh Strata*, from *numerous and distant Places* (all of which were marked) on the range of

* On the 1st of July 1801, the editor of the Monthly Magazine, in vol. xi. p. 525, published a distinct notice of this prospectus.

† For several years after 1800, Mr. Smith made a point of attending nearly all the public Agricultural Exhibitions of the Bath Society; Mr. Coke, at Holkham; the Duke of Bedford, at Woburn; and of Lord Somerville, and the Smithfield Club, in London, *and there publicly hung up and showed his Map of the Strata*, to many hundreds of intelligent persons: which fact has very often been recorded in the newspaper accounts of the proceedings of these meetings; see the Star of the 21st of June 1804, p. 4: Phil. Mag. vol. xxxv. p. 114, &c.

‡ To whom Mr. Farey made this acknowledgement in the most handsome terms, in Feb. 1810, in the Philos. Magazine, vol. xxxv. p. 114; also in a paper read to the Royal Society on 21st March 1811. Phil. Trans. 1812; and in June 1811, in his Derbyshire Report, vol. i. p. 110; and on very numerous other occasions, in the last twenty-five volumes of the Philosophical Magazine, and in several other works, Mr. F. has also stood forward, zealously, and with effect, to assert Mr. Smith's claims, as herein set forth.

 each

each stratum, as shown in his Map; and particularly of the *organic remains*, found imbedded in each of these several Strata. Having rented and kept Rooms for the express purpose of displaying these Specimens, *in the real order and succession in which they occur in the Earth*, and given the freest access to them *gratis*, and to his *map of the strata and sections*, by all persons who applied; as great numbers did, and occupied much of Mr. Smith's time in thus explaining them.

At first, this Collection was shown in Trim-street, Bath, through several years; next in Charing-cross-street, London; and since 1804, for many subsequent years, in Buckingham-street, Strand, at Mr. Smith's present residence, until this Collection was in June 1816, removed to the British Museum, and there arranged by Mr. Smith, in a similar manner and order, for the free use of the public.

15th. Having, in August 1815, published (at Mr. John Cary's, 181, Strand) his large coloured *Map of the Strata*, in fifteen sheets, on a scale of five miles to an inch; and a "Memoir," of fifty-one quarto pages which accompanied it.

16th. Having since published (at ditto) "A Geological *Table of British organized Fossils*," &c. containing a great many useful and interesting particulars, on a single sheet.

Also (at ditto) "A *Geological Section*, from London to Snowdon," on a long sheet.

And (at Mr. Evan Williams's, No. 11, in the Strand), one out of the two intended parts of the "*Stratigraphical System*," in which more than 700 *species of Shells* and other organic remains, which Mr. S. has *arranged in the British Museum*, are each to be *named*, and scientifically *described*; with references to the precise different *Places*, at which the several individuals of each species were dug*, and the particular *Stratum* (with reference to the Map and Section) which it there occupied.

Also (at Mr. James Sowerby's, No. 2, Mead-place, Lambeth) three out of the seven intended numbers of "*British Strata identified*, by their imbedded *Organic Remains*," in which *drawings* are given, and the *names* of all the most *characteristic Shells, Corals, &c. of each Stratum.*

London, Dec. 1, 1817.

* These Places, as enumerated in this first part, are 263 in number; and the number of the individual Shells, &c. 1155; of which an alphabetical list is given in the Philosophical Magazine, vol. L. p. 271.

XXX. *On*

XXX. *On the Question "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?"* *By* HENRY UPINGTON, *Esq.*

[Continued from p. 43.]

To Mr. Tilloch.

Blair's Hill, Cork, Feb. 18, 1818.

SIR, — I HAVE read in your January Magazine my last letter of the 17th December, and am really apprehensive that so long an *exclusive* dissertation upon music will appear to many of your readers an unnecessary procrastination of my subject. I acknowledge the delay; and however interesting the musical question may in *itself* appear, I am determined to conclude it within this present paper, and confine myself, for the future, to its more immediate application to my theme.

Ere I commence with the formation of that *table* which I promised you, I must recall your attention to the mode of calculation by which I suggested that it should be governed; viz. by the creation of *means*, and the subsequent division of those means into the sums produced by the multiplication of the *extremes*—in justification of which procedure I advanced, as you will recollect, to the minor 6th, pointing out its origin from the fourth and octave; the fifth having previously proceeded from the conjoint operation of the base, the fourth, and octave,—while the fourth, in its turn, was generated by the base and octave alone.

Although this retrospect may possibly serve as a competent introduction to the table; yet I would previously state in defence of the *fourth*, one important peculiarity which elevates it above all other numbers, and justly stamps it (at least in my estimation) with that superiority of character insisted by the Greeks. Nature then, throughout all her known operations, has uniformly assigned as the common centre between any two bodies of different magnitude, some certain situation, more contiguous to the greater body than to the smaller: and that this universal principle is equally applicable to that branch of acoustics called Music, as to every other branch of natural philosophy, is evident, from the indispensable construction of all our fixed-toned instruments. Thus, with our piano-fortes the interval between any fundamental and its 4th, is occupied by only *four* keys, while that between the 4th and the octave of that fundamental is occupied by *six*. The *fourth*, therefore, most avowedly, and not the fifth, which would reverse this order of nature, must constitute the musical centre, and consequently the only perfect concord.

TABLE OF ANALYSIS.

Base, for convenience, counted as 12.

Mode of Creation.	*Order on Scale as created.*								
	Base.	*3d Minor.*	*3d Major.*	*Fourth.*	*4th Major.*	*Fifth.*	*6th Minor.*	*6th Major.*	*Octave.*
Originals	12								6
Fourth = 9, *i. e.* $\frac{9}{12}$ or $\frac{3}{4}$ string. *Operation.* Base = 12 } Mean 9. Now Octave = 6 } In ratio as 1 to 2.	12			9					6
Fifth = 8 or $\frac{2}{3}$ Base = 12 × by Octave = 6 ÷ by 4th = 9) 72 (8 ...... Now	12			9		8			6
6th Minor = $7\frac{1}{2}$ or $\frac{5}{8}$ Octave = 6 } Mean $7\frac{1}{2}$. Now Fourth = 9 } Ratio 2 to 3.	12			9		8	$7\frac{1}{2}$		6
3d Minor = 10 or $\frac{5}{6}$ Base = 12 } Mean 10. Now Fifth = 8 } Ratio 2 to 3.	12	10		9		8	$7\frac{1}{2}$		6
3d Major = $9\frac{3}{5}$ or $\frac{4}{5}$ Base = 12 × by Octave = 6 ÷ by 6th Minor = $7\frac{1}{2}$) 72 ($9\frac{3}{5}$. Now	12	10	$9\frac{3}{5}$	9		8	$7\frac{1}{2}$		6
6th Major = $7\frac{1}{5}$ or $\frac{3}{5}$ Base = 12 × by Octave = 6 ÷ by 3d Minor = 10) 72 ($7\frac{1}{5}$. Now	12	10	$9\frac{3}{5}$	9		8	$7\frac{1}{2}$	$7\frac{1}{5}$	6
4th Major = $8\frac{4}{7}$ or $\frac{5}{7}$ Base = 12 × by Octave = 6 ÷ by a new mean* = $8\frac{2}{5}$)72($8\frac{4}{7}$. Now	12	10	$9\frac{3}{5}$	9	$8\frac{4}{7}$	8	$7\frac{1}{2}$	$7\frac{1}{5}$	6

* This mean $8\frac{2}{5}$, which is not on our scale as an interval, is constituted by
3d Major = $9\frac{3}{5}$ } Mean $8\frac{2}{5}$
and 6th Major = $7\frac{1}{5}$ }
Ratio as 3 to 4.

*** The linked numbers *alone* are employed for the creation of means.

Although the major 4th is certainly inferior to the preceding numbers called concords; yet, if this mode of calculation be just, it is by far superior to the proper discords, as will appear by the succeeding analysis.

ANALYSIS OF DISCORDS—(*Base, as before counted,* 12.)

7th Minor $= 6\frac{2}{3}$ *i.e.* $\frac{6\frac{2}{3}}{12}$ or $\frac{5}{9}$ —I shall call it $6\frac{3}{4}$, for I am inclined to consider $6\frac{2}{3}$ an *aberration,* the latter number not being divisible into the extremes, so as to produce any acknowledged interval.

Generated by
Octave $= 6$
and 6th Minor $= 7\frac{1}{2}$ } Mean $6\frac{3}{4}$
In ratio as 4 to 5.

Second $= 10\frac{2}{3}$ . . . . . or $\frac{8}{9}$ (properly called Tone major).
Base $= 12$
× by Octave $= 6$
÷ by 7th Minor $= 6\frac{3}{4}$)$\overline{72}$($10\frac{2}{3}$.

Semitone $= 11\frac{1}{4}$ . . . . or $\frac{15}{16}$
Base $= 12$
and New Number* $= 10\frac{1}{2}$ } Mean $11\frac{1}{4}$
Ratio 7 to 8.

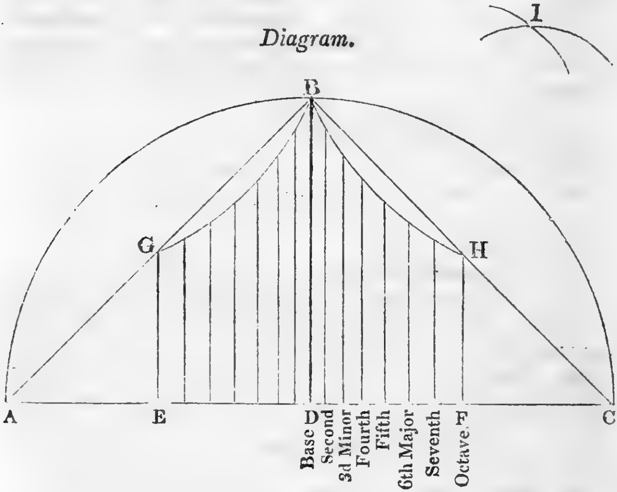
* $10\frac{1}{2}$ is generated
by Base $= 12$
and Fourth $= 9$ } Mean $10\frac{1}{2}$

7th Major $= 6\frac{2}{5}$ . . . . or $\frac{8}{15}$
Base $= 12$
× by Octave $= 6$
÷ by Semitone $= 11\frac{1}{4}$)$\overline{72}$($6\frac{2}{5}$.

Such is the result of those uniform operations which I have adopted in the analysis of our musical numbers; and by those operations may be instantly discovered the superiority of the intervals called concords;—all the discordant intervals, commencing with the major 4th, being generated by *means,* whose originating extremes have descended from the ratio of 3 to 4, to that of 7 to 8. Intervals so basely constituted as the latter can lay no claim whatsoever to proportion. Even our 3ds and 6ths, as originating from extremes in the ratio of 2 to 3, can merit no other than the ancient appellation of *discords,* when compared with the more perfect 5th, and its still purer generator the 4th.

A subject perhaps still more desirable than all our calculations comes next in order, viz. the construction of a musical *diagram,* the relative length of whose lines or strings shall not only accord with our generally received proportions, but at the same time shall indicate, for our guidance, the seven *original* intervals. Whether this or any other suitable diagram has been hitherto constructed I know not, nor is it of importance: facts, not originality, are my object;—should it lead to any discovery for the judicious subdivision of our scale, the musical portion of society will be amply remunerated for the trouble of perusing it.

Diagram.



LENGTHS OF THE STRINGS.

Modern Concords.

Base = 1
Octave = $\frac{1}{2}$
Fourth = $\frac{3}{4}$ minus, a minute fraction.
Fifth = $\frac{2}{3}$ plus, the same.
3d Minor = $\frac{5}{6}$ minus, a somewhat greater fraction.
6th Major = $\frac{3}{5}$ plus, the same.

Discords.

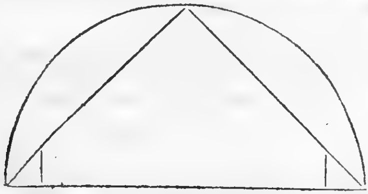
2d and 7th out of all seeming proportion.

Construction of the Diagram.

On the line AC describe a semicircle. From the centre D erect the perpendicular DB; and from the point B draw the lines BA and BC. Bisect AD in E, and DC in F; and from the points E and F erect the perpendiculars EG and FH. Then with the radius BD, and from the points B and H, describe two arcs intersecting in I. With I as centre, and BD as interval, describe

describe an arch which will pass through B and H. Let a similar arch passing through B and G be also described. Then if we divide the arches BG and BH into seven equal parts*, and let fall perpendiculars upon the line AC, we shall not only discover in the length of every line or string, what should properly be termed the "natural scale," but also the probable origin of the Grecian "disdiapason," the line HG as well as the line FH being equal to one-fourth of the original line AC.

All music then is contained within the *isosceles* right-angled triangle ABC; and in proportion as we extend the edifice or system beyond the boundary of the disdiapason, as thus



so shall we deviate more and more widely from that beautiful and perfect order which the great Architect of nature appears to have established.

You will now permit me to offer some remarks on that passage of my former paper which glanced at the proportion of our common chords;—the accusation of "heterogeneous mixture" applied by some persons to those chords, as I mentioned in that passage, not having received my altogether *unqualified* assent.

That certain proportions do actually exist within these chords is undeniable; but that these proportions do not approximate, in the remotest degree, to that superior species called "Harmonic," must be confessed. Simple arithmetical progression is indeed obvious in the chord *minor* (the natural third $=\frac{5}{6}$, not the tempered third $=\frac{17}{20}$ being employed): thus

Base, suppose		60	Progression 3. 4. 5. 6.
3d Minor $=\frac{5}{6}$	..	50	
Fifth $=\frac{2}{3}$	..	40	
Octave $=\frac{1}{2}$	..	30	

But in our chord major even *this* species of proportion is destroyed; viz.

Base	..	.. ..	60
3d Major $=\frac{4}{5}$	..	..	48
Fifth =	..	..	40
Octave =	..	..	30

* A division into 45 equal parts, delineated on a very extensive scale, will afford much gratification to the speculative musician.

Thus

Thus may be seen that the extent of modern attainment is only an approximation towards arithmetical progression; and if the basis of our system (for such I consider the common chords) can lay no higher claim to perfection, then must our harmony be very low indeed. *Harmonic* proportion requires, that of four numbers, the first number shall bear the same ratio to the fourth, which the difference between the first and second number shall bear to the difference between the third and fourth. If harmony, then, in the true sense of the word, be at all attainable, let us judiciously subdivide the scale for the purpose: but, on the contrary, if it be wholly *un*attainable, let us discontinue the torturing of nature by the perpetual violation of her proportions.

[To be continued.]

XXXI. *On the Nautical Almanac.*

To Mr. Tilloch.

Sir, — In the last number of your valuable Magazine (p. 146) a correspondent, under the assumed title of Manchestriensis has thought fit to reply to the observations which I lately made on the gross and numerous errors in the Nautical Almanac: and he is pleased to say, with no small degree of pride and satisfaction, that he is able to refute and contradict my assertions "with the utmost confidence." Notwithstanding the *modest* tone of this sentence, and the magisterial air which the writer assumes, I can only state in reply that, as to the occultation of Mars on Jan. 1, 1820, I have carefully revised my calculations on that subject, and still find (in spite of what Manchestriensis may say) that there *will* be an occultation on that day: and in this, I am partly borne out by the *Connaissance des Tems*, which states that it will be visible also at Paris. The longitude and height of the *Nonagesimal*, and the quantity of the *Parallax* both in longitude and latitude, I have deduced, not from any tables but, from a trigonometrical calculation, on the assumption that the spheroidal figure of the earth is 299: 300.

I have had recourse likewise to the same method, for determining the time of the commencement of the great solar eclipse on Sept. 7, 1820: which (if there be any truth in the formulæ given by all the writers on this subject) will certainly commence at Greenwich at $0^h\ 23'\ 10''$; and the moon will make the first impression on the sun's disc at $48°\ 22'$ from the vertex. If therefore these values differ from those given by Manchestriensis, it must arise from our having made use of different methods: but I am at a loss to account for the bold assertion which he has made that it is not of any consequence whatever to notice the point

point on the sun's disc where the moon makes its first impression: an assertion which evidently shows he has not been in the habit of observing such phænomena.

With respect to the errors in the configurations of Jupiter's satellites, MANCHESTRIENSIS might easily have satisfied himself on that point, had he taken the trouble (no very arduous task) of computing their positions.

But, sir, I have now done with this subject, I hope *for ever.* The frequent and glaring errors in the Nautical Almanac have at length attracted the notice of government; and his majesty's ministers have consequently brought in a bill to amend and reform that work. Mr. Croker, in his speech upon this occasion (on the 6th instant) pays a handsome and deserved compliment to the *late* Astronomer Royal, Dr. Maskelyne, by observing that "he had looked into the whole of the almanacs from the earliest period, and found only two or three errata in any one volume." At the same time he added, "The later numbers however are *very incorrect,* and he was very sorry to be obliged to say that the numbers for the present year did not contain less than *eighteen gross errors;* and the publication for the next year not less than *forty:* in fact, *the Nautical Almanac was a bye-word among the literati of Europe.*"

Before I close this letter, permit me to suggest, through the means of your valuable miscellany, some hints for the improvement of such almanacs as may be in future published by the new commissioners of the board of longitude. In the first place, it ought to be expressly stated in the preface, what *tables* are made use of in the computations, in order that any person may satisfy himself that the calculations are true. The name of the *person* likewise who makes the computations ought always to be stated in the preface: this would not only tend to make the computer more anxious to preserve his scientific character, but would likewise show where the blame (if any) ought to attach. This method is adopted in the *Connaissance des Tems:* but in the ephemerides published at Berlin, Vienna, Coimbra, &c. it is unnecessary, as they are under the known direction of the several authors.

In computing the moon's place from the Nautical Almanac, for any intermediate time, a constant reference is made to Mr. Taylor's sexagesimal tables, a quarto book consisting of upwards of 300 pages: but the *only part* which is wanted for that purpose is contained in *four pages* of that work. Why cannot that portion of the work be reprinted in three or four octavo pages and added as an appendix to the Nautical Almanac? This method is adopted (though in *too abridged* a form) in the *Connaissance des Tems* for 1820, p. 164. The mode however adopted by M. Bode in his ephemeris would be more convenient for practice, although it might give the computer more trouble; which

is,

is, to insert, in contiguous columns to the moon's longitude and latitude, her *horary motions* in each course.

A *correct* list of the *longitudes* and *latitudes* of all the zodiacal stars, down to those of the seventh magnitude, ought to be given in *each* number of the Nautical Almanac, in order that persons may frequently look out for occultations, by the moon, of such stars as lie in her path. A list of this kind was given in the Nautical Almanac for 1773, but has never been continued.

Many other useful tables might likewise be introduced, without much additional expense to the work, as will readily suggest themselves to those persons who have the conducting of it: and many improvements might be made in the arrangement of the different articles. For instance, the configurations and eclipses of Jupiter's satellites ought to present themselves at the *same opening of the book*, as is done in the *Connaissance des Tems* and in M. Bode's ephemeris, in order to avoid a reference from one part to the other. A table of the state of the tides might be introduced, as in the *Connaissance des Tems:* the phases of Venus and the position of Saturn's ring at different periods might likewise be given, in the same manner as by M. Bode in his ephemeris, by means of wood-cuts. The same mode might likewise be adopted for showing the phases of eclipses, and the occultations of some of the principal fixed stars. The columns of the monthly phænomena should be enlarged and thrown together at the end, similar to the plan adopted in the *Connaissance des Tems* and by M. Bode: and *every visible occultation* of the stars noted down, in order that persons may look out for them. The appendix should be revised, and such parts excluded as are not adapted to the state of science at the present day: such as the recommendation of *twenty-feet* telescopes as the *most proper* for observing Jupiter's satellites.

The rising and setting of the sun, moon, and planets, for the different days in the year, should be inserted, as in the *Connaissance des Tems:* for although the Almanac is intended for general use, and adapted to all parts of the world, yet it is more used in *this* country than in any other; and many persons (even of an astronomical turn) are obliged to buy *other* almanacs to obtain that information.

Several other improvements and alterations may probably occur to some of your readers; and they would be rendering an essential service to the science of astronomy if they would publicly suggest them, for the consideration of the new board of longitude: who will, no doubt, attend to any hints which may tend to make the Nautical Almanac more generally useful than it has, of late years, hitherto been.

I am, sir, your obedient servant,

March 10, 1818. ASTRONOMICUS.

P.S.—Would

P.S.—Would it not be desirable to publish separately an annual *Appendix* to the Nautical Almanac, consisting of scientific information and observations relative to astronomy; similar to the plan adopted in the *Connaissance des Tems* and by M. Bode, in his Ephemeris? And might not such a plan be adopted *immediately*, so as to correct and improve the existing almanacs of 1818, 1819, and 1820?

XXXII. *Meteorological Retrospect for the last Half of the Year* 1817*.

Storms and Hurricanes.

Few years have been more distinguished for an extraordinary frequency of violent storms than the last. In the year 824, when, if we may believe the annals of that period, a hailstone sixteen feet in length fell upon the city of Autun;—in those of 1680, 1720, 1739, and 1740, when there were storms of hail of one foot in thickness; in 1767, when Potsdam was devastated by hailstones of the size of an ordinary gourd; in 1771, when the environs of Namur were ravaged by others of nearly eight pounds weight; in 1788 and 1812, which were also remarkable for their storms, and the congelations which accompanied them;—there was still nothing in point of extent of suffering to compare with 1817.

The city of Rheims will long remember the 19th of May. After having experienced on the day preceding an extraordinary and stifling heat, about half past one in the morning there appeared in the heavens an igneous meteor, the red light of which, reflected from all the houses, gave to this ancient Gallic city the semblance of a town involved in one vast conflagration; some strokes of-thunder were followed with rain, which fell in extraordinary abundance for two hours; soon after, a large black cloud gathered over the city and burst upon it with a horrible crash. For five minutes the hail fell in torrents; whole roofs were broken; the trees of the gardens hashed, and some animals killed. The neighbouring country however did not suffer any thing. The same day the hail ravaged with equal severity many communes of the department of the Upper Garonne; and on the following day Semur (*Côte-d'Or*) and the rich vineyards in its environs were visited by another frightful storm, in which the rain and hail fell for a whole hour in one continued flood.

The month of June was especially remarkable for the number and severity of its storms. On the 7th, a part of the communes of Courcon, Beangas, Moulinet, and Bondi, in the arrondissement

* From *Bibliothèque Physico-Economique.*

of Villeneuve (Lot and Garonne) was laid entirely waste; not a stalk of corn was to be seen standing, nor a leaf remaining on a vine in those places which the hail attacked: a heavy rain which fell on the night of that day did still more harm, the quantity of earth which it unsettled being so great as to cover all the meadows with sand. The same day a violent storm assailed the canton of Zurich in Switzerland, the city of Pau (Lower Pyrenees), and some surrounding communes. Some hail fell of such a size that roofs were broken and animals killed. On the 8th, fourteen communes situated in the valley of the Loire, and on the 9th, twenty-seven in the arrondissement of Ambert, were nearly inundated by the quantity of rain which fell, accompanied with large hail. On the 10th, a storm of such severity swept over the canton of Saint Gall, in Switzerland, that a great number of houses were thrown down at Wittenbach, Berg, Horn, and Ober-Steinbach. On the 12th, the environs of Casan (Russia) were devastated; the ravages of the storm fell particularly upon the village of Oura, inhabited by Tartars living in a state of ease, and famous for their fabrics of red-coloured cotton; the rivulet which traverses this village formed all of a sudden an immense torrent, carrying along with it men, trees and habitations, to the distance of twenty versts. On the 14th, another storm still more horrible desolated Belgium: the thunder raged for three-quarters of an hour without intermission; the storm driven by a south-east wind was so violent, that it tore up a number of large trees, overturned many granaries and some houses, and shook all the houses for the space of a league. On the 15th, a shower of hailstones fell upon the town of Lierre in the Low Countries, most of which were of the bulk of a pigeon's egg. The 22d, the 26th, the 27th, and 29th were also distinguished by violent storms which committed great havock.

In the same month the heat was more excessive in England than it had been for several years, and brought on storms which did every where a great deal of damage. At Tewkesbury they were accompanied with large hail; at Salisbury there was one attended with an extraordinary obscurity, and followed by torrents of rain and large pieces of ice, the ravages of which were frightful—trees shattered—men and beasts wounded—houses overturned, &c.

On the 3d of July a storm of the greatest violence, mixed with large hail, burst in the night-time upon the town of Agen and many communes of the department of Tarn. It continued till ten o'clock the next day, which was also distinguished by another tempest, which carried ruin and devastation into the valley between the two mountains of Lure and of Leberon (the mouths of the Rhone) over a space of more than fifteen leagues. On the

4th,

4th, hail of the size of filberts fell at Munich and Lons Le Saulnier; and on the 10th, many leagues in the departments of the Yonne and Ain were in less than an hour laid entirely waste by another storm of hail as large as pigeon's eggs, and precipitated with astonishing impetuosity. The 11th was marked by a storm of still greater fury. At Pforzheim in the duchy of Baden, and on the frontiers of Hungary and Lower Austria, hail-stones were collected of the bulk of the largest hen's eggs; several men and beasts were killed, and the hopes of a fine harvest wholly destroyed. The night of the same day was most ruinous to the cantons of Chateauneuf and Eymoutiers in the department of Upper Vienne. The hail was of such force that even the chesnut-trees were destroyed, and in such abundance that two days afterwards it was found in heaps upon the ground. On the 31st, there fell at Manchester, in England, and its environs, hail of such an extraordinary bulk that two persons were killed by it at Pendleton, and several others grievously wounded.

On the 8th of August, a thunder-storm burst on the town of St. Avold (Moselle), and caused a fire which consumed thirty-three houses and thirty-eight barns*. The 16th was a day cruelly memorable to the departments of the Aisne and Ardennes. The reapers were occupied in collecting one of the finest harvests which had been known for a long time; the heavens became suddenly obscured by thick and heavy clouds; and soon a storm of hail burst forth of such impetuous force, that in ten minutes the crops and fruits of every description in the territory of four villages were hashed in pieces. Some of the hail-stones found were *three pounds* in weight. These congelations resembled a bullet cut in two; the centre of each hemisphere was harder than the rest, and of a brownish colour. On the 22d, after two months of excessive dryness, Rome was the scene of another dreadful tempest: some vineyards were quite ruined, and more than thirty of the largest trees of the villa Panfili were torn up by the roots. On the 26th, there was a hurricane at the estate of Gourgivaux near Epernay, which, though it only lasted three minutes, tore up and shattered a number of trees, carried off several roofs, knocked the barn of a farm topsy-turvy, and scattered to the winds 300 well-bound sheaves. On the 27th, in the valley of Pia near Genoa, there was a similar hurricane, but of a longer duration; the damage occasioned by

* A popular error augments the evils occasioned by such storms at many places. When assistance should be run for, the women discourage the men with the greatest earnestness, saying, that *when the fire of Heaven descends, it is in vain to seek for relief*; *and that water, far from allaying it, will only increase its force and activity.*

which

which it will take many years of prosperity to repair;—vines, trees of every kind, even garden walls, fell prostrate before it.

The 3d of September, at Liverpool; the 11th, at Paris; the 12th, at Antwerp, Brussels, and several other places in the Low Countries;—the 22d at Schaffhausen, &c. and the 28th at Memel, were distinguished by violent and destructive storms, in most of which the size and quantity of the hail was still the chiefly remarkable circumstance.

In the month of October, the place which suffered most from the elements was the old town of Nocera, at the foot of the Appenines. For the third time in the course of five months, it was visited on the 4th by a hail-storm of such tremendous violence that all that had been spared by the previous tempests,—its superb olives, its fruit-trees, and its vines—were completely destroyed. A number of cattle were killed, owing chiefly, perhaps, to the very angular shape of the hailstones in this instance, the largest of which were found to weigh about six ounces. The other places visited by remarkable storms during this month, were the communes of Mesmes-sur-Yevre, Vasselay, and others in the department of Cher, on the 1st;—the environs of Cahors on the 3d;—Foligno, Assisi, and Perugia on the same day as Nocera;—and Alicant on the 13th. In one quarter of an hour this last town and its environs, which produced a great abundance of exquisite fruits and an excellent wine, presented the spectacle of one great wreck*.

Inundations.

Other misfortunes not less disastrous signalized the period under our review. The inundations of rivers and lakes desolated almost all the countries of Europe, particularly Switzerland, the west of Germany, the Low Countries, Holland, the north of Spain; and in the United States, the two provinces of Kentucky and New-

* For near half a century the people in the Maconnais (Saone et Loire) have been in the custom, for averting damage by hail, of firing mortars from the heights at the approach of storms. The first who introduced this scheme was M. de Chevriers, an old officer of marine, proprietor of Vaurenard. The experience of many years having convinced the inhabitants of the neighbouring country of the excellence of this practice; it has been adopted by the communes of Iger, Aze, Romaneche, Julnat, Le Torrins, Ponilly, Fleury, Saint Sorlin, Viviers, and many others, which have ever since been exempt from any ravages by hail. The size of the mortars, and the number of times they are fired, varies according to circumstances and localities. The commune of Fleury makes use of a mortar which carries a charge of one pound of powder at a time. It is ordinarily begun to be fired before the clouds have had time to accumulate in any great number, and the firing is kept up until the stormy clouds are wholly dispersed. The annual consumption for this purpose is from 4 to 500 killogrammes (820 to 1022lbs.)

New-York. In the first days of June, the Rhone tumultuously burst its banks, at the same moment that the waters of the Rhine and the Aar attained a prodigious height*;—that the lakes, the rivers and the torrents of Switzerland, the Grand Lake of Constance, the Necker, the Mein, the Meuse, the Wahl, &c. overflowed upon all points. The detail of the disasters which they caused is fearful. During three months their waters covered whole countries, menaced the foundations of the most solid edifices, and scarcely left in some places the roofs of the houses to be seen, while they kept constantly sweeping away trees and flocks, and a vast wreck of things of all sorts. Fields cultivated with the greatest care were converted into morasses; large tracts were turned into deserts of mire; the finest harvests were every where destroyed. On the 26th, 27th, and 28th of August, a south wind which had prevailed for more than a month was followed by a hot rain, which melted the glaciers in such a manner that the Rhine rose anew beyond all former example, and presented until the 23d of September the appearance of a vast lake: the torrents of the Tyrol were swollen higher than their greatest height in 1789; and the Sill, which falls into the Inn near Inspruck, burst its banks and carried away several bridges, with a vast quantity of trees, houses, cattle, &c. On the 9th of November, a very violent storm burst upon the department of Ardeche, the waters rose to a prodigious height, and committed great havoc in the arrondissements of Tournon, Privas, and Argentiere.

Earthquakes.

June 30th. After a storm accompanied with a hot rain two shocks very violent were felt at Inverness and in the environs of Loch Ness in Scotland.

1022 lbs.) of coarse powder. This practice, which costs little, which is attended with no inconvenience, which is so simple in execution as to be practicable every where, and which is supported not only by theory, but by the experience of a great many years, ought to be generally substituted for the ruinous system of conjuring storms by the sound of the church bells. The misfortunes which every year befall those who have recourse to the clocks, may in the end destroy a prejudice which originated in an æra when the laws of physics were unknown; and when fanaticism attributed to the sound of the bells a supreme power, in virtue of the benedictions and unctions which they received from the hands of the priests. At the same time that the villages of Maconnais adopted the practice of firing mortars, Guenaut-de-Montbelliard, the celebrated co-operator and friend of Buffon, having observed that the hail never formed itself till after violent claps of thunder, proposed to withdraw the electric matter, so as to prevent at the same time both the explosion of the thunder and the formation of the hail (*Journal de Physique*, tom. xxi. p. 146). Guyton-de-Morveau has further demonstrated the accuracy of this theory (*Journal de Physique*, tom. ix. p. 60-67).

* The Rhine rose on the 7th of July, thirty-two centimetres (one foot) above its greatest height in 1770.

July 4. At Barcelona.

7. Porentruy and Schaffhausen.

Aug. 11. } 13. } Saanen, canton of Bern.

14. Rougemont, and the valleys of Gessenay and Senimenthal in Switzerland.

Aug. 19. Inspruck.

Sept. 12. Saanen.

17. Inverness—the fifth since August 1816.

21. St. Helena. The oscillation lasted two minutes, and was felt throughout the whole island and neighbouring sea; also at Saanen, Rothenberg, and environs of Rublihorn.

Sept. 22. Angouleme (Charente-Inferieure), followed immediately by a loud detonation.

Oct. 17. Pays de Vaud, particularly at Yvonaud and its environs.

Oct. 18. Messina.

23. Vostizza in the Morea:—The most violent that has occurred this year. It lasted about a minute and a half. The sea was thrown back to a great distance, so that the ships in the roads of Vostizza were left quite dry: it immediately returned with great fury, rose five metres above its ordinary level, and inundated a considerable space of ground: soon afterwards it subsided into its original position. But the cape which formed the mouth of the river Gaidouroupneiti, after ejecting a very thick smoke, precipitated itself into the sea, and carried along with it the town of Vostizza, the villages of Mourla Dimitropoulu, Loumari, Temeni, and part of their inhabitants. For eight succeeding days shocks less strong, but very frequent, continued to be felt.

Nov. 11 and 12. At Geneva, and the two sides of the lake, the shocks were stronger than were ever experienced in this quarter before.

Drought.

In the early part of this year the south of Europe was almost desolated by a severe drought, which still continued in a manner truly distressing. In June it dried up the lake of Ouveillan in the arrondissement of Narbonne, and drained the fountains and wells in the greater part of the departments of the mouths of the Rhone, the Var, and the Lower Alps. In July, such was its intensity in the department of the Eastern Pyrenees, that it converted into salt a great part of the waters of the lakes of Saint Nazaire and Villeneuve. At Marseilles and Montpelier the greatest inconvenience was also felt for the want of water.

Great Heats.

We have had daily the most remarkable heats. On the 7th of June the thermometer at Paris rose to 26° centigr. where it remained the whole day. On the 18th it was at 28°, and on the 20th at 30°. In some parts of Great Britain it rose still higher. At London on the 28th, between three and five o'clock in the afternoon it was 39° centigr. being 10° above the greatest heats of ordinary summers. In the north of Asia, on the contrary, there was scarcely any summer at all this year, the cold continuing until the 21st of June, the time at which the fine season in the northern parts of Siberia usually terminates. In the hyperborean regions of Europe, again, the heat was so intense that the coasts of Greenland, which had been covered for ages with enormous masses of ice, were completely liberated, and the sea was laid open as far as the mountains of Spitsbergen, and even as high as the 84° of latitude. Enormous masses of ice descended into the Atlantic sea as far as the 40° of latitude without melting.

The months of June, July, August and September were of a stifling heat, especially at Rome, at Naples, and at Trieste, where it was impossible to go abroad till evening. The warmest day at Perpignan was the 4th of July; at Marseilles, the 17th of August, when the thermometer exposed to the sun remained stationary at 44°. At Cayenne, winter, which is the rainy season in that country, was unknown; it ordinarily lasts six months complete, but last year there were only sixty-two days of rain, and that slight and intermittent.

Untimely Colds.

After long intervals of heat, of abundant rains, and wasting storms, we were visited on the 23d of August with squalls of cold rain, and weather truly autumnal. The atmosphere was wholly changed. The equinoctial winds raged with violence; at Paris they tore up the stoutest trees by the roots. On the 23d of September, the weather was mild, and of a temperature rather more elevated than suited the period of the year; but next day a strong wind arose from the north-east, which dried up the earth and gave all the chill of winter to the atmosphere. On the 10th of October, the Parisians felt as if in the middle of January. The like unseasonable cold was felt in the south. From the climate of Africa to that of Lapland was a common transition. After more than ten months without rain, and a heat the most ardent, they were obliged on the 15th of October to have recourse to fires, the temperature having become on a sudden icy cold.

The damage occasioned by this unseasonable cold, in the two nights of the 22d and 24th of August, to the standing crops of all descriptions, was very great in the northern provinces of Sweden, particularly Helsingland and the environs of Gefle, and in Franconia and Wirtemberg. At the beginning of October there fell a great quantity of snow in Scotland, principally in the counties of Ross and Aberdeen, where it lay two feet deep. On the 4th of the same month there was snow on the fertile plains of Bayreuth to the depth of three inches; on the 9th it covered the mountains of Urach, Vosges, and Brisgau; on the 12th the elevated plain of Woivre, in the department of the Meuse; and on the 16th, the mountains of Lozere and the environs of Mende. It was concluded from these premature appearances, that we should have a rigorous winter; and in support of the predictions to this purpose, as infallible as those of Mathieu-Laensberg, we had the old theory of nineteen years, and even that of an hundred-and-one years brought forward. In the first category the winter of 1817 corresponded to that so long and severe of 1793; in the second to those of 1716, of 1615, and of 1514. But the temperature changed anew in the first days of November, and continued so till December. On the 2d, 3d, and 4th of November we had at Paris thick mists, which gave place to a succesion of very fine days, so much so that on the 22d the country of Niort and the borders of the two Sevres presented all the verdure of spring time.

Terrestrial Phænomena.

On the 27th of June, at two o'clock P.M., some women of the commune of Vauvert (Gard) having washed a number of pieces of cotton muslin, and others of linen, spread them on a meadow newly cut to dry. Shortly after there was a great deal of very vivid lightning, which played particularly about the meadow where the clothes were lying; and on examining them it was found that all the pieces of cotton had become tinged with a yellow colour similar to that of nankeen, while those of linen had lost none of their whiteness. The yellow tinged stuffs were washed repeatedly with soap, but to no purpose; it was found impossible to take the colour out of them, or even to free them from the sulphurous odour which they had acquired.

About the same period numerous swarms of those beautiful insects which are vulgarly named *Demoiselles* or *Libellules aquatiques* (but of a sort apparently new and very large) were observed in several parts of East Holland, particularly in the environs of the town of Sneeck, subsequently at Hamburgh, and lastly at Stockholm, and several other parts of the north of Sweden, where they disappeared. They came from the south-west. They formed so dense a body that they resembled the thick clouds which precede a fall of snow. When they wanted nourishment,

nourishment, they descended all at once upon some field, sojourned there for some hours, and afterwards resumed their course. At night the air was quite crowded with these insects.

On the 2d of July the mountain of Hansruck in Upper Austria disappeared and gave place to a lake. This mountain was of great elevation, and gave its name to the country around. During the preceding month there had been various phænomena, which augured some ruinous event:—subterraneous noises—slight explosions on the exterior, &c. they had disquieted the people of the country greatly, and seemed as if designed to forewarn them of their danger.

On the 24th of the same month, the very opposite of this phænomenon occurred in Italy. An astonishing noise was heard in the territory of Ferentino; after which the waters of the lake of Porciano suddenly disappeared, and left their ancient bed quite dry. Eastward of the lake, at the foot of a neighbouring mountain, they discovered an enormous chasm, produced by some violent commotion, down which the waters had precipitated themselves into certain subterranean caverns which now serve as their receptacle.—The Romans prevented accidents of this sort by their famous canals of outlet, as we see in the lake of Albano; but the modern inhabitants of the volcanic country of Rome have not the same foresight.

In the month of August, another displacement, owing without doubt to the subterraneous conflagration of a bed of coal or sulphurous matters, happened near Salzbourg in Bavaria, on the borders of the Salza. A space of ground, of the extent of about fifteen acres, sunk down, and from the chasm left, flames continued to issue for four days afterwards, exhaling a strong sulphurous odour.

On the 12th of May preceding, the bailiwick of Rattenberg presented a spectacle still more frightful. A whole mountain tumbled down, and transformed into a desert a very fertile and well-peopled valley. The cause to which this has been attributed was the enormous quantity of snow which fell in the Tyrol, in February, March, and April, and which a sudden change of temperature and abundant hot rains had precipitately melted.

On the 5th July, at one o'clock in the morning, the waters of the sea suddenly withdrew from the port of Marseilles, and left it for some moments quite dry; but soon after returned, and spread as far even as the city. The same phænomenon was observed with still more remarkable characters on the 27th of June 1812, and occurred also in 1775, at the time of the famous earthquake of Lisbon.

The atmospheric whirlpools, which are attributed to a displacement of heated air, and by the action of which it is easy to explain

plain the pretended showers of sand, insects, &c. have presented two singular enough phænomena in the state of New-York and in the kingdom of Naples. The first was dinstinguished by some extraordinary circumstances:—it raised a young man to a great height, afterwards pitched him on a tree, from which it again snatched him and conveyed him to the foot of a mountain at some distance. The second happened on the 16th of August. Some washerwomen at work beside a fountain, out of the city of St. Angelo, saw in a serene sky a whirlpool advancing upon them: seized with fear they fled in great haste; immediately afterwards the whirlpool dashed upon the fountain, absorbed all the water out of it, and carried off the linen spread out on the neighbouring meadows to a distance of more than a mile, whence it returned in about an hour to the environs of the fountain, where it ceased, and redeposited all that it had carried off. The linen was found torn and full of holes, as if it had been perforated by gunshot.

Celestial Phænomena.

In the period of time under our survey the spots of the sun were successively dissipated and renewed. The grand spot, which covered nearly all the disc of that orb on the 23d of July, disappeared on the 4th of August. A great number of small spots were afterwards formed, which gradually united and concentrated into one:—subsequently in the month of September a division again took place into several groups, which between the 23d and 27th of October totally disappeared, before having touched the west limb of the sun. On the 5th and 6th of November a large spot was observed on the southern part of this orb: it is now divided into groups more or less numerous, some isolated, others more approximated.

On the 7th of August, Professor Stark, astronomer at Augsburg, observed a luminous band in the direction of the head of *Serpentarius* in the constellation *Hercules*. (For this, see account already given in Phil. Mag. for August 1817.)

On the 8th of September, at eight o'clock at night, there was seen in the vicinity of Richmond, in England, a globe of fire proceeding in a direction from south to west. It appeared of considerable size, and emitted from its top long streams of fire. Its progress was slow; but all of a sudden it glanced up into the heavens, and disappeared among the clouds. A similar phænomenon was observed on the 19th of November, at three o'clock in the morning, at Rochelle.

On the 19th of September a beautiful *aurora borealis* was observed at Glasgow. (For which, also, see account in Phil. Mag. for January 1818, by M. Chev. Dupin.)

Baro-

Barometrical Observations.

The barometer on the 26th of January and 1st of April attained the extraordinary height of 73 centimetres (27 inches); and on the 1st of November, at 52 minutes past eleven at night, it exceeded that by one degree and 6-10ths of a line, which is a millemetre more than the height to which the mercury rose on the 23d of February 1815.

XXXIII. *On the Influence which the Increase or Diminution of the Polar Ice has on the Climates of other Latitudes.*

To Mr. Tilloch.

Sir, — If the following observations appear of sufficient importance to merit a place in your journal, their insertion will much oblige your sincere well-wisher,

March 9, 1818. Z. A.

One perpetually finds given as a reason of the great cold in the high latitudes of the southern hemisphere, the great accumulation of ice round the south pole. That popular writers should transfer an idea derived from what we experience when ice is *brought into* a warm neighbourhood, or what we experience when ice, formed in the neighbourhood or in the place itself, begins, from the action of new and extraneous causes, to warm and melt—that *they* should have transferred this idea to the general and total effects of ice formed in any place, is not at all surprising;—but that the same notion should have been adopted by so many scientific persons, is indeed a lamentable proof, among a thousand others, how little the world is improved in the art of strict reasoning.

The formation of ice in any place tends to *warm* the neighbourhood, not to *cool* it. If the ice, without melting, accumulates year after year, there is obviously a perpetual yearly accumulation or accession to the world of free caloric; namely, of all that set free by the conversion of the water from a fluid to a solid state. And consequently, whatever *partial* cold may be produced, the *general* effect is an increase of warmth. Suppose the water of a lake in any country to become ice—What is the cause of this change? Not the presence of the water surely; but either the abstraction or the *locking up* of caloric in the neighbourhood, in consequence of certain grand processes of nature *of which we know nothing*, and which we do not imagine to depend on the existence of a lake in that place. This abstraction of caloric reduces the temperature about the lake below the freezing point; and then, by the tendency of contiguous bodies towards equal temperatures, (of the laws of which tendency we know

 something,

something, though not much) converts the water into ice. But the conversion itself is accompanied with circumstances that tend to temper and lessen this reduction of temperature; or, in other words, to warm the neighbourhood: namely, the impossibility of water (even when reduced below the freezing point) becoming ice, without further giving out a large portion of caloric.

If the basin of the lake was filled with sand instead of water, the cold experienced in the neighbourhood from the operation of the grand causes above alluded to, would be severer than it is when water occupies the place; that is, the presence of water tempers the cold. It is true indeed, that afterwards, if by some other grand process of nature the air of the place should be warmed, and any of the ice should melt, that action of melting being accompanied by a specific absorption and locking up of caloric, the temperature of the air in the neighbourhood, and in the places to which that air is carried, is, *at that time*, made lower than it would be, if, instead of ice being present, sand or stone at the same temperature occupied its place. But even if the whole of the ice should melt, the *loss* in temperature *now*, is no more than the *gain before*.

In general, one may suppose that the presence of a large body of water tends to equalize the temperature of a place; cooling the air in the warm weather, and warming it in the cold. But however this may be, it never can be fairly said that the *accumulation of ice* tends, *on the whole*, to lower temperature. And if it be true, that ice has been accumulating for centuries back about Greenland, that accumulation must have given warmth to Europe (or at least to the world) during those centuries. And if this ice be now melting, that process of melting is one occasion of diminished heat. And when the ice shall be totally gone, supposing such an event possible, the then persevering of the waters still and unfrozen, spite of the freezing cold superinduced upon their neighbourhood by the grand processes of nature, would be no benefit to the world with respect to warmth; but the formation anew, and perpetual new accumulation of ice would, on the contrary, furnish a perpetual supply of caloric to warm the world, or temper the severity of the cold produced by other causes independent of the existence of these waters.

That there has been a tendency of late years in the ice round the north pole to melt and not to accumulate, may be plausibly inferred from what has been noticed of late of its breaking up, and detaching itself in masses. We observe ice to break and detach (and become what the boys call rotten) in thawing weather; while its thickness shall be still double or triple of what, in the contrary state of the atmosphere, would have ensured its firmness and the adhesion of its parts together.

XXXIV. *On a Case of Formation of Ice on an Alkaline Solution.* *By Mr.* Gavin Inglis.

To Mr. Tilloch.

Dear Sir, — A curious case came under my observation this morning, of a formation of ice on a solution of ashes. It had so much attracted the attention of the servants before I got sight of it, that a number of them were ranged round the boiler in a state of admiration, looking at what they called *the pattern*, alluding to beautiful six-pointed stars of the most regular formation which covered the surface of the liquor, each point bearing a most striking resemblance to the termination of a full-spread fern leaf. The most beautiful and perfect were in the centre, towards the sides the same form of leaf continued, but they were laid rather like a parcel of stars previously formed, thrown confusedly over one another. The complete stars measured from the centre to the point of the figure $2\frac{1}{2}$ inches. The first glance of this ice struck me as bearing a strong and marked resemblance to the snow observed by Dr. Clarke during his stay in St. Petersburgh. I immediately sent for that volume of his Travels, and on the spot compared the figure given in vol. i. p. 12, and found it was impossible to give a more exact representation, than by extending the dimensions of Dr. Clarke's fig. 1. The beautiful radiations of this ice must have proceeded from the component parts of the solution, which was made from ashes recovered from waste lees highly carbonated, containing some ammonia and a portion of nitre. The latter is formed in considerable quantity in the lees during the operation of bleaching, particularly when cottons are under operation. The specific gravity of this solution was 1·115. Two other boilers containing a solution of carbonate of potash, the specific gravity 1·057 and 1·073, were covered with a coat of ice, soft and porous, better than an inch in thickness, rather resembling wet snow slightly compressed, having no regular figure, and little more adhesion than to admit its being taken off in flat pieces: no appearance of lamination whatever, whereas the laminated ice was thin, solid, and shining.

Dr. Clarke in his Travels, 4th edit. vol. i. p. 11, marked on the margin "Extraordinary Phenomenon," says, "The season began to change before we left Petersburgh, the cold became daily less intense, and the inhabitants were busied in moving from the Neva large blocks of ice into their cellars. A most interesting and remarkable phænomenon took place the day before our departure; the thermometer of *Fahrenheit* indicating only nine degrees of temperature below the freezing point, and there was no wind. At this time snow, in the most regular and beautiful crystals, fell gently upon our clothes, and upon the sledge as we were driving through the street. All of these crystals possessed

exactly

exactly the same figure and the same dimensions. Every one of them consisted of a wheel, or star, with six equal rays, bounded by circumferences of equal diameters; having all the same number of rays branching from a common centre. The size of each of those little stars was equal to the circle presented by the section of a pea into two equal parts. This appearance continued during three hours, in which time no other snow fell; and as there was sufficient leisure to examine them with the strictest attention, we made the representation given in fig. 1.

"Water in its crystallization seems to consist of radii diverging from a common centre, by observing the usual appearances on the surface of ice:—perhaps therefore it may be possible to obtain the theory and to ascertain the laws from which this structure results.

"*Monge*, President of the National Institute of Paris, noticed in falling snow, stars with six equal rays descending, during winter, when the atmosphere was calm. *Haüy* records this in his observations on the *muriate of ammonia*."

As all regular crystallization must be governed by, and depend on, some unalterable laws in nature, I have no doubt but the Russian snow observed by Dr. Clarke, and the Parisian stars noticed by M. Monge, and the above radiations on this alkaline solution, were identically from the same cause—the presence of ammonia, and nitre, in both. The quantity of ammonia produced in large cities must be immense: Independent of every other source, what must be formed in the ordinary culinary operations of the kitchen? this must be driven into the atmosphere. From the same source nitrogen *per se* may be supplied in no mean quantity, or liberated by the decomposition of a portion of the ammonia. May not condensation be of use in atmospherical combinations, and nitrates as well as ammoniacal salts formed, and the aqueous vapours impregnated with these saline productions, prior to freezing or forming into snow, and the beautiful regularity of this phænomenon proceed from the habitudes of ammoniacal crystallization as recorded by *Haüy?* May not this also account for the extraordinary quantity of nitre found in some soils where deep vegetable mould predominates? The nitrogen descending with rain or snow, may combine with the potash of decayed vegetables already existing in the soil, and become the parent of this native salt. Or can it be possible that the mere abstraction of caloric has any share in the formation of potash, and hence nitre? It is well known that frost alone produces in potatoes a saccharine matter that renders them sensibly sweet to the taste. It is also known to you, that potatoes once gone into putrefaction by the effects of frost, contain *nitre* in such quantity as to answer the purpose of making match paper: before the potatoe undergoes these changes

changes by the effects of frost and putrefaction, no saccharine matter is perceptible, nor nitre to be found: From whence come *hey?* I remain, dear sir, yours,

Feb. 6, 1818. GAVIN INGLIS.

XXXV. *On supposed Lunar Projectiles, on Aëroliths; and on Notices of Comets.*

To Mr. Tilloch.

SIR, — I DOUBT you have bestowed more elegance and *delicacy* of *engraving* on my hasty illustrative diagram than it merited.

I anticipate some objections.

First, That the *course* of a projectile from the *moon* would be much nearer *rectilineal.*

But in *this* particular instance it would require to be much otherwise to allow for any *possibility* of reaching the *earth* at all had it been projected from the *moon;* then not far distant from the meridian of our antipodes.

Had it been thrown from the moon it is pretty evident, from the path described, that we saw it while near its *utmost altitude* above our hemisphere.

Had its course been nearly rectilineal, its apparent path must have differed greatly from that described.

It follows that it was either a *volcanic* projection from the *earth,* or inflamed *hydrogen,* in *our* atmosphere, or an *electric* fire-ball, or a proper *aerolithic** mass generated in the air in the manner suggested in the communication which introduces mine.

The advocates for *lunar* projectiles will perhaps say, that to carry such a projectile to the *earth* it needs to proceed no farther than to the point where, *mass compared with distance,* the terrestrial attraction would overbalance the lunar, or about 7 or 8000 miles from the moon's centre.

But I own my doubt, whether it would be so far within the earth's attraction as to fall on it unless the projectile force first carried it to a distance *within* the *common centre* of *gravity* of the *moon* and *earth,* or about 6000 miles from the *earth's centre.*

For it is to be considered, that to carry a planet, *primary* or *secondary,* from its orbit, requires not merely a force greatest at the point of its *immediate influence* than the attraction of the primary, but *so much* greater as will be *incompatible,* when it has passed *that* point, with its being at all *retained* in its *orbit.* Now this demands a far more preponderating attraction than is required to alter considerably for the time the *figure* of the orbit. A comet, for instance, in its *aphelion* may be much more attracted by *Jupiter* or *Saturn* or the *Hersilian* planet, in the *moment* of

* Αερολιθοι.

passing

passing near them, than by the SUN; but this transient disturbance may be very far from sufficient to prevent its *continuing* in its orbit, though for a single evolution it may *retard* or (according to circumstances) *accelerate* its return.

It is further to be considered, that the projectile force acting at the moon must be exceedingly great to throw the body even 15 or 20 *semidiameters* of the moon, and may be such as instead of ever carrying it to the *earth* would carry it in a *parabolic* or *hyperbolic* curve immeasurably *beyond* the earth. And that it is therefore perhaps not too much to suppose the probabilities many *millions* against one, against a *lunar* projectile *ever* being so thrown under such circumstances, of its *direction* and *rate* of motion, and the *position* both of the earth and moon, as would admit of its falling to the earth. It would rather *fall back* to the moon, revolve *about* the moon, or about the *common centre* of gravity both of *that* and the *earth*.

I doubt whether the *sound* were connected with the *phænomenon* of 8th Dec.* (7° 12¼ Astr. &c.) If it had, the *meteor* being at least 38° above the horizon might have been seen to *break*;—it was above 60 at its greatest altitude. Its sudden disappearance might be owing to *extinction* (See W. HUME's *Essay*, p. 87.) or to *very rapid* motion in a line *receding* from the *earth*. That it was either a *gaseous* body, or kindled *different* gases in its passage, appears highly probable from its beautifully and *variously coloured* light.

If a *meteoric* body of the iron kind, I believe we may say that *two miles* height above the *earth* is a very moderate supposition. And then, if it subtended an angle of about equal to the moon, its *real* diameter at that distance will be found about 44 feet.

And an *arc* of 60° being near = rad., supposing its *path* to have been two miles, if it described it in 8″ this is a quarter of a mile for each second, or 1320 f. 15 miles per min.: 900 by the hour. If in 5″ it would be nearly double of this velocity, 1056 × 2 = 2112 f. per second.

Observations are unfortunately wanting in *other* places which might have determined its *parallax*: and consequently its *height* in the atmosphere, *magnitude* and *velocity*.

Most of the excellent rules of Dr. MASKELYNE that are of chief importance, seem conformable to the mode in which this observation was taken, and has been reported. I regret that I missed seeing so beautiful and interesting a spectacle.

Permit me, before I close this, to add a word or two on comets.

I need not say to you that the *foreign* papers, especially the *French* and *German*, including *Prussia*, give, or used to give, the

* See Phil. Mag. for December last, p. 469.

rate

rate and *direction* of apparent motion, the *Right ascension* and *declination*, or if not these, the *longitude* and *latitude* referred to the *ecliptic*, or the *polar distance* of the phænomenon observed. They give, at least, *two* such observations to the periodical publications, at more or less distance as circumstances may render expedient; these are *early* given, and with *promptitude* inserted. And if they add the *popular* description by *constellations*, they do not say in *Andromeda*, *Hercules*, *Orion*, or *Draco*, or *Cygnus*, simply; but in what *part* of the constellation, or near what known *principal star* or between *two* such, or in a triangular, equilateral, or equiunial; an acute or obtuse angle with such or such stars, instead of leaving such an enormous scope for uncertainty as renders the description as to those vast constellations, of hardly any use.—While in *our* papers the *earliest* notice of such phænomena for the most part is many weeks later, and the original article I fear generally very imperfectly and vaguely *abstracted*.

With *two* such observations as the *foreign* papers were accustomed to give, the *plane* of the *ascending* and consequently of the *descending* node, rota of *mean* apparent motion, *place* and *time* of coming to the *perihelion* were all accessible to a common observer, sufficiently near in general to *trace* its path on a great circle of the *globe*, or a line drawn on a *chart* of the heavens; from which it would not sensibly differ, unless perhaps near the end, and thus to anticipate its *visible path*, and *where* to find it on a particular day.

The *comet* seen by Professor *Olbers*, 1 *Nov*. last, is probably the *same* with that which our papers say was observed by Mr. *Du Pons* at *Marseilles*, and Mr. *Blanpain* at *Paris*, in Dec. and June last. But the statement which I have seen is so vague as to be no guide whatever. It might have been passing from its ascending node to its perihelion northward, or southward from its descending node, and where to look for it is left equally at random.

I have observed some nights a cometary appearance between *Betelgaeze* and *Procyra*, unequally *nebulose*, with a *brighter* speck *not* in the centre, about from 12′ to 16′ in length, and about 8′ in width. I take it to be in the *nostril* of *Monoceros*. I find *no nebula* or cluster registered very near that situation: with the nebula not at all near it I am pretty well acquainted. Between 27th Feb. and 9th March inclusive, I think it seems to have advanced about 2° in R. A. from about 93°; but for want of requisite astronomical apparatus I am uncertain. This *intimation* may lead perhaps to better intelligence.

I remain, sir, yours sincerely,

CAPEL LOFFT.

May

May I request the correction of the underwritten *errata*:—those in *figures* with one exception may be my own.

Page 112, *for* Crichman, *read Crichmore*: page 113, line 2, *read* at $4\frac{1}{2}°$ at $2\frac{1}{4}°$ *instead of* and. In the plate *for* 146,000, *read* 130,000: and in the other numbers 156, 182, 208, 234,000: and *for* 922, *read* 322,000.

XXXVI. *Geological and Mineralogical Survey of Part of the Yorkshire Coast. Drawn up by the Rev.* GEO. YOUNG *from Materials chiefly furnished by Mr.* J. BIRD*.

THE mountainous tract, bounded by the plain of Cleveland on the north and west, and the vale of Pickering on the south, may be distinguished into four parallel ridges of hills, running from east to west. The first commences with the lofty cliffs at Boulby, and terminates at the western extremity of Barnaby moor. The highest parts of this ridge are Easington heights, Huntcliff, Burleigh moor, and Eston Nabb, which rise from 600 to 800 feet above the level of the sea. The second ridge, which comprehends the moors of Aislaby and Danby, and extends to High Cliff Nabb and Rosebury, is considerably more elevated; Danby beacon being 966 feet high, and Rosebury Topping 1022 feet. This ridge is separated from the former by the vales of Guisborough, Skelton, Lofthouse, and Dalehouse. On the south, the vales of Kildale, Common Dale, and the Esk, part it from the third ridge which is much more extensive, and forms the central and most elevated part of our moors; beginning at Peak and the Fyling hills, and proceeding westward by Lilla cross, Silhoue, Cock Heads, and Ralph cross, to Burton Head, Cold moor, and Cranimoor. The western part of this ridge is by far the most lofty, the heights at Cock Heads and Ralph cross being 1400 feet above the level of the sea, Burton Head † 1485, and Cranimoor upwards of 1500. A spectator on Cranimoor can observe the sea over the summit of Rosebury. This ridge is of great breadth, especially in the middle, where it reaches from Danby dale to the valley at Lestingham and Hutton. Immediately beyond the latter valley we find some of the hills of the fourth ridge, which takes its rise near Scarborough, and includes the hills of Seamer, Silphoue, Langdale, Crosscliff, Saltergate, Cawthorn, Spaunton, Gillimoor, &c. extending to the vale of the Rye. In point of height,

* From the History of Whitby and Statistical Survey of its Vicinity. By the Rev. Geo. Young.

† This hill is erroneously called Botton Head by Colonel Mudge. The Colonel has committed an error in regard to the angle taken at Burleigh moor formed between Barnaby moor and Rosebury Topping; he makes it 42° 58′ 56″ 5, whereas it is only about $38\frac{1}{2}$ degrees.

height, these hills correspond nearly with the second range; but they are of a different character from all the rest, being distinguished by the striking similarity of their abrupt northern fronts, forming the same angle with the horizon, and having the same smooth appearance, wearing a covering of short ling and moss, and rarely presenting any broken ground or naked rocks.

In all these four ridges, as in many other mountainous tracts of Britain, the hills generally rise with a gentle slope from the south, and fall abruptly in steep cliffs towards the north. A few of the smaller hills are nearly round, so that they appear like works of art; as Freeburgh hill, Oliver's mount, Blakey Topping, and some hills on the west of Langdale; most of which have tabular summits. Freeburgh and Blakey have indeed been pronounced artificial, by authors who had never examined their structure.

II. *Nature and Order of the Strata.*—None of our hills belong to the primitive class; they are all of the secondary formation, composed of strata, or beds, of various descriptions. There are few places where the stratification can be examined with equal facility; for, besides the opportunities for such investigations afforded by our inland cliffs, and by cuts or deep channels worn by rivers and mountain streams, our bold and lofty shores present complete sections of the strata along the coast. The strata, as in most other hills, are seldom parallel to the horizon, but generally dip towards the south, their inclination corresponding with that of the hills themselves, as above described: and they often assume an undulating form, the undulations bearing some proportion to those of the surface, rising in the heights, and falling in the valleys; the strata being thickest and highest in the most elevated situations.

The great bed of aluminous schistus, or alum-rock, as it is commonly termed, first demands our attention. At Boulby cliffs this immense stratum rises about 450 feet above the level of the sea. In the upper part of the bed, the rock is of a dark slate colour, feels soft and unctuous, like indurated clay; the laminated fracture is smooth and shining, the transverse fracture dull and earthy; it divides horizontally into thin laminæ, and, where exposed to the effects of the atmosphere, splits into shiver or shale, which is blown about by the winds. The natural seams, or partings, are in an inclined direction, dividing the rock into regular rhomboidal sections, the size of which increases in every successive course from the top downwards, the texture of the rock becoming harder and firmer as we descend. At the depth of about 250 feet from the top of the bed, the schistus loses its smooth unctuous feel, and becomes mixed with a large portion of sand and mica in shining scales. In this part of the bed, about

60 feet

60 feet in thickness, the colour changes to a light yellowish gray; and we find here some bands of iron-stone, alternating with the schistus. Below this part, the rock recovers its softness and smoothness; and at the depth of 140 feet more, the schistus sinks below the level of the sea, and how far it descends has not hitherto been ascertained.

From the experiments made by Mr. Winter*, the schistus is found to contain alumine, silex, magnesia, lime, oxide of iron, bitumen, sulphur, and water; the proportions of which vary considerably in different parts of the bed. The upper part abounds most with sulphur, and therefore yields the greatest quantity of alum; a cubic yard at the top being as valuable as five cubic yards at the depth of 100 feet. Of course the specific gravity of the schistus is not uniform: Mr. Winter states it at 2·48.—Calc spar often occurs in the veins of the rock.

The aluminous schistus abounds with pyrites, which makes it subject to spontaneous combustion, when great quantities of that substance become suddenly exposed to moisture and the effects of the atmosphere. Some years ago, a considerable part of the cliff between Sandsend and Kettleness fell down and took fire, and continued to burn for two or three years.

In this bed, that curious stone, called conical coralloid, occurs in abundance. It is found adhering, like a shell or crust, to large oval or lenticular blocks of hard calcareous stone, from which it is not easily separated. It is composed of an immense number of cones, from an inch to six inches in height, with all their apices pointing towards the central block, and the interstices between them filled with calcareous matter. The cones are variously aggregated; the larger containing several concentric cones within them, and one cluster often encroaching on another, or reclining on the side of another, so as almost to make their apices meet. When the stone is broken, the cones are very discernible in the fracture, and may often be taken out singly, or in clusters: they are transversely marked with undulating striæ, and their structure appears lamellated. In colour and feel, the stone resembles the alum-rock; but it properly belongs to the calcareous tribe, and bears some analogy to the stink stone.—The lenticular masses, incrusted by this fossil, are from a foot to six feet in diameter. In some of them are cavities, lined with crystals of calc spar, and filled with petroleum in a very fluid state. From the fragments of these stones exudes a kind of pitch, or indurated petroleum, which readily melts with heat, and when ignited burns with a crackling noise, and emits a strong bituminous smell.

* See his Essay in Nicholson's Philosophical Journal, for April, 1810, p. 247.

The

The same bed contains numerous nodules of what we may call cement stone, being the stone from which Roman cement is manufactured. The nodules vary in their form and size: they are often globular, and sometimes two are joined by a slender bar, so as to resemble a double shot. Many of them are coated with a shell of pyrites, a quarter of an inch think, and of a bright metallic lustre: they often contain extraneous fossils. These stones appear to be principally composed of argillaceous and calcareous earth, with oxide of iron, so mixed by nature as to form the proper composition for *terras*, or Roman cement.

On the top of the aluminous schistus rests a stratum of hard compact stone, from six to twelve feet in thickness. The workmen call it dogger, a name which they also give to the cement stone; and indeed its component parts seem to be nearly the same, but with a greater mixture of iron. The colour of the recent fracture is blueish gray, but, when exposed to the atmosphere, it changes to a deep purple brown. The transverse partings divide the stone into large blocks, nearly cubical; each parting contains thin plates, resembling rusted iron, and between the plates a soft ferruginous earth, apparently the result of decomposition. This bed of stone always covers the aluminous schistus where the strata are entire.

The superincumbent strata consist of alternate beds of indurated clay, iron-stone, coal, bituminous shale, and granulated sandstone; varying in number and thickness, according to the height of the hills in which they occur. The indurated clay always rests on the dogger. It is of a light ochrey colour, is soft and gritty, and divided into thin laminæ. Alternating with the strata of clay are several thin beds of iron-stone, and generally one or more seams of coal. Where the surface is low, the coal is seldom more than an inch or two in thickness; but where the hills are highest, the principal seam is from six to eighteen inches.

A little above the coal seam, there usually occurs a bed of siliceous sandstone, 20, 30, or even 40 feet in thickness. Over this stratum, bituminous shale and sandstone rise, in alternate beds, to the tops of the hills, in the first three ranges formerly described. Nodules of rich iron-stone abound in the shale: some of them are of the granulated kind, in which the green specks that often occur seem to indicate the presence of copper.

In the upper end of Tripsdale, a branch of Bilsdale, is a bed of bituminous schistus, of a dark brown colour, and soapy feel. It is easily divided into thin plates, which are used by the inhabitants of the neighbouring vales for baking cakes. The slates are soft and elastic when first dug out; but are prepared by roasting them in hot turf ashes, after which they will bear the heat of a common fire for several years.

 A stra-

A stratum of limestone, but too much contaminated with iron to be used for agricultural purposes, crops out on the east side of Cold moor. It is about eight or ten feet thick: and in the transverse veins are observed stalactites, curiously formed, some of them studded with pyramidal crystals of calc spar, commonly called dog's tooth spar.

In the front of some of the Cleveland hills, where the beds of indurated clay crop out, are seams of a fine yellow ochre, similar to the Oxford stone ochre. The same hills contain, in the bituminous shale, balls of a rich yellowish brown ochre, perfectly free from grittiness; perhaps produced by decomposed pyrites.—In the upper end of Greenhoue Burton, is a rock called the Rudd scar, from a seam of ruddle, or red ochre, which it contains, with which the farmers mark their sheep.

The sandstone beds which lie above the aluminous schistus are all siliceous; but differ greatly in their texture and hardness, some being soft and friable, while others are well adapted for building. On the tops of some of the moors a very hard siliceous stone, called crow stone, occurs. Near Hunt-house, in Godeland, is a large bed of stone, composed of fine white crystals, having so little cohesion, that the stone is easily crumbled to pieces between the fingers: the powder is used by farmers for sharpening their scythes.—Most of the sandstone contains mica; which occurs in a schistose state between the strata of sandstone, and is also found in fissures, in loose scales, which from their bright lustre have been sometimes taken for metallic ores.

Such is the stratification of the first three ranges of hills, which we may call the alum hills. In the southern slope of the third line, the aluminous bed sinks below the level of the sea, and rises no more. Its descent is rather rapid; for though it appears at a great height at Stoupe Brow, it sinks about a mile to the south of Peak; and the descent takes place in a similar form, throughout the whole of this range of hills, from Peak to Osmotherley, the place where it disappears on the Cleveland side. The superincumbent strata sink at a proportionate distance to the south, and then commences a new series of stratification, composing the fourth, or southern, line of hills. These we may term the limestone hills, as they consist of alternate strata of limestone, marl, and sandstone, resting on a bed of clay slate, of a coarse granular texture, and a light gray colour. This slate lies over the upper strata of the former series, that sinks beneath this; for this series has the same inclination as the former, dipping gradually towards the south, till it sinks in the vale of Pickering, or of the Derwent; beyond which another series appears in the chalky strata of the wolds.

The limestone is chiefly of the oolite or roe-stone species; and contains

contains in its fissures great quantities of calc spar, in beautiful lenticular crystals, about an inch in diameter, adhering to the rock by their edges. Fine specimens of this kind of spar may be seen in the rock on the north side of Scarborough castle.

In the limestone hills are numerous subterraneous fissures and chasms. There are no apertures to admit our entrance into them, as in the Craven lime rocks; but their existence is demonstrated by their effects, particularly in the absorption of water. In these hills it is rare to meet with a spring, till we come down to where their bases join the plain on the south; their dales and deep cuts are streamless and dry, except where rivulets flow through them from the hills of the third range: the waters are wholly absorbed by the fissures of the strata, and running down in these subterraneous channels at last burst out at the foot of the hills in springs of immense size, or rather in whole rivers. At Keldhead, near Pickering, the Costa rises from the earth in one vast volume of waters: at Brompton, a river bursts at once from the caverns of the limestone, and is collected at its very source into a large mill-pond, so that it drives a mill in descending from the ledge of rocks out of which it issues: and similar phænomena are observed at Ebberston, and other places along the foot of this range.—Nor do these cavernous hills absorb their own waters only, they also swallow up the rivers and streams which pass through their dales from the hills beyond them; for these streams, on their arrival at the limestone beds, suddenly disappear, and afterwards rise again on the south side of the hills, in a line with the springs which issue from their bases: at the same time a channel is left above ground, in which a portion of the water flows during winter, or in occasional floods, when the subterraneous channel is insufficient to admit the whole. The Rye sinks a little above Helmsley, and rises at a small distance from its proper channel, about a mile below: the Riccal disappears about a mile above the new bridge on the Helmsley and Kirkby Moorside road, and rises at Haram, a mile below, a few yards from its channel: Hodge beck descends into the rock a few paces below Holme Caldron mill, near Kirkdale church, and bursts up again at How-keld-head *, on the south side of the road, a mile west of Kirkby Moorside, and about a quarter of a mile east of its channel: the Dove, or Dow, sinks about twenty yards below Yawdwath mill, and after running near half a mile under ground, resumes its old channel about a furlong above Keldholm bridge; Hutton beck, or Catter beck, disappears about a mile north of Catter bridge, on the Kirkby Moorside and Pickering road, and starts up

* Keld-head means Spring-head; How-keld-head is Deep-spring-head, a name fitly given to that frightful basin from whence this river boils up.

again about half a mile below: and lastly, the Seven is swallowed up a little above Sinnington, and appears again in its own channel, not all at once, but by successive risings, between Sinnington and Normanby. Thus, in skirting the foot of these hills, the traveller crosses a succession of subterraneous rivers.

Caverns are also formed in beds of sandstone, not only by currents of water, but by the action of the atmosphere and the rains, washing away the loose sands or soft strata below, and leaving the harder strata above, in the form of a roof. In some instances, insulated fragments of the hard strata are left standing on a kind of pillars, like monuments of art. The rocks called the Bride-stones, running along the margin of a deep ravine, in the moors near Saltergate, about two miles south of Blakey Topping, furnish curious examples both of caves and insulated rocks. Some of the latter appear like mushrooms, supported on a narrow stalk; particularly one which is about thirty feet high, and in one direction near the top about twenty feet broad, while the stalk or pillar, which supports it, is only three feet across in one direction, and about seven feet in the other.

In the cliffs along the coast, the strata are not only liable to be decomposed by the atmosphere, but undermined and wasted away by the tides, especially in storms. The ratio in which this decay proceeds is not easily ascertained; but it does not appear on an average to exceed a yard in ten years, or ten yards in 100 years; for though in some spots the decay is much greater, in others it is much less. The notion that our abbey was a mile from the sea at its first erection is a groundless fancy: the port of Whitby always was where it now is: the cliffs might project 100 or 150 yards further in Hilda's time than at present, but that is the utmost extent that can reasonably be allowed. For the sake of future investigations on this subject, I would here state, that the distance from the outer edge of the north buttresses of the transept of the abbey, measured in a line with the middle of the transept, to the edge of a hole that seems to be an old quarry on the margin of a cliff, was found in 1816 to be exactly 634 feet, and the distance across that hole to the verge of the precipice, 46 feet more; making in all 680 feet from the edge of the cliff to the nearest part of the abbey, in the line of the transept. I may add, that the distance from the middle of the outer court gate in front of Mrs. Cholmley's hall, to the verge of the cliff, taken in a line with the cross, is 238 yards, or 714 feet: and, that the distance from the north-west angle of the tower of Whitby church to the nearest edge of the precipice behind Henrietta-street, is 70 feet.

Besides the numerous veins and vertical fissures that cross the

strata

strata in our hills, and the frequent undulations of the strata already noticed, some remarkable interruptions occur which demand observation. At the mouth of the Esk, a slip or downcast has taken place on the north side, the whole mass of the strata on that side being 80 or 100 feet lower than the corresponding strata on the south side; and this interruption seems to be continued throughout the whole vale of the Esk. A similar break is seen about two miles to the south of Carleton alum-works, where the north part of a hill has sunk wholly down about ten feet, exposing the section of a bed of sandstone, which, when viewed from the north, appears exactly like a stone wall running across the whole ridge from Bilsdale to Scugdale.

But the most singular interruption of the strata is that produced by the whinstone dyke, or basaltic ridge, which traverses our hills, like a vast vein. This is perhaps the most remarkable ridge of the kind in Britain, being 40 feet thick and often more, and being traced on the surface to the extent of 60 or 70 miles, in a straight line. It runs from Cockfield Fell in the county of Durham to the river Tees near Preston; and then, entering Cleveland, it crosses our district in the line laid down on the map, but has not been traced quite to the coast, the last discernible portion being at Blea hill, near the upper end of Harewood dale. The ridge rises perpendicular to the strata, and consequently inclines towards the south, the dip of the strata being in that direction: it proceeds nearly from W.N.W. to E.S.E. and seldom deviates from the straight line. In many places it does not reach the surface; in some, the top of it is on a level with the surface, or protrudes only a foot or two above it, as on the moor between Maybecks and Silhoue*, and in the descent from Silhoue towards the Mirk Esk; in other places it rises to a great height above the surface, as at Parker's houe near Lealholm Bridge, and especially in the long and lofty ridges which it forms in Cleveland. In these prominent parts of the whinstone dyke, it occupies a much wider space than the breadth of the vein; for there the higher portions of the ridge, having nothing to support them, have fallen down on both sides, especially on the south side to which it inclines: and hence such protuberances assume the form of oblong hills. The most remarkable hill of this description is on the south and west of Rosebury Topping: it is

* Here the moor road that runs contiguous to it is called the High-street, probably from the resemblance which the ridge bears to a paved road; though it is possible, that a Roman vicinary way may have passed in this direction, from the camp on Lease-rigg to the fort at Peak. I might have noticed, in speaking of the Roman roads, that some houses near Loftus are called Street-houses, which favours the idea that a Roman road might run that way from Dunsley to the mouth of the Tees.

named Langbargh, from its form, a name which it has imparted to the whole wapentake *.

This singular ridge is composed of blocks or masses, generally oblong, and lying across the vein, parallel to one another, in a form approaching to that of basaltic pillars, yet without any regularity of shape or size. The interstices are filled with a kind of ferruginous earth, or decayed whinstone, and the blocks are coated with a crust of the same colour: the recent fracture, which is rough and granular, presents a dark blue colour, with a number of small shining crystals. The stone is exceeding hard, and is excellent metal for making roads. Mr. Bailey, in his Survey of Durham, (p. 32.) justly remarks, that it "seems to have been in a state of fusion when it filled up the fracture, as the seam of coal, for some feet distance on each side, is turned into a sooty substance, which becomes a cinder as the distance from the whinstone increases, and by degrees assumes the natural appearance of coal with all its properties: which takes place about 50 yards from the whinstone." What impression it has made on the aluminous schistus, which it traverses in our alum hills, has not been ascertained: but in Langbargh quarry we see the south side of its bed, against which it has leaned, appearing smooth and firm, as though it had been baked.

XXXVII. *On the transverse Strength and Resilience of Timber.* *By Mr.* Thomas Tredgold.

To Mr. Tilloch.

Sir, — The growth of our own *ship-timber* has always been considered to be of great importance to this country; but on account of the slow growth of the oak, the demand, it is probable, will soon far exceed the produce of the British Islands: therefore, the introduction of the *larch*, which has been very extensively planted by a few patriotic individuals, is very justly esteemed an object of national importance, as the rapid growth of the larch far exceeds that of most of our native trees, and "it is remarked," says Dr. Hunter, "that those trees which have been planted in the worst soils, and most exposed situations, have thriven the best †."

The timber of the larch is durable; it does not burn readily, and

* The original name Langberg signifies Long-hill: the ancient name of Rosebury was Ohtneberg or Hogtenberg = High-hill. The wapentake courts were formerly held at Langbargh, and the steward still holds his court, *pro forma*, beside Langbargh quarry.

† Notes on Evelyn's Silva, i. 280.

and it is not inferior to any of the common kinds of timber (native or foreign) either in strength, toughness, or elasticity.

Results of Experiments, on the transverse Strength of Timber, made at Mr. Atkinson's, Grove End, St. John's Wood, on Thursday, March 12th, 1818.

The pieces were each an inch square, except No. 3, which was only 8-10ths of an inch in breadth. The numbers in the table show the weights it would have borne if it had been an inch square; the pieces were supported at each end, and were loaded by putting 5lbs. at a time into a scale suspended from the middle; —the distance between the supports 30 inches.

Description of Timber.	No. 1. Memel Timber.	No. 2. Red Larch.	No. 3. Red Larch old and very dry.	No. 4. English Oak.	No. 5. English Oak.	No. 6. Riga Timber.
Compar. stiffness—or the weight that bent each piece half an inch	145lbs.	30 lbs.	93 lbs.	60 lbs.	65 lbs.	125 lbs.
Compar. strength—or the weight that broke each piece	212 lbs.	253 lbs.	295 lbs.	222 lbs.	231 lbs.	212 lbs.
Compar. extensibility—or the space through which the middle had bent at the time of fracture	2·25 inch.	3 inches.	2·75 inch.	2·5 inches	1·4 inches	1·3 inches
Weight of a cubic foot of each kind of timber in the nearest whole numbers	34 lbs.	40 lbs.	31 lbs.	41 lbs.	46 lbs.	30 lbs.
Remarks	Broke short.	Splintered.	Broke short.	Broke short.	Splintered.	Broke short.

As the strength of small pieces depends much on the position of the annual rings, the pieces were placed as nearly alike in this respect as possible. When the pieces were in the position in which they were broke, the dark lines or portions of the annual rings that appear in the section of a piece were vertical.—From the results exhibited in the preceding table, it appears very clearly, that larch is best adapted to resist the force of a body in motion;—but to leave no doubts in this respect the following experiments were made.

Experiments on the Resilience of Timber.

The pieces were each an inch in depth, and laid upon supports thirty inches apart. The weight fell between two vertical guides (similar to a pile engine), upon the middle of the piece.

No. of Exper.	Description of Timber.	Breadth of the Piece.	Weight.	Height from which the Weight fell.	Effects.
No. 7.	Oak, same kind as No. 4.	1 inch.	7 lbs.	48 inches	Broke.
No. 8.	Larch, same kind as No. 2.	1 inch.	7 lbs.	48 do.	No effect.
	The same			54 do.	No effect.
	The same			60 do.	Set to a slight curve.
	The same			66 do.	A little more curved.
	The same			72 do.	Curved about an inch.
	The same, convex side upwards			72 do.	Curved the contrary way.
	The same		14 lbs.	42 do.	Broke.
No. 9.	Larch, same kind as No. 3.	0·8 inch.	7 lbs.	48 do.	No effect.
	The same			54 do.	Broke.
No. 10.	Oak, same kind as No. 5.	1 inch.	7 lbs.	48 do.	No effect.
	The same			54 do.	Broke.
No. 11.	English Oak	1 inch.	7 lbs.	54 do.	No effect.
	The same			60 do.	Broke.

No. 11 was a dark-coloured and apparently very strong piece of wood; specific gravity 0·872 or 54½ lbs. per cubic foot *. On the whole then it appears, that larch is superior to oak in stiffness, in strength, and in the power of resisting a body in motion (called resilience); and it is inferior to Memel or Riga timber in stiffness only.

I am, sir, yours, &c.

Grove End, March 16, 1818. THOMAS TREDGOLD.

* These experiments were made in the presence of his Grace the Duke of Atholl, Lord Prudhoe, Lord James Murray, John Deas Thomson, Esq. William Adair, Esq., Mr. Geo. Bullock, and Mr. Atkinson, architect to the Ordnance.

XXXVIII. *No-*

XXXVIII. *Notices respecting New Books.*

Encyclopædia Metropolitana; or, Universal Dictionary of Knowledge, on an original Plan: comprising the twofold Advantage of a Philosophical and Alphabetical Arrangement. Part I.

The word *Encyclopædia* is current amongst us, as the title of various dictionaries of science, whose professed object is to furnish a compendium of human knowledge, whatever may be their plan;—but to introduce any greater *method* into such a compendium than what belongs to a mere adherence to alphabetical arrangement has never, we believe, been attempted until the appearance of the present work.

In the *Encyclopædia Metropolitana*, (the first part of which is now before us,) three great natural divisions have been adopted, for the purpose of rescuing the body of philosophy and history from the confusion of miscellaneous and incidental information, viz. the Philosophical, the Historical, and the Miscellaneous; and the first of these has been subdivided into the domain of pure and that of mixed science—making, in all, four divisions. Of each of these the editors propose to publish a portion in every *part*, to make, in all, 25 volumes, the last of which is to be an index to the whole.

The work is preceded by a general introduction or preliminary treatise or method, which lays before the reader a general analysis of knowledge, or *arbre encyclopédique*, and states certain methodical principles on which the union of the sciences is conceived to depend. This is, in truth, doing what the term *Encyclopædia* implies ought always to be done; namely, forming a circle of science, all radiating as it were from one common centre—whereas in other works of the like kind we have seen, at the most, but segments of very different magnitude, and ill-assorted position.

The second division begins with a treatise on *Mechanics*, that science standing at the head of the class called the mixed, and applied. We have not room to enter into the whole of its detail: it appears, however, to be well and carefully written. It is introduced by a short historical sketch of the rise and progress of mechanics, as relating to solid bodies. Then follows a general view of *Statics*, their definitions and theorems, leading to a consideration, first, of the simple mechanical powers, the lever, inclined plane, funicular machine, wheel and axle, pulley, screw, and wedge, and afterwards of their combinations; and in this part is introduced an account of the recent experiments of Col. Pasley,

Pasley, at Chatham, on the pressure of earth, with the new and unexpected results to which they led.

The following extract from some remarks suggested by these experiments, as to the method of resolving the forces adopted by M. Rondelet, but rejected by M. Mayniel as incorrect and unscientific, will afford a fair specimen of the ability which has been brought to the support of this important branch of the work.

"A variety of methods have been proposed relative to the determination of the resultant and direction of the forces of the particles forming the solid DBE : some authors having estimated it by the power or force which is requisite when acting horizontally against the centre of gravity, as M. G., to support the body on the plane; and consequently these have found the point of application to be at 2-3ds of the height of the bank from the bottom of the revetment; others have found that point to be at one-half of the height, and others again at 1-3d. The latter has been the most general determination, and is obviously the necessary result of a correct theoretical examination and valuation of the direction of the forces : it has been also experimentally verified by Gauthey; but it is necessary to observe that this refers to the interior face of the revetment. Still, however, a singular error was committed in the resolution of the resulting force which was supposed to be made at the point F; so that K F being taken to denote the direction and intensity of the thrust of the bank at F. this was resolved into a horizontal and vertical force at that point which we may denote by L F and F H; but the latter of these was rejected, as having no efficacy either in causing the wall to turn about the point A, or in resisting that motion :—this would have been true, had the wall been merely a line without breadth, because then the points A and B would have coincided, and the force F H would have had its direction passing through the fixed point B : in giving to the line AB, however, any dimension or length, it is clear that while the product of L F × L A denotes the efficacy of that force to turn the wall about the fulcrum A; F H × A B will represent the contrary or opposite moment of the force F H, to resist that motion; and consequently when these products are equal, the wall will have the same stability as before any earth was thrown at its back; but when the former product is the greater, the stability will be diminished; and when the latter is the greater, the stability will be increased: results which were found to obtain from various practical experiments reported by Col. Pasley in his work above referred to.

"We shall come to the same conclusion, if we resolve our forces

forces at the point K; in which case the vertical having its direction passing through the fixed point A will truly become ineffective, and there will remain the product K H × K A, for the moment of the force by which the bank tends to overturn the wall: but here again it is obvious, that if the earth at the back of the wall be only so high that the line G F produced meet the base of the wall in the point A, the stability is the same as before any pressure took place: and if the line F G produced, cut the vertical C A, the stability will be diminished; but if it meet any point in the base A B, it will be increased. We have before observed, that these deductions are exactly conformable to the experimental results of Col. Pasley; and as to the point which we have assumed, or rather theoretically determined, for our point of application, it has been verified by the experiments of Gauthey; and therefore, thus far experiment and theory go hand in hand; which is always satisfactory to observe, particularly in cases where they have hitherto been found to give incomparable and anomalous results."

The historical division is introduced by a chapter, in two parts, viz. 1. On the uses of history as a study; and 2. On the separation of the early facts of history from fable.

These introductory essays are followed by a chapter on the Antediluvian Period—another on the Patriarchal Age, and the lives of Moses, Joshua, Sesostris, and Theseus.

The fourth division is formed on a plan intended to combine all the uses of an English Lexicon with those of a miscellaneous Dictionary of Science. In the former character it promises to present a much more complete history of our language than has yet been attempted.

"By commencing with authorities wherever they can be detected, from the earliest periods of English composition, and continuing them successively through the different stages by which it has arrived at its present state of copiousness and refinement, this Dictionary," say its writers, "will aspire to the pretension of exhibiting to the English reader a sketch at least of some very interesting and instructive portions of a history of his own language."

The plates by Davis, Landseer, &c. are neatly and even elegantly executed.

Mr. S. F. Gray (apothecary and teacher of botany and materia medica) has in the press and nearly ready, a work intended to serve as a Supplement to the several Pharmacopœias, containing the medical uses of all such plants as have been hitherto examined, and an arrangement of their uses, a glossary of the terms and contractions used by physicians in their prescriptions:

usual

usual medical formulæ arranged in classes: botanical practice of medicine, offered as hints to regular practitioners to improve the art.

This work is meant to supply the deficiencies of the present College Pharmacopœias, which, being merely intended to direct the preparation of the medicines most usually employed by regular practitioners, are of course defective in regard to the very numerous articles that are kept in the retail shops to supply their other customers; as medicines, perfumery, liqueurs, sauces, British wines, paints, varnishes, &c.

Mr. Luke Howard will shortly publish, in two volumes, a work entitled "The Climate of London, deduced from Meteorological Observations made at different Places in the Neighbourhood of the Metropolis." Vol. 1 will contain an Introduction relative to the construction and uses of several meteorological instruments; tables of observations for ten years, with notes and results; accounts of collateral phænomena in other parts of the world, and occasional dissertations. Vol. 2 will contain a methodical account of the climate of London, under the several heads of the Winds, Barometer, Temperature, Rain, Evaporation, Electricity, &c. deduced from the facts contained in the first volume; with copious general tables, and an index to the whole work. To which will be added, An Essay on the Modifications of Clouds, by the same Author, several times heretofore printed.

The first volume will appear in a few weeks.

A Prospectus has just appeared of a new and corrected edition of the Delphin Classics; with the Variorum Notes appended. To be entitled "The Regent's Edition." To be printed and edited by A. J. Valpy, M.A. late Fellow of Pembroke College, Oxford.

The whole will be printed uniformly in octavo, price 18*s.* in boards, each part, to subscribers, and 1*l.* 1*s.* to non-subscribers. Each part will contain 672 closely printed pages, without reference to the conclusion of any author, so that the subscribers may bind each author in as many volumes as they please, and arrange them *alphabetically* or *chronologically*, as most convenient.

Some copies will be struck off on very fine thick royal paper, with a large margin, and hot-pressed, price to subscribers 1*l.* 16*s.*, to non-subscribers 2*l.* 2*s.* each part. The price will be raised higher to non-subscribers, as the work advances.

The whole will make about 120 or 130 parts—and twelve parts will be printed in the year.

Among

Among the physiological publications of this month, we have to announce Mr. Curtis's Introductory Lecture to his Course on the Anatomy, Physiology and Pathology of the Ear, as delivered at the Royal Dispensary 1816. This lecture contains much ingenious reasoning on the structure of the Ear, from which the author has deduced important practical conclusions, rendering the lecture both interesting and amusing.

XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 5. The remainder of the paper by the Rev. J. Brinkley, professor of astronomy in the University of Dublin, On the Parallax of the Fixed Stars, was read.

March 12. A paper, by B. Bevan, Esq. On Belemnites and Orthocaratites, was read. The principal object of the author appeared to be that of demonstrating the proper connexion subsisting between these two fossils, which had been by many persons considered as distinct genera. The paper referred to a fine specimen presented by the author some years since to the British Museum, discovered between 90 and 100 feet beneath the surface of the ground, in a navigable tunnel on the Grand Union Canal in the parish of Crick, in the county of Northampton, and about 430 feet above the level of the sea.

ROYAL ACADEMY.

Sculpture.

Lectures by Mr. Flaxman, R. A. Feb. 23d.—The Professor commenced with a general introductory view of the progress of the art in ancient and modern times. He observed, that in rude times, before the art of printing was known, and for a considerable time after, the bas-relievo ornaments on churches formed the great source of instruction to the people; and that even at this day many thousands in Roman Catholic countries derive their chief information from them. The Professor, in alluding to the advantages afforded to students at the Academy, mentioned the valuable collection of casts presented to it by the Prince Regent, and expressed great satisfaction at the restoration of the originals to the Roman states, where they had been raised, at a great expense, from the earth under which they had been buried. The discovery of the celebrated Mosaic, comprehending the battle of the Centaurs and Lapithæ, and the Tritons and Sea Monsters, had alone cost 30,000*l.* sterling. Mr. Fuseli was in the chair, and the Lecture was well attended.

Feb.

Feb. 26. Mr. Flaxman commenced his discourse of this evening by stating that his inquiry would be directed to the origin of sculpture in Greece. This was at first said to be confined to the rude representation in stone and wood of the twelve divinities then worshipped. Examples of these figures would be found in ancient bronze, in their best finished state; they had a diadem round the head, on which there was a sort of engraving like that on the tomb of Agamemnon; the arms were fixed downwards, close to the side of the body, and the legs close together. About 1300 years before the Christian æra, sculpture began to attract universal attention; and Dædalus, the friend of Theseus, is described by Diodorus Siculus, by Pausanias, and others, as a distinguished artist. Of his style, some idea may be formed from the small bronze figures in the British Museum, representing the naked Hercules advancing with the right arm ready to strike, and on the left the shield of the lion's skin. This showed that energy and strong feeling were studied in those early ages in the statues of which these figures must be copies. The early Greeks employed the arts in the representation of their choruses. Pausanias saw the celebrated statue of Minerva, by Dædalus, from which the Greek coins are taken; but it must be observed, that though they gave energy to their figures at this period, they wanted distinctive and characteristic expression; their figures had all the same faces, and were only distinguishable from each other, their Jupiter by his thunderbolt, the Neptune by the trident, and the Hercules by the bow and palm. After the burning of Athens by Xerxes, Pericles employed Phidias to work at and superintend the re-edifying the city. The superior genius of this great artist as a painter before he practised sculpture, gave a softness and delicacy to his figures, which soon established a style very different from that it preceded, and which was stiff rather than dignified, and the drapery arranged rather in geometric lines than in the simple form and beauty of nature. The Professor here quoted the splendid testimony borne to Phidias and his works by Pliny, Quintillian, and other writers. The magnificent temple dedicated to Minerva, at Athens, from which the Elgin Marbles were taken, would immortalize his fame. Here the Professor described the dimensions of the pediments, and the different allegorical subjects of which the bas-relievos consisted; such as the mythological processions, the contests between the Lapithæ and Centaurs, and concluded his discourse by observing, that it took ages entirely to destroy the genius of grace in the arts—it did not lose its graces until the time of the Antonines—it preserved much of its character in the fifth and sixth centuries, when the breaking of images obliterated its works; and even until the overthrow of Constantinople, the

little

little islands of Greece were celebrated for some perfection in small works;—this taste still subsisted in the Christian æra in those states, from the Scriptural embellished missals at present in existence.

March 2.—Mr. Flaxman proceeded to consider the relation which the arts of design bear to all the branches of knowledge, and the powerful illustrations they afford. They were the arts of design, which enlivened early ages with the first dawn of knowledge, and which poured a fuller blaze upon succeeding generations. The Professor commented upon the subjects in which the arts would be most advantageously employed; and this would appear to be (in the words of Socrates) the human form animated by the human soul. The great powers that the ancients possessed in expressing actual life in stone, were displayed in the animals of the Pope's Museum, and more particularly in the horses of the Elgin Marbles. The state of art among barbarians was noticed, and the first rude attempts at expressing that form which they did not completely understand. The learned Professor then went through the rise and progress of Sculpture in antiquity, from the rude and stiff figures of the earliest ages to the splendid works of Phidias, in the time of Pericles, and concluded his observations by pointing out the imperfect perspective in the drawings of the ancients, and referring for a proof of this defect to Vitruvius, and the work called Euclid's Optics, but which was in fact a collection and compilation from those who preceded him. By the modern improvement, such as was exemplified by Michael Angelo, a depth in perspective was given greatly superior to the drawing in this respect of the ancients. Through this imperfection the drawings of the architectural works of antiquity were too often incorrect and inadequate for the display of their beauties.

March 16.—The Professor entered into the detail of the science of disposing *Drapery*, under two heads; first, as it is subject to the laws of gravity; and, secondly, as it is influenced by the laws of motion.—With respect to the former, he observed, that drapery, in its simplest state, represented one fold hanging from the principal projection of the body, and becoming complex according to the number of folds from different parts of the body, and their variation into different courses as they fall by gravity. The tunic of the Romans was particularly noticed, as were likewise some beautiful figures in Henry the Seventh's Chapel. Some ancient draperies were much finer and more transparent, and their folds strongly resembled modern muslin; this was particularly the case in figures of nymphs, terrestrial and marine, Bacchanalians, &c. As a specimen of this description of drapery, a very beautiful drawing from an antique of Iris was exhibited.

In

In some transparent draperies, the limbs were seen as though bare; the drapery, playing in beautiful creases over the bolder parts, adding the magic of diversity to the charms of beauty. The lecture concluded with appeals to the Prophets of Michael Angelo, the Apostles of Raphael and Albert Durer, and the Holy Men in Henry the Seventh's Chapel, as exhibiting the most sublime conceptions expressed in drapery.

March 23.—Mr. Flaxman delivered a concluding lecture, the subject of which was *Composition.*—The very early compositions of Greece were generally the representations of heroic subjects, or of celebrations of rites, as the combat of Theseus and the Minotaur, Hercules and the Centaur, Dejanira and Nessus, the rites of Bacchus, &c. all under the influence of a rude character. After the stores of Persia became the treasures of Greece, the art was characterized by truth, beauty, and inspiration. It saw its zenith of perfection in the Jupiter and Minerva of Phidias. When the Greeks were no longer free, those large compositions which had astonished the world ceased to appear; yet under the influence of the same love for their country and its traditions, they exercised their genius upon smaller works. The abasement of sculpture in Italy was noticed. Upon turning again with interest to Greece, it was observed, that the nation which had propagated by art the heathen traditions, was the one to exercise its genius on subjects relative to the true religion. The particulars of composition were entered into; its forms, as the pyramid, the cone, the inverted cone, &c. Bas-relievo may be considered as painting without colour. The ancients held simplicity in so high respect, that in their bas-relievos they generally placed their figures all upon a line; yet, there are to be seen in some, files of horses in perspective, with riders, without the least confusion. The Professor then entered into some particulars of science, especially the harmony of lines, and the effect of light and shadow in sculpture.

Mr. Flaxman divided sentiment in sculpture into three classes; —the sublime, which generally refers to the different acts of creation; the heroic; and thirdly, the pathetic; which is exemplified in the Charities and Holy Families of Michael Angelo.

Comments next followed upon some of the most sublime compositions that the world has seen; the Judgement by Michael Angelo, and the Fall of the Rebel Angels by Rubens. Here the beholder is thunderstruck, with the vastness of the conceptions and the tremendous precipitation of energy enveloped in clouds of furious smoke. The work of Michael Angelo was the most consummate performance of the two, and was the parent of Rubens's. If Rubens held the superiority in profundity of colour, skill of light and shade, and breadth of masses, Michael Angelo far

far exceeded him in character, in pathos, and in design. It is only equalled in individual character and energy by the Laocoon and the Boxers.—Perhaps the first great composition that appeared since the Grecian æra was that by Leonardo da Vinci, from which Michael Angelo probably imbibed some of those principles which regulate his large works. It is to the productions of Michael Angelo that we are indebted for the celebrated hunting pieces of Rubens, in which the genius of the artist is so admirably displayed, that the men shout and cry, the horses snort and kick, the animals howl and roar. The limits which circumscribe sculpture were pointed out; the human figure was the principal, and almost the only subject of the sculptor's study. Some remarks followed upon the utility of rules, during which, it was observed, that a servile observance of the best rules, superadded to a power of manual labour, would produce nothing; but that sentiment was the life and soul of liberal art, and gave an invisible charm to the rudest imagery. The lecture concluded with a general review of all the discourses.

Lectures on Painting. By M. FUSELI, R.A.

[Continued from our last.]

March 5.—The lecture of this evening was entirely upon *chiaro-scuro* and back-grounds.—The literal sense of this compound word is well known. In its contracted sense it is applied to a single figure only; and in its most extensive, to a composition. The use of the *chiaro-scuro* is to give substance to form, place to figures, and to create space. It is legitimate or spurious: when the former, it is the assistant of expression, form, and character; when the latter, a palliative for them. Of every subject unity is the soul; unity, therefore, is inseparable from *chiaro-scuro*:—the next requisite to *chiaro-scuro* is truth. Mere light and shadow, as seen commonly in nature, are not legitimate *chiaro-scuro*; it is the business of art to arrange, by fixing one central light from which all others must emanate like rays. The most extraordinary, the most astonishing effects of light and shadow, when directed by comprehensive genius, become legitimate *chiaro-scuro*. The most natural, without it, is spurious. It was remarked, that *chiaro-scuro* sprang from Leonardo da Vinci, and the degree of doubt, and even censure, with which the use of it was then received, was very extraordinary; only one member of the Tuscan school adopted it, and it was afterwards suffered to dwindle to evanescence. The Roman school never adopted it; no principle of it is to be traced in any of the works of Raphael; it was the school of colour, Venice, which paid implicit obedience to its mandates. There it first appeared with Giorgione. Several

veral fine works of the masters of this school were commented upon, for exemplification; particularly the Peter Martyr and St. Laurence of Titian, and the celebrated awful picture of Tintoretto; where the expression of twilight, eclipse, or what precedes a storm, an earthquake, a hurricane, displays all the powers of *chiaro-scuro*.—In the pictures of Paolo Veronese, on account of that gaiety of mind which ever led him to represent large assemblies, *chiaro-scuro* is in a great measure absorbed in variety of colours. But the highest summit of perfection in light and shadow was to be found in Corregio; design, composition, and colour, were all subordinate to his *chiaro-scuro:* he expanded its powers through heaven and earth. The Professor gave a masterly character of Corregio.—The rest of the lecture was occupied by observations upon *back-grounds*: it would be tedious to follow him with any degree of minuteness. The great advantage Poussin's pictures derive from them, and their total neglect by Raphael and Michael Angelo, were observed.—A picture might become sublime and pathetic, by an appropriate back-ground; by this, also, a sublime and pathetic picture might become more interesting. The figure of a female, seated on a rock, would express little more than insipidity; the genius of Reynolds had elevated its character, by the contrast of a boisterous ocean in the back-ground. The genius of Reynolds, likewise, in the figure of a female contemplating the moon upon the lucid waters, had amalgamated elegance with sympathy and desire.—In the course of the observations upon the back-grounds, it was remarked, that the Apollo Belvidere and Jupiter, of Phidias, were never formed for a room, and that we might very frequently discover how far an artist had penetrated his subject, by his choice of scenery.

Thursday, March 19.—The Professor began by observing, that the commencement, the progress, the finish, the reputation of an artist's work depended upon the faculty of *Invention*.—The first demand from invention was a complete whole; the second, that it should clearly tell its own tale—for, as a poem would be little entitled to praise that depended for its light upon annexed notes, so a painting would deserve little reputation that required illustration from a commentary. The Professor divided subjects for the practice of invention into three classes; 1st, positive, advantageous, commensurate; 2dly, negative, uninteresting in themselves and depending upon the genius of the artist; and 3dly, repulsive—that cannot pronounce their own meaning, on which the genius of the artist is wasted, and which never make impression, or stamp a work with perspicuity.

With respect to the first; without much boldness of invention, it draws its subjects from the lap of nature; such are the Madonas

donas of Raphael, the burning of the Borgio, the Petrus stabbing himself while clasping the body of Arria, the tugs, the grasp, the groan of the Laocoon. These subjects, without reference to time or place, speak the language of all mankind. Under this class also comes History, which expresses time, and gives a "local habitation and a name." We are pleased with the former subjects as men, with the latter, as members of society.

Legislators, philosophers, discoverers, polishers of mankind, patriarchs and divines, fall under the latter head. Whatever makes time and place contribute to display pathos and character, is the legitimate style of elevated history, more properly denominated Dramatic. Mr. Fuseli pointed out the cartoon of Paul preaching at Athens. The dramatic was the style of Euripides, of Shakspeare, and of Raphael. Under the first class was likewise included, in the name of commensurate, the Epic, with its allegoric and symbolic attachments. The epic, which was the most sublime effort of human invention, was the allegory of a maxim. It admitted history as its basis, by its vastness; it concealed its boundaries. Nature reflected character; character was overwhelmed by *genius*. In it, heaven and earth mingled; men became demigods; gods descended to men. This was the style of Homer, of Michael Angelo, of Milton. Mr. Fuseli took this opportunity to comment upon allegory, which, of all the paltry subterfuges to palliate invention, was one of the foremost. Dr. Johnson had said that the plastic arts can illustrate, but not inform; which the Doctor attempted to prove by saying, that a child, on being shown a figure of Justice with a pair of scales, would mistake it for a cherry-woman with steel-yards. The Professor conceived that this would depend entirely upon the genius of the artist. The general allegoric ideas of the ancients had a uniform taste. The inverted torch, the drooping flower, were accompaniments of, not substitutes for, Death. Mercury deprived of his *caduceus*, Apollo of his lyre, would still inform us of their characters.

A celebrated picture of Tiziano was also noticed. Under this class may be comprehended the ornamental style. Here, midst refulgent architecture, are displayed sumptuous tables of magnificence and luxury; gaudy bands of music, the lustre of eastern draperies, crowds of pages and dwarfs, glittering riches, splendid chaos; the whole embrowned and toned by a meridian sky. Under this class also may be comprehended portrait; not that portrait which comprehends and embodies the character and soul; but the mere transcript of feature. To this class also we add a similar description of landscapes, an imitation of hill and dale, clumps of trees, not assisted by nature, nor dictated by genius; little better than topography.—The landscapes of Titian,

tian, of Claude, of Rubens, and of Rembrandt, spurn all relation with this description of map-making.

On proceeding to the third, or repulsive class of subjects, which are impossible to be told by art, there were mentioned, as instances, the Delivery of the Keys to Peter, painted by Raphael, the Testament of the Athenian, and Moses in the Bullrushes, by Poussin. These were compositions without subjects.

XL. *Intelligence and Miscellaneous Articles.*

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for February 1818, it appears that during that month the following was the work performed by the engines reported, with each bushel of coals.

		Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
25 common engines averaged		22,424,449	various.
Woolf's at Wheal Vor	..	26,158,828	17·2 lib.
Ditto Wh. Abraham	..	35,364,694	16·8
Ditto ditto ..	..	28,012,278	5·3
Ditto Wheal Unity	..	32,306,943	13·1
Dalcouth engine ..	..	41,354,103	11·2
Wheal Abraham ditto	..	36,180,740	10·9
United Mines engine	..	31,830,623	14·6
Treskirby ditto ..	..	39,375,488	10·6
Wheal Chance ditto	..	32,319,967	8·9

THE NORTHERN EXPEDITIONS.

We are anxious to correct a mistake in our last Number. It is not true that any difficulty was experienced in obtaining suitable hands for the voyage, and that, in consequence, the vessels will complete their crews at the Orkneys. On the contrary, so many men offered their services, that four times the number of ships might have been instantly manned; and in fact most select crews have been obtained, composed of men from those parts of our coasts which experience has proved to furnish persons naturally endued with the largest portion of bottom, hardihood in dangers, and unconquerable spirit—qualities which do not belong to the seamen of the very northern regions, who, though from *necessity* they often expose themselves to cold, are easily depressed by dangers, and therefore but little qualified for such enterprises as are now contemplated.

These expeditions have excited a great degree of interest, not merely among philosophical and speculative men, but among all classes;

classes; and, as might be expected, various thoughts have been offered to the public in different forms, bearing upon this subject. The state of our climate for a long series of years has been investigated, and, in the Journal of the Royal Institution, great influence has been ascribed to the Greenland ice. The same argument has since appeared in the Quarterly Review from the pen of Mr. Barrow, who has produced a most interesting paper on the subject. His conclusions, however, are questioned by many; and it is probable the investigation which has thus commenced, may lead to the publication of various useful papers on the causes of periodical changes in the temperature of climates. On the physical laws which cooperate, we have inserted, in a preceding part of this number, a short but interesting paper from a correspondent under the signature of Z. A. to which we here add the following observations copied from the Glasgow Chronicle.

Mr. Barrow contends that "our climate, in the course of the last three years, has been particularly affected" by "the appearance of ice in the Atlantic;" that "in the summers of 1815, 1816, and 1817, more particularly in the two last, islands of ice, unusual in size and number, were seen as far down as the 40th parallel of latitude;" that one of the islands of ice was two miles in circumference, and 200 feet in height; that others were several miles in circumference; that Newfoundland during last summer was completely environed with ice; that greater quantities existed then in that quarter than were ever before observed; that "during the last two summers the mercury invariably fell with westerly winds;" and that it can scarcely be doubted that the late coldness of the seasons was produced by an atmosphere, chilled and condensed over ice-bergs and ice-islands, "rushing directly upon the British islands from the westward."

In proof of the coldness of the last three summers Mr. Barrow contrasts their temperatures with those of 1805, 1806, and 1807. The mean results are as follows:—

1805	59·3	1806	62·3	1807	62·85
1815	61·55	1816	57·8	1817	58·73

This statement of itself refutes Mr. Barrow's speculation. One of his *cold* years is 2·25 *warmer* than one of his warm ones, and the other two are only 1·5 and ·55 colder. In 1805, when there were no ice-bergs, the mean temperature of the summer was 59·3. In 1815, when there were ice-bergs, the temperature was 61·55. Wherefore, according to the Quarterly Review, the ice-bergs have chilled the climate!

It is true that 1806 and 1807 were 4·5 and 4·1 warmer than 1816 and 1817, but 1805 was scarcely any warmer; and if the temperature of that summer was not influenced by ice, how shall Mr. Barrow say that it affected 1816 and 1817?

 The

The following are mean temperatures observed at Kinfauns Castle:—

	1814	1815	1816	1817
Annual mean.	43·4°	45°	42·7°	44·26°

This is also fatal to the icy theory; for in 1814 we have no particular record of frozen islands; yet its temperature was lower than those of 1815 and 1817, when they are said to have abounded.

Again, the position of Mr. Barrow on which his argument principally rests, that "during the last two summers the mercury invariably fell with westerly winds," is wholly contrary to the fact; as may be seen from the following result of Mr. Howard's observations at Tottenham:—

	Easterly winds.		*Westerly winds.*	
1816, April to Sept. both inclusive.	52 days	53·6°	101 days	54·57°
1817. Do.	61	53·27°	95	55·23°

The attempt, therefore, to explain the weather by the ice-bergs fails in every point.

The facts not merely jar with Mr. Barrow's view, but are in direct opposition to it; and the slightest examination of his general principles is equally unfavourable to his conclusions. It is not conceivable that our west wind, merely by passing over the ice near Newfoundland, would be materially chilled in its general volume; nor, if it were, is it reasonable to suppose that the coolness would not afterwards be removed by the passage of the wind over 500 leagues of open sea. In the close vicinity of the ice, the air will indeed be sensibly chilled; and this effect is unhappily experienced by the eastern states of America, where the north-east wind is extremely injurious: but the case is entirely changed when the broad bosom of the ocean presents the warmth of the latitude of 50° to repair the partial diminution of temperature.

The real cause of the coldness of 1816 and 1817 was undoubtedly the wet and cloudy weather that almost constantly prevailed in summer. Rain is yet without a theory; but it seems pretty certain that it is governed by other circumstances than those which occur on the surface of the globe; and it is probable that the unequal distribution of electricity is the principal source of diversified seasons. If the theory of the Journal of the Royal Institution was authentic, we should have had less rain during the last two years than usual; inasmuch as the wind, being condensed by the ice-bergs, would lose a proportional share of its moisture.

What has been advanced respecting the vineyards and orchards of Great Britain, is nonsense too pure for controversy.

Philosophy is never useful but when her general reasonings are applied to special cases: but this is not done without hazard; since

since many who fancy themselves to be wooing her prosperously by abstractions, find, when their attentions become more particular, that she has only been coquetting with them.

While on this tubject, we may also notice the theory of Professor Parrot of Dorpart, who has written on the freezing of salt water as it respects the origin of the polar ice. Though navigators say that the polar ice contains no salt, yet the author thinks and proves that mere tasting cannot decide the problem. If the ice in the polar regions contains no salt, it cannot be frozen seawater, but ice of glaciers which cover the pole of our earth, and to which our European glaciers are mere molehills. The unsalt water flowing from the glaciers is lighter than the sea-water, and consequently keeps on the surface, makes the latter less salt, and thus more liable to freeze: therefore the ice which covers the polar regions must increase and continue to increase every year in height and extent; for this reason the climate of Iceland and Greenland becomes continually more severe, and these countries lose more and more of the inhabitable surface, &c.

HOTHOUSES.

The deformity and unseemly appearance of all the constructions that have been resorted to for hothouses, are proved by their being generally banished to a distance from the mansion of the owner, and hid in the kitchen-garden; and the attempts hitherto made to remove the defects by stone piers, parapet walls, and other architectural forms, have only tended to defeat the object of the structure by obstructing a portion of the light.—A new system has just been offered to horticulturists by Mr. Loudon, who has been long known as a professional rural architect, which seems to promise considerable improvement in this department. He has invented a solid wrought-iron sash bar not only applicable to all the purposes to which wooden frames have been hitherto applied for framing glass; but, being capable of receiving any form by bending, capable of being made into the form of domes and other elegant structures, fitted to ornament, as appendages, the most elegant mansions. He has, as the simplest way of showing their advantages, erected a variety of these structures on his premises at Bayswater.

FLUAT OF LIME.

It is remarked in a late Number of Thomson's Journal, that fluor spar occurs very rarely in Scotland. We are informed that it has been observed by Dr. MacCulloch, on more occasions than one, in the granite of Aberdeenshire, occupying small veins and unaccompanied by any other substance: in these cases it was always of a purple colour, but not crystallized. The same mineralogist

logist has also found it in Banffshire, crystallized, of a white colour, and occupying cavities in a granite vein. It has further occurred to him in the gneiss of Sutherland near the eastern extremity of Loch Shin. It is here also of a purple colour, and is often diffused in such a manner among the other ingredients of the rock, as in some measure to form an integrant part of its general structure: in other instances it is accumulated in lumps among its laminæ.

ZIRCON.

This mineral has, we understand, been discovered by Dr. MacCulloch in Sutherland. It occurs in a compound rock formed of copper-coloured mica, hornblende, and felspar.

This rock forms one of the occasional beds in the gneiss, and bears a resemblance in its composition to the zircon syenite of the north of Europe; the crystals are a quarter of an inch in length and well defined, and their colour is an obscure crimson approaching to that of cinnamon.

OPHTHALMIA.

Sir William Adams having had the honour to be nominated by His Majesty's Government to superintend that part of York Hospital, Chelsea, which has been appropriated to the reception of the blind pensioners belonging to the army, navy, and artillery, feels it a duty fully to lay open to the profession at large his new modes of treating them. This duty is suggested as well by the peculiar confidence which has been reposed in him, as by the high sanction thus conferred upon his improvements in ophthalmic surgery. He therefore freely invites all medical practitioners and students who are interested in the advancement of this branch of surgery, to attend his operations at York Hospital, which for their convenience will be performed in future on Tuesdays and Fridays, between the hours of seven and nine in the morning.

To remove all doubt or misconception with regard to Sir William Adams's practice, he proposes on each of these days to give a description of the nature of one of the diseases to be operated upon—the general modes of performing the operation—his peculiar mode—and his reasons for deviating from the usual practice, where such deviation has been found necessary.

The records kept of each case, from the patient's admission into the hospital to his final discharge, will be open at the periods already mentioned, for the inspection of such gentlemen as attend; so that the profession will be enabled fairly to appreciate the character of the *new* as compared with the *old* modes of practice.

It is expected that from *fifteen hundred* to *two thousand patients*

tients will successively be placed under the care of Sir William Adams in this Institution.

26, Albemarle-street, March 10, 1818.

DRY ROT.

The Eden sloop of war (new), which was lately sunk in Hamoaze, to endeavour to cure her of the dry rot, has been risen, commissioned, and taken into dock. On opening her, she has been found defective in every part, and must undergo a thorough repair. The Topaze frigate, also ordered for commission, which was repaired not long since, is found to be in the same state. The Dartmouth frigate, built at Dartmouth, three years old, never at sea, is also undergoing a complete repair. Not a ship is taken into dock but is found to be nearly rotten. The very best ships do not average more than twelve years existence. The San Domingo, 74, was ripped up (four years old) at Portsmouth. The Queen Charlotte, 110, was built at Woolwich, sent round to Plymouth, found rotten, and underwent a thorough repair; she was also several months under the care of Dr. Lukin, an Admiralty chemist, who received 5000*l.* for his ineffectual labours to stop the progress of vegetation in the ship. After a short cruise, the Queen Charlotte was laid up at Portsmouth, where she remains in a very defective state.

NEW OPINION IN REGARD TO POMPEII AND HERCULANEUM.

It is, at present, the general belief that the two celebrated cities of Pompeii and Herculaneum were overwhelmed and destroyed by an eruption of Vesuvius in the year 79. It is now, however, maintained, that this was not the case. Pompeii is said to be covered by a bed of lapillo, of the same nature as that we observe daily forming by the agency of water on the shore at Naples; while Herculaneum is covered by a series of strata, altogether forming a mass sixty feet thick, of a tuff having the characters of those tuffs formed by water. From the facts just stated, it is conjectured that the cities were destroyed by a rising of the waters, which deposited over them the stratified rocks, and not by matter thrown from Vesuvius. It is also said, that no eruption of Vesuvius took place in the year 79.

MANUSCRIPTS OF HERCULANEUM.

A letter from Naples says—"Among the manuscripts discovered at Herculaneum, there is a copy of Justin, and one of Aulus Gellius, in such a state of preservation that the persons appointed to decipher these manuscripts are able to read them almost without any difficulty. This discovery is the more valuable, on

account

account of the alterations that are known to have been made in the texts of these two authors; and because the eighth book of the *Noctes Atticæ* of Aulus Gellius, which was lost, is now recovered."

LIST OF PATENTS FOR NEW INVENTIONS.

To Alexander Haliburton, of Haigh Iron-work, near Wigan, Lancashire, for certain improvements in steam-engines and boilers.—2 months allowed to specify. Dated 27th Feb. 1818.

To Joshua Routledge, of Bolton-le-Moor, in the county of Lancaster, for an improvement or improvements upon the rotative steam-engine.—27th Feb.—6 months.

To John Sutherland, of Liverpool, and also of No. 99, Houndsditch, London, for various improvements in the construction of an apparatus for the purifying of liquids.—7th March.—2 months.

To Thomas Heppenstall, of Doncaster, Yorkshire, for an improvement upon the engine or machine for cutting or reducing into what is called chaff, different articles as dry fodder for horses and cattle.—7th March.—2 months.

To George Wyke, of Bath, and William Sampson, of Bristol, for certain improvements on pumps, which improvements are applicable to machinery of various descriptions.—March.—6 months.

To John Read, of Tipton, Staffordshire, and William Howell, of Wednesbury, in the same county, for their new system of working and getting the maine or thick mine of coal.—14th March.—2 months.

To Richard Penn, of Richmond Hill, Surrey, for his improved mode of manufacturing ornamental wooden furniture, by the application of machinery.—14th March.—2 months.

To John Ashton, of Great Tower-street, London, and Thomas Gill, of Greek-street, Soho, in the county of Middlesex, for certain improvements in or on instruments and apparatus for ascertaining the strength of spirituous liquors, and also the specific gravity of fluids and metals.—14th March.—6 months.

NEW COMET.

A new comet has been discovered in the constellation of the Swan. It was first observed on the 26th of December last, by M. Blanpain, at Marseilles, who has communicated to the Bureau of Longitude at Paris his observations upon it down to the 18th of January. The astronomers of Paris have been since constantly on the watch; but, in consequence of a very clouded state of the heavens, they have not yet been able to discern it. The movement of the comet, as described by M. Blanpain, is very

very slow, its right ascension increasing only seven minutes in twenty-four hours, and its declination, in the same time, not diminishing more than from thirty-three to thirty-five minutes. The observations of M. Blanpain embrace but a very small arc. M. Nicollet has, however, deduced from them a parabolic orbit, which, though only a mere approximation to correctness, may enable observers for some time to trace pretty exactly the course of the comet. According to his calculations, it would pass its point nearest to the sun on the 3d of March last, at fifteen minutes past eleven. Its perihelial distance will be equal to 1·12567, that of the earth to the sun being taken for unity.

The inclination of its orbit to the ecliptic..... = 88° 38′
Longitude of the ascending node............ = 68 5
Longitude of perihelium, calculated by the orbit = 187 32
Its heliocentric movement is direct.

As yet, there is nothing very interesting in its physical appearances. In the first days of January it resembled a small nebulous body, not of any determined form, and of a very feeble light. On the 18th it appeared sensibly augmented, both in size and brilliancy.

ASTRONOMICAL PHÆNOMENA, APRIL 1818.

D. H. M.		D. H. M.	
5.16.39	☾ ♀	20. 5.20	☉ enters ♉
7. .	♀ ☿, ♀ 39′ S. of ☿	20. .	☾ eclipsed visible
8.22.18	☾ A. ♉	20.18.54	☾ λ ♍
10. .	☾ in apogee	21. 8. 3	☾ α ♎
11. .	♃ o ♐, * 44′ N.	22. .	☾ in perigee
12.18.20	☾ ι ♊	22.12.12	☾ δ ♏
12.23. 2	☾ υ ♊	23. .	♃ 763 Mayer * 4′ S.
13.14.34	☾ ψ^{2} ♋	25. 1.26	☾ φ ♐
15.20.54	☾ η ♌	25. 9.20	☾ λ ♐
17.21.46	☾ ${}^{s}\nu$ ♍	27.22.14	☾ ε ♑
18.22.56	☾ γ ♍		
19.20.19	☾ ♍		

PHYSICAL PHÆNOMENA.

The storm of the 23d of February, from the effects of which our shores were exempted, spread its ravages over the greatest part of the continent. At Turin, it was attended with two shocks of an earthquake. Genoa, Savona, Alanco, and San Remo, were thrown into the greatest consternation for two days by repeated concussions, and several houses were partly demolished at Alazzes, but happily no lives were lost. At Antibes, in Provence, the whole day (the 23d of February) had been very tempestuous.—About four minutes past seven in the evening, a most tremendous rush of wind took place, and was followed by an instantaneous calm.

calm. A dull subterranean noise was heard, the sea suddenly dashed against the rocks, and in the space of three seconds three oscillations of the earth were felt in a direction from the S.E. to the N.W. The wind then rose again, and all the violence of the storm revived. At twelve o'clock a fresh concussion was experienced: and at a quarter past eleven the next morning a fourth, which was also preceded by the same deep and solemn rumbling. Before seven o'clock on the morning of the 25th, a fresh phænomenon presented itself; a parhelion was distinctly observed north of the rising sun; but the earliness of its appearance prevented its being generally noticed, and adding to the terrors of the people. The shocks were felt throughout all Provence, where no earthquake had been experienced for eleven years.

Letters from the Tyrol announce that the Glacier of Ortler in the vicinty of Chiavenha has increased this winter in a most extraordinary manner, notwithstanding the general mildness of the season. From the depths of the ice, incessant and tremendous roarings are heard. The Suldenbach stream, which formerly issued from this glacier, has been dried up ever since Michaelmas 1817, and great apprehensions are entertained for the neighbouring countries, should the heats of summer reopen a passage to the waters which seem to have collected within the bosom of this immense mass of ice. Similar phænomena have been observed in the glacier of the valley of Naudersberg.

On Saturday the 7th of March a water-spout burst at Stenbury, near Whitwell, in the Isle of Wight, which did considerable injury. It was preceded by a violently agitated atmosphere, the noise of which, for half an hour, resembled a roar the most dismal and appalling. When the cloud poured forth its contents, it seemed to the inhabitants of Stenbury farm as though the flood-gates of the sea had broken, and their destruction was inevitable: the water rolled down the hill in such irresistible torrents, that it beat down a lofty wall, flooded all the lower apartments of the farm, and set the cattle loose among the streams—the affrighted inhabitants seeking shelter, with their children, in the upper rooms. The terror and painful feelings are indescribable.

Extraordinary Fall of Rain. On the 21st of October 1817 (which was the day the hurricane commenced in the West Indies) at the island of Grenada, with the wind west, and the barometer standing at 29·40, eight inches of rain fell in twenty-one hours; and the rivers rose thirty feet above their usual level. From the 20th of October to the 20th of November, seventeen inches of rain fell.

Meteoro-

Meteorological Journal kept at Walthamstow, Essex, from February 15 *to March* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between *Noon* and Two P.M.]

Date.	Therm.	Barom.	Wind.
February			
15	29 40	29·87	E.—White frost, and very clear; very fine day; cloudy; afterwards rain.
16	42 51	29·87	E—S.—Clear, fine-coloured *cirrostratus;* at eight, hazy; fine day; star- and moon-light.
17	41 51	29·98	SE.—Gray morn; very fine day; *cirrostratus* and clear; clear night and windy.
18	44 51	29·98	S.—Cloudy early; small rain after 9 A.M.; showers, and wind; rainy evening, and very windy.
19	40 53	30·00	SW.—Clear high; hazy low; sun and *cirrostratus;* very fine day till after 3 P.M.; stormy, wind and rain.
20	31 43	30·00	W by S—SW.—White frost; showers and sun; star-light; some slight showers.
21	43 47	29·78	SW.—Cloudy, and windy; sun and wind; great showers and sun; cloudy. Full moon.
22	39 39	29·33	SW—N.—Cloudy; rain and *stratus*; at noon snowing; at 10 P.M. *deep* snow; still snowing; after 11 snow ceased.
23	30 40	29·76	NW.—Beautiful sun-rise; clear; deep snow five inches thick; fine sun at noon; rapid thaw; lumps of ice and snow fell from the trees; fine sun and wind; rainy evening.
24	35 42	29·50	W.—Clear, and wind; some snow still remains; very fine day; sun, and wind; night dark, and rainy and windy.
25	48 53	29·54	NW.—Windy, and *cirrostratus*; very fine day; night dark and windy.
26	39 41	29·40	NW.—Showers and wind; after 9 A.M. a great snow shower; 1 P.M. sun and wind; 2½ P.M. great hail-storm; bright star-light.
27	36 48	29·50	S.—Very rainy and calm; rain ceased at noon; showers, and sun after; stars and windy. Moon last quarter.
28	37 48	29·60	W.—Clear, and *cirrostratus*, and wind; a cloudy windy day; rainy night.

March

Date.	Therm.	Barom.	Wind.
March			
1	38 47	29·50	W.—Clear and wind; showers, sun, great wind; great storms; bright star-light night.
2	35 48	29·60	SW.—Clear, and *cirrocumuli*; hazy after 8 A.M. and soon after rainy; some sun after 4 P.M.; star-light.
3	34 52	29·71	SW—S.—Clear high; hazy low; very fine day; some slight rain; night dark and windy.
4	34 47	29·27	SW.—Clear high; hazy low; very fine day; night violently stormy; wind and rain; lightning about midnight; then stars appeared, and *aurora borealis*.
5	42 42	29·90	SW.—Some wind; clear high, and hazy low; great shower at 10 A.M.; sun and shower; fine after 1 P.M.; star-light.
6	39 47	29·20	SW.—Clear high; hazy low; showers, sun, and wind; star-light. New moon.
7	41 45	29·30	S.—Rainy morn; very rainy day; stormy and windy; evening great showers, and stars alternate; bright star-light at 1 A.M.; very clear.
8	39 48	28·90	SW—NW.—Clear, and *cirrus*; and wind; showers and sun; the showers *not* violent; a dark night.
9	33 44	29·34	NW.—Clear, and *cirrostratus*, and sun; fine day; sun, and *cumuli*; snowing and windy.
10	50	29·30	SW.—*Cirrostratus*, clear and windy; *cumuli*, and clear; clear moon- and star-light.
11	35 46	29·40	W.—Clear and windy; windy, clear and *cumuli*; fine day; calm at 5 P.M.; night windy again; rain and snow.
12	33 47	28·80	W.—Snow on ground and trees; clear and windy; wind and *cumuli*; storms after 4 P.M.; 10 P.M. *cumuli*, moon- and stars.
13	33 43	29·55	N.—*Cirrostratus*; orange sun-rise; *calm*; showers, sun and wind; clear star-light. Moon first quarter.
14	36 47	29·80	W.S.—White frost; clear above; hazy low; fine day; some rain after 3 P.M.; cloudy night; some stars.
15	42 47	29·50	SE.—Windy, and cloudy; gray day; night rainy and windy.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Feb. 16	11	43·	30·01	Cloudy
17	12	51·	30·07	Ditto
18	13	50·	29·92	Rain
19	14	47·5	29·99	Very fine
20	15	42·5	30·03	Cloudy
21	full	45·	29·44	Ditto
22	17	43·	29·30	Ditto
23	18	44·	29·83	Very fine—rain in the evening.
24	19	43·	29·86	Cloudy
25	20	47·	29·57	Ditto
26	21	43·	29·65	Very fine
27	22	49·	29·35	Cloudy—snow A.M.
28	23	45·	29·65	Ditto
Mar. 1	24	47·	29·10	Stormy
2	25	48·	29·64	Fine
3	26	47·	29·69	Cloudy
4	27	45·	29·41	Fine—a hurricane at night [P.M.
5	28	31·	28·91	Rain and hail—heavy fall of snow
6	29	49·	29·37	Fine—sharp frost with snow at
7	new	47·	28·90	Rain [night
8	1	47·	29·03	Fine—frost
9	2	41·5	29·40	Ditto ditto—with snow at night
10	3	39·	29·35	Ditto ditto ditto
11	4	42·	29·55	Ditto ditto at night
12	5	41·	28·94	Cloudy—heavy fall of snow A.M. [and frost at night
13	6	40·	29·66	Ditto—frost at night
14	7	44·	29·91	Ditto

The first twelve days in March have been exceedingly tempestuous. Wind S.E—E.S.E.—N.W and N.N.W.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For March, 1818.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 27	40	50	40	29·25	0	Showery
28	40	47	43	·51	24	Cloudy
Mar. 1	40	52	44	·52	21	Showery
2	40	48	46	·49	20	Stormy
3	42	52	44	·50	22	Fair
4	36	51	46	·20	20	Cloudy in the evening the Bar. fell to 28·55, and a violent storm succeeded
5	44	52	39	28·92	27	Fair
6	40	51	38	29·40	30	Fair
7	44	49	39	28·72	0	Stormy
8	42	46	38	29·00	0	Showery
9	37	45	33	·36	30	Fair
10	33	42	32	·25	0	Showers of sleet
11	36	48	38	·40	28	Cloudy
12	34	47	37	28·85	22	Fair, a storm in the night
13	36	50	40	29·49	27	Fair
14	36	49	40	·80	24	Cloudy
15	37	51	44	·35	20	Cloudy
16	39	47	41	·60	0	Showery
17	47	52	44	·82	26	Cloudy
18	45	54	47	·98	25	Cloudy
19	47	53	47	·88	27	Cloudy
20	47	54	39	·75	33	Fair
21	40	50	41	·85	19	Showery
22	40	47	49	·60	0	Stormy
23	47	50	40	·40	0	Stormy
24	40	49	38	·65	14	Hail storms
25	42	50	40	·51	22	Stormy
26	40	43	38	·15	0	Rain

N.B. The Barometer's height is taken at one o'clock.

XLI. *Account of Antiquarian Researches in Egypt by M.* Belzoni, *charged by the British Government to make Collections for the British Museum. Contained in a Letter from M.* Belzoni *to M.* Visconti *.

Cairo, Jan. 9, 1818.

. . . . I have arrived from Upper Egypt, and am preparing to return to Nubia for the third time.

In my first journey to Thebes, in 1816, I had succeeded in embarking on the Nile the upper part of the famous statue of Memnon. This grand wreck, which has lain for so many centuries amidst the ruins of the palace destroyed by Cambyses, is now on its way to the British Museum. It is a colossal bust, of a single block of granite, ten feet in height from the breast to the top of the head, and twelve tons in weight. Other travellers before me had conceived the design of transporting it to Europe, and renounced it only from not conceiving the means of effecting it. The great difficulty was in moving such a mass for the space of two miles, until its arrival at the Nile, whereby alone it could be conveyed to Alexandria. I succeeded in effecting it, without the aid of any machine, by the sole power of the arms of some Arabs, however ill qualified this people, now sunk into the indolence of savage life, may be for such rude labours. As such, it has been the work of six months.

From Thebes I went up towards Nubia, to examine the Great Temple of Ybsambul, which is buried more than double its height in the sands, near the second Cataract. There I found the inhabitants very ill disposed towards my projects, and from whom I prepared to encounter some difficulties. However, the season being too advanced, was my sole motive in deferring this enterprise to another time.

In the mean time I returned to Thebes, where I occupied myself in new searches at the Temple of Karnack. There I found, several feet under ground, a range of sphinxes, surrounded by a wall. These sphinxes, with heads of lions on the busts of women, are of black granite, of the usual size, and for the most part of beautiful execution. There was in the same place a statue of Jupiter Ammon, in white marble. It was not until my second journey, in 1817, that I discovered the head of a Colossus much greater than that of Memnon. This head of granite, and of a single block, is by itself ten feet from the neck to the top of the mitre, with which it is crowned. Nothing can be in better preservation. The polish is still as beautiful as if it had but just come from the hands of the statuary.

After this, I again took the road to Nubia, where some severe trials awaited me. The people of this country are quite savages,

* From the *Moniteur.*

without any idea of hospitality. They refused us things the most necessary; entreaties and promises had no effect on them. We were reduced to live upon Turkish corn soaked in water. At length, by dint of patience and courage, after twenty-two days persevering labour, I had the joy of finding myself in the Temple of Ybsambul, where no European has ever before entered, and which presents the greatest excavation in Nubia or in Egypt, if we except the tombs which I have since discovered at Thebes.

The Temple of Ybsambul is 152 feet long, and contains fourteen apartments and an immense court, where we discovered eight colossal figures thirty feet high. The columns and the walls are covered with hieroglyphics and figures very well preserved. This temple has then been spared by Cambyses, and the other ravagers who came after him. I brought some antiquities from thence—two lions with the heads of vultures, and a small statue of Jupiter Ammon.

On returning again to Thebes, I applied myself once more to discover what has been, from time immemorial, the object of discovery for all travellers of every nation—I mean the tombs of the kings of Egypt.

It is known that, independent of those tombs which are open, there existed several under ground, but no person has yet discovered in what place. By means of observations on the situation of Thebes, I at length found the index that should lead me on the way. After various excavations, I succeeded in discovering six of these tombs, one of which is that of Apis, as it seems to be pointed out by the mummy of an ox found there. This mummy is filled with asphaltes. For the rest, nothing that I can say would enable you to conceive the grandeur and magnificence of this tomb.

This is undoubtedly the most curious and the most astonishing thing in Egypt, and which gives the highest idea of the labours of its ancient inhabitants. The interior, from one extremity to the other, is 309 feet, and contains a great number of chambers and corridors. The walls are entirely covered with hieroglyphics and bas-reliefs painted in fresco. The colours are of a brightness to which nothing within our knowledge is to be compared, and are so well preserved that they appear to have been but just laid on. But the most beautiful antiquity of this place, in the principal chamber, is a sarcophagus of a single piece of alabaster, nine feet seven inches long by three feet nine inches wide, within and without equally covered with hieroglyphics and carved figures. This large vessel has the sound of a silver bell, and the transparency of glass. There can be no doubt that when I shall have transported it to England, as I hope to do, it will be esteemed one of the most precious *morceaux* of our European Museums.

XLII. *On*

XLII. *On the Use of the common Thermometer as a Hygrometer, and Description of a Self-registering Hygrometer. In two Letters from* R. G. K——s*.

LETTER I.

On the Use of the common Thermometer as a Hygrometer.

Mr. Editor,—It is not my intention to enter into any long or minute detail of the numerous instruments that have been proposed for ascertaining the state of the atmosphere with regard to moisture, or to attempt deciding on the comparative merits of Saussure's hair, and De Luc's whalebone. I believe it may be safely affirmed, that a correct, at least a permanently correct, hygrometer, never can be constructed on the principle of any such contrivance, and for this obvious reason: however accurately the instrument may be originally made, it no sooner begins to operate than it begins to change, the alternate expansions and contractions of the substance producing necessarily, however lowly, some derangement in its natural texture. The contrivance itself may be extremely ingenious; but from the very nature of the materials employed, such hygrometers must be imperfect, in as much as they are subject to changes, the extent of which it is impossible exactly to appreciate. Now, is it not very strange, that after all the complaints that we have heard among meteorologists, and philosophers in general, about the want of a hygrometer on accurate principles, they should hesitate a single moment about adopting one as simple and accurate as it is elegant and philosophical? I allude to the differential thermometer of Professor Leslie, which the ingenious inventor has applied, among many other useful purposes, to that of measuring the relative dryness of the atmosphere, and which does so upon principles as fixed and determinate as those of the common thermometer. For the sake of such of your readers as may not be conversant with the subject, I shall give a short description of it nearly in the Professor's own words: "It consists of a thermometer tube, curved like the letter U, with a hollow ball at each extremity containing air, and holding an intermediate portion of sulphuric acid, tinged with carmine. When these balls are of the same temperature, the liquor will remain stationary; but if one of the balls be warmer than the other, the liquor, urged by the increased elasticity of the air, will descend proportionally on that side. To measure the difference of heat between the two balls, the whole interval between freezing and boiling water is

* From Blackwood's Edinburgh Magazine for July 1817.

divided

divided into a thousand equal parts. If one of the balls be covered with cambric or silk, and wetted with pure water, the instrument forms a complete hygrometer; for it will mark, by the descent of the column in the opposite stem, the constant diminution of temperature which is caused by evaporation from that humid surface, and it must consequently express the relative dryness of the ambient air." It is hardly necessary to observe, that hygrometers constructed on this principle must always indicate the same dryness, in the same circumstances, and may therefore be as readily compared with one another as thermometers themselves. But my object is not so much to discuss the merits of the instrument itself, as to show that the common thermometer may be used in its stead; and that though it may not possess the same degree of delicacy, it is sufficiently accurate for all the ordinary purposes of meteorology. Let two spirit-of-wine thermometers be chosen, as nearly of the same size as possible, and graduated so as exactly to coincide at different temperatures. Let the bulb of one of them be covered with blue or purple silk while the other remains naked, and let them be suspended at about the distance of two inches from each other. Let the covered bulb be then wetted with pure water, and the two thermometers will very soon indicate a difference of temperature; the wetted one, from the cold produced by the evaporation, sinking below the other, more or less, according to the rapidity of the evaporation; that is, according as the air is more or less dry. If the thermometers be graduated according to Fahrenheit's scale, each degree of difference must be multiplied by $5\frac{1}{2}$, and the product will express the degrees of the Professor's hygrometer nearly; or if they are graduated according to the centigrade scale, the degrees of difference, multiplied by 10, will give the hygrometric degrees exactly. From numerous comparative observations, I am able to say, that the average dryness of a month, as indicated by the thermometers, will not differ from that indicated by the hygrometer more than two hygrometric degrees, a quantity that may be safely overlooked in a series of observations which do not admit of extreme accuracy. It may perhaps look like presumption, but I cannot help observing, that the thermometers appear to me better calculated to give the mean dryness of the air than the hygrometer itself, as the latter, from its extreme delicacy, is sometimes affected by a sudden gust of wind at the moment of observation, so as to rise two or three degrees. There is, however, one obvious advantage which the thermometers possess over the hygrometer, and that is, their showing not only the difference between the temperatures of the two bulbs, which is all that the hygrometer shows, but also the actual temperature of both the wet and dry surface, a circumstance

cumstance necessary to be taken into the account, in estimating the absolute quantity of water held in solution by the atmosphere at the moment. I hope it will not be supposed that these remarks are intended to throw any obstacles in the way of a more extended and general use of an instrument which is likely to be of such essential service to science, and which has already done so much honour to the ingenious inventor. My object is to press upon those who may not have had an opportunity of making any observations with the hygrometer, but who are familiar with the use of the thermometer, not to neglect the means which they possess of collecting facts on a branch of science which is still in its infancy, and which never can make any advancement, but by the patient application of the inductive philosophy.

I remain, sir, yours respectfully,

K——s, July 2, 1817. G.

LETTER II.

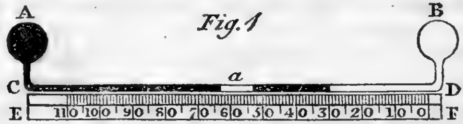
Description of a Self-registering Hygrometer.

Mr. Editor,—I have already endeavoured, more than once, to direct the attention of your readers to the subject of hygrometry, as a branch of science naturally interesting, and which has of late become still more so, from the ingenious discoveries of Leslie and Anderson. To the former of these gentlemen we are indebted for the best, and indeed the only, philosophical instrument hitherto employed for ascertaining the state of the atmosphere with regard to moisture; and to the researches of the latter we owe some beautiful theorems for its practical application to the science of meteorology*. Simple, however, and philosophical as that instrument is, it appears to me to be still capable of improvement. In its present form it can be employed only in finding the hygrometric state of the atmosphere at the moment of observation; nor have any attempts been made, as far as I know, to construct it so as to mark the extremes of dryness and moisture, in the absence of the observer. I formerly thought that the instrument might easily be made to register the greatest dryness, but that it would be difficult to construct it so as to mark the greatest degree of moisture. It has since occurred to me, however, that the latter object may be as easily accomplished as the former, and both on the same principle with the self-registering thermometer commonly in use. For a description of the instrument, as originally constructed by Professor Leslie, I refer your readers to that author's treatise on heat and moisture, and to the article Hygrometry in the Edinburgh Encyclopædia. In

* The researches of Mr. Anderson were published, for the first time, in the Edinburgh Encyclopædia, conducted by Dr. Brewster.

the modification of it which I am now to propose, the principle is the same, though the form is somewhat different.

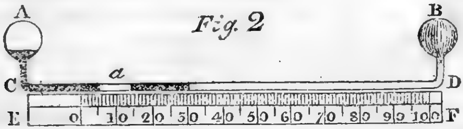
Fig. 1



C D (fig. 1.) is a tube, such as is commonly used for constructing a self-registering thermometer, bent upwards at C and D, and terminating in a bulb A. Into this bulb is introduced a portion of sulphuric acid, sufficient to fill the tube and a small part of the bulb; and along with the acid a small bit of glass *a*, of such a diameter as to move easily in the tube when the instrument is inverted. To the extremity, D, another bulb, B, is attached; and the air contained in both bulbs is so adjusted, that when they are at the same temperature, the liquid stands at a point near the extremity D, and which is marked 0 on the attached scale E F. If the temperature of the bulb B be now increased, or, which is the same thing, if that of A be diminished, the portion of air in the upper part of the bulb will contract, while that contained in B will expand in the same proportion, and the liquid will of course be forced from D towards C. In the scale adopted by Professor Leslie, the distance between the freezing and boiling points is divided into a thousand equal parts, and is hence denominated the *millesimal*. In this climate, however, a tenth part of that scale, or one hundred degrees, will embrace the greatest range of the instrument; and that point may be thus obtained:—Let the bulb A be surrounded with melting snow, while the instrument is placed in an atmosphere of the temperature 50, and let the point be marked at which the liquid becomes stationary. The distance between zero and this point will then be 18 degrees of Fahr. or 100 of the millesimal scale; and that distance being divided into an hundred equal parts, will give the graduation required. To prepare the instrument for observation, it only remains to cover the bulb A with silk, and moisten it, taking care that the two bulbs be as nearly as possible of the same colour. The index, or small bit of glass, *a*, is then to be brought to the extremity of the liquid, by depressing the extremity D, and the instrument to be exposed in a horizontal position. As the evaporation from the surface of the bulb A goes on, the air within contracts, from the depression of temperature produced by the evaporation; and the liquid is forced from D towards C by the elasticity of the air in B, carrying with it the index *a*. When the evaporation has reached its maximum, the liquid, as well as the index, becomes stationary; but

but should the process of evaporation diminish, the liquid will again move towards D, while the index is left behind, thus marking *the maximum of dryness in the absence of the observer.*

Fig. 2



To find the greatest degree of moisture, another instrument is to be employed, which is represented in fig. 2. The only difference between this and the former is, that the air in the two bulbs is to be so adjusted, that when they are at the same temperature, the liquid may stand near the extremity C, the distance between C and zero being a little more than the length of the index *a*; and the bulb B is to be covered as A was in the former. The scale is graduated as before. When the instrument is adjusted and exposed, evaporation goes on from the surface of B; and the air within being thereby contracted, the liquid moves towards D, continuing to do so till the maximum effect is produced. When the evaporation diminishes, the liquid is again forced backwards towards C, till it arrives at the index *a*; and should the evaporating force still continue to diminish, the index itself is then carried towards zero, till the evaporation be at its minimum. The liquid then becomes stationary; and though it should afterwards mount higher, in consequence of an increased evaporation, still the index remains at the lowest point to which the liquid had sunk, thus marking *the minimum of dryness in the absence of the observer.*

Your readers will observe, that in the modification which I have now proposed of the original hygrometer of Professor Leslie, no new principle has been introduced; the contrivance for marking the extremes being the same as that which was invented by Dr. Rutherford, and which has been long employed in constructing a minimum thermometer. I can lay no claim, therefore, to the honour of a discovery, but I hope I have some little to that of an improvement. It is well known to meteorologists, that observations of temperature were comparatively of little use till the invention of self-registering thermometers; and why may we not be allowed to hope, that the use of a self-registering hygrometer will hereafter bring to light some important facts regarding the laws which regulate the distribution of moisture in the atmosphere?

One word as to the construction of the instrument, and I have done. In the first, and indeed the only attempt that I have yet

 had

had time to make, I succeeded in constructing a small hygrometer on the principle of fig. 1.; and though the range is too limited to give the necessary degree of accuracy, it is sufficient to convince me that the construction is not only practicable, but, to those who are dexterous in the use of the blowpipe, extremely easy. I remain, sir, your obedient servant,

Feb. 27, 1818. R. G.

In the above, as well as in the original form of the hygrometer, the covered bulb may be kept continually moist with water, conveyed to it by filaments of floss silk from an adjoining vessel.

XLIII. *Report of the Committee appointed to superintend the Experiments of Dr.* Sickler, *for the Purpose of proving the Efficacy of a Method proposed by him, for unrolling and deciphering the Herculaneum Manuscripts.*

Previously to a detail of the proceedings of the Committee charged with the superintendence of Dr. Sickler's experiments, it appears proper, for a full elucidation thereof, to recapitulate the circumstances under which the views of His Majesty's Government were directed to the interesting subject of the Herculaneum rolls. It is well known that, from the first discovery of these remains of ancient literature among the ruins of Herculaneum, a more efficacious method of ascertaining their contents has been a desideratum, the attainment of which has occupied the attention of men of learning and science, in almost every country of Europe; and much has already been done, though by a very laborious and protracted operation, at Naples, within the last thirty years; during a part of which period, some persons were employed there by His Royal Highness the Prince Regent, in concert with the establishment formed for the purpose by His Sicilian Majesty.

Soon after the re-establishment of peace, and the return of His Sicilian Majesty to Naples, and whilst steps were taking at Paris for applying the Neapolitan method to some Herculaneum manuscripts which were in that capital, it came to the knowledge of a member of this Committee, that Dr. Sickler, a professor of eminence at Hildburghausen, had made some successful experiments, likely to lead to the accomplishment of the object, by a more easy and expeditious mode of proceeding.

A correspondence which has been laid before the Committee, took place with Dr. Sickler; in the course of which, he expressed his readiness to come, upon certain conditions, to this country, in order to pursue his experiments upon the rolls which his Sicilian Majesty had recently presented to His Royal Highness the Prince

Prince Regent: referring, at the same time, for a declared opinion upon this method, to a Report of the Royal Society of Arts at Göttingen, made on the 9th of November 1814, and to the testimony of Professor Millin, of the Royal Library at Paris, respecting his character and literary acquirements.

The Report of the Royal Society of Göttingen, which was founded upon the evidence of an experiment upon one of the manuscripts at Naples (a detached column of which Dr. Sickler had laid before the Society), as well as upon experiments made in the presence of a committee of that learned body, upon rolls of paper, prepared for that purpose, seemed to afford considerable grounds for a belief, that the material difficulties which had hitherto occurred could be subdued by the application of a liquid, discovered by Dr. Sickler, and by a system of manipulation altogether new. Under these circumstances the following propositions, grounded upon a prospectus transmitted by Dr. Sickler in the month of January last, were authorized by His Majesty's Government to be made to that gentleman:—

1. That Dr. Sickler should come to England, and reside here as long as might be thought necessary for bringing his projects to a decided result.

2. That his expenses to England with his family should be paid, as well as of his residence while in England, either in London or Oxford.

3. That either before he left the continent, or within one month after his arrival, he should convince a Committee of gentlemen, named for the purpose*, that his scheme was available, and one for which the Government would be warranted in incurring further expense.

4. That in case of failure, in so far satisfying the Committee, Dr. Sickler should clearly understand that he was to expect from the Government no further pecuniary aid than the expenses of his return to Hildburghausen:—And

5. That in the event of the Committee making a report which should authorize the Government to proceed, Dr. Sickler should, in that case, receive one hundred pounds for every manuscript which he should succeed in unrolling to the satisfaction of the Committee; and that when the Committee, who must, in the first instance, have been put in possession of the whole of the secret, whether in preparation or manipulation, should judge that

* The Earl of Aberdeen, President of the Society of Antiquaries; the Lord Grenville, Chancellor of the University of Oxford; the Lord Colchester; the Right Honourable Sir Joseph Banks, Bart. G.C.B. President of the Royal Society; Sir Thomas Tyrwhitt, Knt.; Sir Humphry Davy, Knt. Fellow of the Royal Society; the Rev. Charles Burney, D.D.; William Hamilton, Esq. His Majesty's Under Secretary of State for Foreign Affairs.

there

there would no longer be any occasion for his assistance or presence, Dr. Sickler should receive from their hands such a sum, as, together with what he should have successively received for each unrolled manuscript, should make up the sum of two thousand five hundred pounds, the expenses of his return home, and an engagement to be allowed one hundred pounds per annum for the remainder of his life.

These propositions being communicated to Dr. Sickler, by a member of this Committee, who repaired to Hildburghausen for that purpose, the same were accepted with the following modifications, detailed in a memoir, dated the 19th of May last, a copy of which has been laid before the Committee:—

1. That his absence from Hildburghausen should be limited to a period not exceeding six months.

2. That an allowance should be granted to him, previously to his departure, for the expenses of his outfit:—And

3. That a specific sum should be named for the expenses of his return to his own country, in the event of the failure of his experiments:—All which being acceded to, Dr. Sickler left Hildburghausen with his family, arrived in London on the 12th of June, and soon after commenced his preparations for the undertaking.

The Committee having detailed the circumstances under which the propositions of Dr. Sickler originated, and referred to the testimonials laid before them, concerning his method of operation, his character and literary reputation, as well as the principles upon which their Committee had been instituted, proceed to state that Dr. Sickler having completed his preparations, they held their first meeting in Black Rod's apartments, in the House of Lords, on the 27th of June last, when the following members were present, viz. the Lord Colchester, Sir Thomas Tyrwhitt, Sir Humphry Davy, the late Rev. Dr. Burney, and William Hamilton, Esq. At this meeting Dr. Sickler gave an explanation of the machine used by him in carrying on his operations; after which, in conformity to a stipulation in the memoir before referred to, he selected, from among the twelve manuscripts recently received from Naples, a roll, which was delivered into his hands for the purpose of commencing his experiments: and it was then notified to him that the Committee, at their subsequent meetings, would be ready to receive any written or other communication, which he might have to make, in the progress of his labours.

At their second meeting, on the 3d of July, Dr. Sickler presented two memoirs, the first containing his observations upon the roll, which had been delivered to him, and the second upon the commencement of his operations; both which were entered on their minutes; and the Committee adjourned to the following

day;

day; when Dr. Sickler submitted to their inspection the process of his method.

At the fourth meeting of the Committee, Dr. Sickler produced several pieces which he had detached from the roll, but upon which no characters were discoverable. A second and a third machine were, in consequence of a suggestion of the Doctor, directed to be prepared; in order that he might employ himself upon two or more rolls at the same time.

The further result of Dr. Sickler's operations was submitted to the Committee, at their fifth meeting, on the 18th of July, when it appeared that of the first roll, the pieces taken off, continued to afford no trace of characters, but a second roll, which the Doctor had selected at the last meeting, appeared by his memoir presented thereon, to be in a much better state of preservation than the preceding, both with respect to its carbonization and to the texture of the papyrus—the pieces detached therefrom containing several Greek characters, and some entire words easily made out; from which circumstance, and the extreme fineness of the papyrus, as well as the perfection of the written characters, Dr. Sickler expressed his opinion that a further operation upon this roll might be productive of a more satisfactory result. At this meeting the Committee, considering the unfavourable colour and texture of the first roll, directed Dr. Sickler to discontinue his operations thereon, and he proceeded to select a third roll, which, in his opinion, from its external appearance, most resembled that which had already exhibited written characters.

At their subsequent meetings, on the 21st and 24th of July, Dr. Sickler reported the progress of his labours; but the hopes which had been entertained, of the further success of his operations upon the second roll, having been disappointed, by the total absence of characters on the pieces which had recently been detached, together with the unfavourable appearance of the interior of this roll; as well as the injured state of the third roll which had been placed in his hands: The Committee, considering the whole, in connexion with the observations of Dr. Sickler in his several memoirs down to this time, did not feel themselves in possession of sufficient evidence upon which to found a report of the success of the method, within the stipulated period of one month after the commencement of the Doctor's experiments. It was, therefore, with a view to a more satisfactory result, thought proper that Dr. Sickler should continue his experiments upon the rolls then in his possession, as well as upon others which the Committee, or visiting members, should, from time to time, direct to be placed in his hands for that purpose; and he accordingly pursued his operations down to the 10th of October last;—the Com-

mittee

mittee having, in the mean time, been assisted, in the examination of the result of his labours, by the advice and opinions of Sir Charles Blagdon, Mr. Taylor Coombe of the British Museum, and of Sir William Drummond and Sir William A'Court; the two latter of which gentlemen, from their residence at Naples as His Majesty's ministers, had had ample opportunities of watching the operations which were carried on there, for the purpose of unrolling the Herculaneum manuscripts, and were also familiar with the external appearance of the rolls in the places where they are preserved.

The system of operation by Dr. Sickler may be considered under three distinct heads:—

1. As to the improvement of the machine made use of by him;
2. As to the liquid applied to the roll;—and,
3. As to his mode of manipulation.

With regard to the first, the Committee have been favoured with the observations, which they have recorded in their minutes, of Sir William A'Court; and it appears that the general principle of the machine made use of by Dr. Sickler is nearly similar to that in use at Naples, as far as respects the manner of placing the roll for operation, and the means of turning it for the purposes of examination. The material difference appears to consist in the process of detaching the layers of the papyrus; Dr. Sickler performing that operation with the hand, after attaching to the back of the part to be unrolled a piece of thin linen, or cotton cloth, and the Neapolitans by the rotatory movement of a piece of machinery, placed immediately over the roll, and connected with the lining of the part to be separated (which lining consists of narrow slips of goldbeaters' skin) by a number of silk threads, acting with equal and simultaneous force upon the whole surface.

Upon the second point, the Committee, in an early stage of their proceedings, viz. at their third meeting, on the 4th of July, judged it expedient that the liquid made use of, in order, as Dr. Sickler stated, as well to attach a lining to the surface of the roll, as to facilitate the separation of the layers, should be subject to the examination of their distinguished member, Sir Humphry Davy; whose opinion of its composition satisfied the Committee that no injury could accrue to the papyrus, from the use of such liquid: but Sir Humphry Davy, at the same time, expressed his conviction, that it did not possess any power to help the separation of the layers; a point which the Committee had been led to consider as the principal feature in this part of Dr. Sickler's discovery.

As to the mode of manipulation, the Committee have also been assisted, by the knowledge and experience of Sir William A'Court, in in forming their opinion thereon. The exterior of the Herculaneum rolls generally presenting an uneven surface, composed of different strata, or layers, the first object, with a view of obtaining entire and consecutive columns of the manuscript, must be to remove, in succession, these irregularities all around the roll; an operation which, considering the delicate nature and fragile quality of the papyrus, should be carried on with the greatest care and nicety. With respect to Dr. Sickler's method, it did not appear to Sir William A'Court calculated to accomplish this primary object; since his mode of indiscriminately covering the whole surface of the roll with the liquid preparation (by which the lining is attached to the papyrus) must, in the operation of taking off, necessarily produce a number of broken and interrupted layers, belonging to different *strata*, leaving a surface equally, at least, if not more, uneven than before. By this means it must follow, that, however the characters in the manuscript may be preserved, a part of the roll so taken off could not be expected to exhibit more than detached letters, or words, of as many columns as there are layers so separated.

The principal object in an undertaking of this nature being, after an even surface has been gained, to separate entire columns on single layers, in regular succession, the Committee, in the course of the experiments which have been made under their superintendence, have constantly directed their attention to the success of Dr. Sickler's system of operation in this respect. Previously, however, to a declaration of their opinion thereon, they consider it necessary, in justice to Dr. Sickler, to state that, after the result of his operations upon the four first rolls had been laid before them, and an attentive perusal of his observations thereon, they did not entertain any sanguine expectations that he would be enabled to produce any thing like a series of columns of the contents of any one of them; and that they were willing to attribute the total failure of his endeavours, in this particular, to the injured state of the rolls, and to the probable destruction of the characters, from the effect of damp or otherwise, as stated in Dr. Sickler's several memoirs; an opinion expressed by Sir William Drummond, as well as by Sir William A'Court, upon an examination of some of the rolls in the course of operation: but while they are ready to admit Dr. Sickler's observations, as to the deteriorated state of most of the seven rolls which were placed in his hands in the progress of his experiments, they nevertheless conceive, that the result of his labours affords them sufficient grounds for stating the following objections to his method of unrolling the Herculaneum manuscripts:

1st. The machine made use of by Dr. Sickler does not, in the opinion of the Committee, appear to be calculated to remove any

of

of the difficulties which have hitherto occurred in the system of unrolling these manuscripts.

2dly. The liquid, from the application of which the Committee were induced to hope that the separation of the layers of the papyrus would be considerably facilitated, does not, in the judgement of the Committee, appear to possess any effective power beyond that of acting as a glue for the lining of that part to be detached :—And,

3dly. That the mode of manipulation adopted by Dr. Sickler is too violent an operation to produce entire consecutive columns, or single layers, of the papyrus; and that his method of indiscriminately covering the surface of the roll with the lining which, being attached to the roll by the liquid preparation, brings off with it, in the process of detaching, the part so lined, is very imperfect; since, in raising the layers, it is scarcely possible to observe by the eye, whether one or more layer is about to separate from the mass; a part of the operation which, at Naples, is carried on with the greatest caution.

Upon a minute inspection of a great number of the pieces taken off by Dr. Sickler, on some of which the characters are preserved, the Committee observe, that there is scarcely an instance to be found where a single layer has been detached with a regular surface throughout; a circumstance which, in their opinion, seems decisive of the inefficacy of Dr. Sickler's method. Upon this point their judgement has been formed, more especially after an examination of the pieces detached from the second roll, of which Dr. Sickler pronounced an opinion, favourable as to its state of preservation, before referred to, upon which the written characters are very perfect; but which pieces appear to be composed of two or more layers adhering together.

Of seven rolls which have been placed in the hands of Dr. Sickler, in the progress of his experiments, and which have been more or less opened, the Committee have to observe, that only two have exhibited the smallest visible traces of letters on some of the columns, the rest presenting a rough brown surface, without characters; from which circumstance, the Committee are not without their apprehensions that, either by excess of zeal or want of caution, Dr. Sickler's mode of operation may, in some instances, have produced a separation of the intermediate or blank leaves which compose the papyrus; a point to which the Committee were induced to direct their attention, after an explanation of the preparation of the papyrus, with which they were favoured by Sir Charles Blagdon and Mr. Taylor Coombe, and an inspection of specimens of the Egyptian and Sicilian papyri, laid before them by the latter gentleman.

Upon the whole, after a scrupulous examination of the result of of Dr. Sickler's experiments, and an attentive consideration of the same, in connexion with the several memoirs, containing his observations, presented from time to time to the Committee, they regret to observe, that Dr. Sickler has totally failed in his endeavours to satisfy the Committee, that his method of unrolling the Herculaneum manuscripts is available, and such as can warrant them in recommending to His Majesty's Government a further perseverance therein.

In closing their Report upon this inquiry, the Committee must observe, that they had reason to be satisfied with the zeal and assiduity which Dr. Sickler continued to manifest throughout the whole of the operations in which he was engaged, under their inspection; and that, although by the letter of his original propositions, and the agreement ratified by him, previously to his departure for England, he could not consider himself entitled to any pecuniary aid beyond the expenses of his return to his own country; yet, inasmuch as he had been engaged in this country from the 12th of June to the 11th of October, they have thought proper to recommend that such a remuneration should be made to him, for the time which has been devoted to his experiments, as in the judgement of the Committee, from the information before them, would be sufficient to cover his actual loss of salary and emoluments, by his absence from his professional duties; and they fixed the amount of remuneration to two hundred pounds. This sum, together with one hundred and fifty pounds for the expenses of his return to Hildburghausen, conformably to the agreement before referred to, Dr. Sickler received, previously to his departure from England; the expenses of his residence in this country having from time to time been discharged, under the direction of the Committee, out of the sum which His Royal Highness the Prince Regent has been pleased to direct to be placed at their disposal for that purpose; the amount of which issue and expenditure is hereto annexed.

By order of the Committee,

London, Dec. 31, 1817. JAMES PULMAN, Secretary.

Statement of the Expenditure of the Committee charged with the Superintendence of Dr. SICKLER'S *Experiments for the Purpose of proving the Efficacy of a Method, proposed by him, for unrolling and deciphering the Herculaneum Manuscripts.*

Travelling expenses consequent upon the negotiation with Dr. Sickler's Journey to England with his family, including the sum allowed to him for his outfit £351 17 6

Charges

Charges for machines, instruments, and other contingencies, in the course of the experiments ..	69	11	0
Expenses for board, lodging, &c. of Dr. Sickler and family, from the 12th of June to the 26th of October (on which day he left London), including the hire of his servant	239	18	6
Sum allowed to Dr. Sickler in remuneration for the time which he devoted to his experiments ..	200	0	0
Sum allowed to Dr. Sickler to defray his travelling expenses to Hildburghausen	150	0	0
Allowance to the Secretary of the Committee ..	100	0	0
	£1,111	7	0

By order of the Committee,

London, Dec. 31, 1817. JAMES PULMAN, Secretary.

XLIV. *Anatomical Description of the Fir Tree.* By ROBERT McWILLIAM, *Architect and Surveyor**.

THE fir is a genus of the class *monœcia*, order *monadelphia*. It has male and female flowers on the same tree, and is propagated by seed.

After what has been said already of vegetation in general, it is not deemed necessary to enter again into this subject at any considerable length. The roots of the fir tree are hard and slender. They require a hard, dry, gravelly, or sandy soil; and a cold climate, not very liable to sudden changes of temperature. Hence it is, that the Scotch and other firs do not arrive at such perfection in this country, as in higher latitudes.

When in a favourable soil and climate, fir trees grow to the height of ninety or a hundred feet, and sometimes much higher.

The longitudinal divisions of this tree, for it appears to me doubtful whether they can be called vessels, are seemingly of two sorts; and when viewed at right angles to the radiates, they appear like a bunch of dressed flax or hemp, after being somewhat pressed, so as to render the fibres a little wrinkled. When viewed parallel to the radiates, they have the appearance of a web of the same materials, having four or five threads of the woof close together, as in muslin or cambric, and a space rather more than equal to this without any woof, similar to the space between handkerchiefs in the piece, where the warp only appears without any woof: and thus they are continued in alternate bars one

* From Essay on the Origin and Operation of the Dry Rot, with a View to its Prevention or Cure.

over

over another, from the extremities of the filaments of the roots to those of the branches.

The radiates lie along that part which looks as if it were woven; and in many cases they have the appearance of vessels of considerable dimensions: but this is only to be seen when they are filled with coloured matter, which is evidently turpentine. Where this is not visible, there is generally something like a very fine dew in miniature on the reticular part, standing in rows both longitudinally and horizontally. The radiates apparently perform the same office as in the oak and other trees. The sort of network likewise lies in the horizontal direction, and breaks the rectilineal perpendicular pressure of the juices contained in the longitudinal vessels; if such they be. At all events it prevents the juice of the tree from descending in a right line, whether contained in vessels or in the longitudinal cavities between the portions of fibrous matter not connected in a tubular form, yet sufficiently close together to support moisture on the same principle as that of capillary attraction in tubes. This principle of attraction, indeed, is not limited to vessels of any particular form, or to solids forming any vessels at all; for two plane surfaces of glass, or any other matter, being moistened, and placed sufficiently near to each other, without touching, will occasion water to ascend between them, as it would in a tubular vessel.

If these woven flax-like fibres be hollow tubes, it is probable, that they principally contain the more watery matter absorbed by the plant, before chemical union takes place; and they may be presumed to be formed of a film, similar to that which soon appears on the surface of water when poured on tar. Though after the texture of the wood is formed, and the chemical union of the different elements of the tree has taken place, it might be supposed, that the contents of the vessels would be coloured matter; yet they are quite transparent and colourless. Still it is difficult to say, whether the little globules, like dew in miniature, which appear in beautiful rows, are contained in longitudinal vessels, or move along by attachment to the filmy-formed thread, now become woody fibre, in a manner similar to the perspiration on the hairs of animals.

The globules which are considerably tinged with colour, and are, no doubt, the blood of the plant, are pressed along the woody fibres, as from one piece of network or lodgement to another. The other sort of longitudinal vessels, if such they may be called, are very irregular; and appear like a number of the small ones ruptured, and combined into one, being evidently open all round like a net purse. They generally contain considerable quantities of fluid; which is supported by thin and seemingly tough membranes. The radiates frequently cross these larger vessels; and the general appearance of the blood in them both, where they pass each other, seems to indicate, that they communicate together in their passage. The longitudinal vessels however appear to contain a much greater portion of air than the radiates. In all the specimens that I have examined, these largest longitudinal vessels are generally from one-eighth to three-eighths, and sometimes half an inch apart, on the circular line of the plant; and about three of them in the breadth of each concentric ring, or year's growth. In other cases they are much further apart, but they are extremely irregular. Sometimes three, four, or five will occur in a cluster. Where there is such a cluster, the turpentine will generally be found exhaling from the plant by these canals. This however is far more common in the silver fir, the Weymouth pine, and some others, than in the *pinus silvestris*, or Scotch fir.

The chemical analysis of the woody fibre of this plant in various experiments has yielded such different results, that I think it unnecessary to mention them. I shall therefore only take notice of the juices, and the process of their elaboration as they relate to temperature. A low temperature appears most congenial to the fir tree, although it is to be found in very warm climates. Yet from the nature of its juices it cannot be expected to arrive at great perfection in a lower degree of north latitude than 53° or 54°; or rather except in a mean temperature that is proportionate to these degrees of latitude, say 45° or 46° of Fahrenheit. It is necessary too, that the temperature should approach much nearer to permanency, than is required by most other plants. This is most obvious with respect to that called the Scotch fir, which by the by is to be found in every country in Europe; is the species that furnishes the red or yellow deal; and is confessedly of the best grain and texture, and most durable of any sort of fir yet known, the larch excepted.

The *succus proprius* of fir trees is of a peculiar kind, and known by the general name of turpentine, different varieties of which are produced by different species of the fir. It is obtained in considerable quantity by boring holes, or cutting deep notches, in the trunk of the tree; but more expeditiously by means of artificial heat, when it assumes the name of tar. To procure this, the wood of the trunk, branches, and roots, is heaped together, covered with turf, and then set on fire, so as to be exposed to a smothering combustion, as in preparing charcoal. A gutter is formed at the bottom, to receive the turpentine, which flows out strongly impregnated with carbon, whence the tar acquires its black colour.

In

In making pitch from tar by inspissation after it has been imported into this country, the first product that distils over is a brown, acid water, mixed with a good deal of oil. As the process proceeds, and the heat is increased, the acid diminishes, and the oil increases. According to Aikin, from 600 gallons of tar, or 18 or 20 barrels, the product will be about 10 barrels of pitch, or 22 cwt., 17 6gallons of oil, and about 40 gallons of acid. The oil and water, which are distilled over, do not again mix, so that they can easily be separated by decantation.

Where the temperature, when this tree is planted, is raised above a certain degree, the equilibrium of the compound is destroyed; and the oil, being the most volatile of any of the vegetable oils, flies off in the shape of vapour. The resinous substance at this time, being much heated, becomes more fluid; and the air being expanded through the whole exterior texture of the plant, the juice is pressed upward by the same means and force as in other plants. Though the assistance of the leaves is comparatively trifling, their deficiency is supplied by the very expansive quality of the juice: a quality it possesses to such a degree, that in barrelling tar at Archangel, it is found necessary, to leave a considerable space in the top of the barrel empty; I have been told as much as four or five inches, in order to meet the expansion of our temperature.

When by a high temperature the juice of the tree, or rather the oil, is forced off in various ways, as at any amputated branch or wound in the tree, by the buds and leaves, &c.; on the return of the evening, the resinous matter congeals, and fixes itself in the situation where it happens to be, with a very little decline of the temperature; and becomes almost incapable of being raised any higher by the power of capillary attraction. In consequence of the oily substance being driven off by the heat of the day, and the vessels becoming comparatively empty, they are then compelled to absorb more than an ordinary portion of the circumambient atmosphere, not only to restore the *succus communis*, that the tree had perspired in the heat of the day like other plants, but likewise to fill up the space before occupied by the volatile oil of the *succus proprius*.

Thus the fir tree in a warm climate is not only liable to be deprived of its native juices, by one part, which appears to be about twenty-five per cent. of the whole juice, being driven off in oily vapour, and another rendered thick and pitchy, so that it is incapable of being acted upon any more in the shape of sap; but likewise to be loaded with matter, to fill the space before occupied by the volatile oil now dissipated. This coming in some measure into contact with the resinous substance by the returning heat of another day, extracts from it something like the film, that will appear on the surface of water impregnated with resin or pitch; and thus the native juices are rendered solid and hard.

Hence the lower or butt ends of fir trees, the growth of this country, are frequently found as hard and full of resin, as if they had been saturated with it in a boiling caldron; while the trunk and the branches have no more than is absolutely necessary to constitute the woody fibres, which become very dry and brittle. When such trees are cut down for purposes of carpentry, they are found very difficult to work.

In the first instance the sawyers experience difficulty in cutting them. They are not only hard, but they clog the saw, filling up the teeth, while a resinous matter adheres to the sides of even the best saws. When, after considerable trouble and expense, they are cut into planks, the same effect is found on the carpenters' tools; the edge, however fine, being very soon clogged up; so that if the work be small, it is scarcely possible to make fine tools work at all, without a very large portion of hog's lard, or some other grease.

This causes the difficulty of working firs of British growth; and is wholly occasioned by the oily part of the juice being driven off: for if the workmen apply oil of turpentine to their tools, it is found the best for their purpose, though most expensive; and this is the very matter of which the timber is deprived by a high temperature.

It is well known, that in a warm climate the fir tree thrives best on the tops of hills. When Maundrell visited Mount Lebanon, on the 9th of May 1696, of the trees so much spoken of in holy writ, he could find only sixteen of any considerable size. But there were many young ones growing from amidst the snow on the very top of the hill.

It is likewise found, that in colder climates many species of fir grow in abundance in the valleys and low lands, where the Scotch fir grows to a very large tree. In consequence of the temperature being low, the equilibrium of its juices is kept up, and they ascend together in the tree, affording to every branch and fibre its proper nutriment.

The concentric rings of the oak, the ash, &c., are principally marked by the number of large holes, or longitudinal vessels, by which they are formed. On the contrary, those of the fir tree are most perspicuous from the more hard, solid, brown part of the rings, which contain a far greater proportion of turpentine than the other parts of the tree. The sap of this tree appears to be in motion throughout the winter, by which winter motion this dark part of the circle is formed. Not that there is really so much addition to the diameter of the tree in that season, for the original frame (if the term may be allowed) of the woody fibres

fibres appears in every way the same as the others, and their number the same and no more than that of the other parts of the wood in an equal space; so that the whole number of the woody fibres of the fir, like those of other trees, may be presumed to be formed in what is commonly termed the vegetating season of the year: but a few near the outside are rendered more hard and firm, by the change of temperature acting upon them when the heat is not sufficiently strong to penetrate deep into the body of the wood; for from the nature of the juice a blink of sunshine even in the time of frost and snow will most probably affect the turpentine, as it affects the motion of the sap of other trees in a warmer season. The heat however having little force, and the time being probably short before it is abstracted again by the ambient air, which gives it the countermotion, it cannot penetrate deep into the wood.

If Gay-Lussac and Thenard be correct in their opinion, "that a vegetable substance is always resinous, or oily, or spirituous, whenever it contains oxygen in a smaller proportion to the hydrogen, than it exists in water; and that a vegetable substance is always acid, whenever the oxygen it contains is to the hydrogen in a greater proportion than in water;" it is at least probable, that the turpentine part of the juice is principally formed in the winter, or at other seasons of low temperature, when the hydrogen may more readily be obtained for the necessary chemical union than in a high temperature; and the acid may for the same reason be principally formed in a higher temperature, when the chemical union with oxygen is more readily effected.

Whether the causes of the respective chemical combinations be sufficiently well understood, or not, thus far appears quite clear, that the dark part of the concentric rings principally assumes its colour in the winter season; and is more strongly distinguished from the other part of the ring in trees that grow in warm situations, or in the skirts of a wood, than in those in the interior of a wood, where the temperature is more permanent. Not that there is much difference in the quantity of turpentine in the whole ring; for, if the dark part have more turpentine than its due proportion, the other part has less.

It is probable too, that the high temperature of our summers, or vegetating seasons, prevents that chemical union, which constitutes the turpentine, or juice of the fir tree: and further, that which is afterward formed in the winter is partly driven off, as already mentioned, in the following summer.

The fir known by the name of the pitch pine of America, so much used in this country of late, which grows in a cold climate in low or wet land, has very little resin, but a very large pro-

 portion

portion of volatile oil and *succus communis*. This tree grows very spungy and soft; and its juices, which appear scarcely thicker than water, can be driven off with a very little heat, leaving the fibres of the wood almost naked. Hence it is, that this sort of timber, when moderately dry, will absorb nearly half its own bulk of water, without increasing its size in any degree beyond what it had when in full vegetation.

This water being again evaporated, and leaving the fibres open to the action and reaction of the atmosphere, the wood very soon decays. Pliny writes, that this tree was used for no other purpose among the Romans, than to rend for laths, or to form staves for coopers to make tubs and barrels, and a few thin boards for pannels.

The Siberian hunters of the ermine, when their fermenting yeast, which they carry with them, is spoiled by the cold, so that it will not serve to make the acid liquor they call quass, scrape off the alburnum, or half-formed wood, which is under the bark of the fir tree, and digest it with water over the fire for an hour. They then mix it with their rye meal, bury the dough in the snow, and in twelve hours' time find a new ferment prepared. This is presumed to be a strong, if not a decisive proof, that these juices contain a very large portion of caloric, and have the power of resisting cold more than any other vegetable juice. For although there may have been considerable heat in the dough when buried, yet it might have been supposed, that the snow would very soon have reduced the temperature to an equilibrium with itself, or 32 degrees at least: and this must soon have stopped the fermentation, for there appears no evidence, that fermentation will go on at all under 36 degrees. The snow also, in abstracting caloric, would have melted round the dough, and the moisture thus produced would have had a further tendency to resist the fermentation.

These juices then have the power of retaining caloric sufficient to protect the vital principle of the plants, to which they belong, against a greater intensity of cold than the juices of most other plants.

In this country, or perhaps any other of equal temperature, if a great number of fir trees, or even small clumps of them, be planted together on any plain, for ornament or use, where there is no shelter from one side more than another, they are generally found to thrive best on the north side of the clump. The reason is, that those on the south side shelter them from the direct rays of heat, though the heat or general temperature of the day may be nearly as high on the north as on the south side. A few instances of this fact near London may be mentioned. In the whole

whole of those clumps on Hounslow heath, where the trees are not destroyed, but grow so as to shelter each other, and in that other clump on the high ground south of the same road, between Egham and Virginia Water; although the trees are all young, there are several feet difference in the height between those on the north and those on the south side of the same clumps.

Hence it is inferred, that no species of the fir tree will arrive at such perfection in our country, on account of its high and *rapid* change of temperature, as in the higher latitudes.

Yet nature is bountiful in providing for all; as the reverse of this is the case with the oak tree.

XLV. *Receipts for Enamel Colours, and for staining and gilding Glass.* *By Mr.* R. Wynn.*

Sir, — The liberality of the Society in encouraging and rewarding communications in every useful art, induces me to offer to their notice a concise and complete method of composing enamel colours. Painting in enamel colours has always been considered very interesting, and one of the most costly productions of art in every country where practised: but the real preparation of the colours has always been confined to the knowledge of a few persons who have made a mystery of it, and whatever has been yet published on the subject, appears to be chiefly the compilation of writers unskilled in the profession. Many artists of superior talents, in different parts of this country, could practise the art but for the difficulty of procuring a complete set of good colours: indeed it is extraordinary with what suspicious secrecy the art of making the proper enamel colours has hitherto been conducted. I have been acquainted with several of the best manufacturers, whose colours were used by the most eminent painters on the finest and most elaborate works of the time, who have died without ever benefiting their country by publishing their acquirements, or leaving any documents behind them. By a continued perseverance in such secrecy, it is not impossible that the present improved state of knowledge in the art might, under unfavourable circumstances, be entirely lost, if some experienced professional person did not seek for an opportunity of making it public, and more generally useful. With these motives I take the liberty to offer the accompanying treatise, which

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1817. Twenty guineas were voted to Mr. Wynn, by the Society, for this communication.

 consists

consists of the most valuable selections from the experience and labours of above twenty years.

I am, sir,

Your most obedient servant,

Wellington Place, Vauxhall, March 11, 1817. ROBERT WYNN.

To A. Aikin, Esq. Sec.

Introductory Remarks.

The different qualities or degree of purity in the ingredients as usually met with, will, with the same prescriptions, produce some slight variations in effect; but the best enamel colours may always be obtained with certainty by careful attention to the following receipts.

When metals are dissolved, the solutions should always be perfectly saturated. In making the fluxes, they should be sufficiently melted in the crucibles, so as to flow liquid and pour out easily.

The various qualities of the material, or surface, on which painting in enamel is performed, require the colours to be adapted to the degree of heat it will bear or require when burnt; and it is generally the practice of the best painters in enamel, to use for the first painting and burning, colours considerably harder than those used in the second painting and burning, the latter being fine soft enamels, in order to finish the work, and give a beautiful smoothness. It cannot but be obvious to every person who uses enamel colours, that the hardening them if necessary, will be effected by adding more of the colouring matter; or lessening the quantity of flux, and, if required to be more soft, or to shine more when burnt, to add a little more flux; and this is best done by a very soft flux generally used for this purpose, such as No. 8; but when particular directions are necessary, they will be found mentioned in the receipt for making the colour. It will be better to make a few ounces at least of each colour at a time, and they are all to be ground as soon as made in water, with a glass muller, or a piece of plate glass, and dried before the fire, then scraped off in powder, and kept in bottles for use.

When used in painting they are ground in spirits of turpentine, and thickened with thick oil of turpentine, which quality the fluid oil of turpentine acquires in three or four years.

PREPARATION OF INGREDIENTS.

Flint Powder.

Take pieces of flint, which have been burnt white at the chalk lime burners (they may be had at the lime burners at Vauxhall), make them clean with hot water and a brush; then throw them red

red hot into cold water; after having thus treated them two or three times, pulverize them in a biscuit-ware mortar with a pestle of the same, (which will be easily effected,) and then grind them in water on plate glass.

Where no opportunity occurs of procuring burnt flints from the lime burners, the common black flints broken into pieces, made hot in boiling water (to prevent their flying in the crucible,) and treated in the same way, will produce a fine white powder.

Red Sulphate of Iron.

Sulphate of iron, otherwise called green vitriol, is to be pounded and placed in an earthen ware muffle, (which may be had at the ironmongers, in Foster-lane; or at Mr. Accum's, in Compton-street) till the moisture is evaporated and a gray powder left: which place in a crucible in a charcoal fire, and stir it with a piece of steel bar, till it is of a fine red colour; then let it fall out of the crucible into a pan of cold water, under a chimney, to avoid the disagreeable fumes that arise; when settled at the bottom wash it in several hot waters, and then dry it for use. The more it is burnt, the darker the red.

Brown Sulphate of Iron.

Take sulphate of iron in lumps, and calcine it in a red charcoal heat till it becomes of a dark brown; and let it cool in the crucible, afterwards wash it repeatedly in hot water.

Black Oxide of Copper.

Take copper and dissolve it in aquafortis, till the acid refuses to take up any more metal; then dilute the solution with water, and add to it some subcarbonate of potash, dissolved in water; a green precipitate will fall to the bottom, which must be washed in several hot waters; when settled, pour off the superfluous water, and place the green matter at the bottom on a piece of coarse open canvass, tied over a large earthen pan, on which a piece of blotting paper is laid; after the precipitate has been thus drained, it should be taken off and made perfectly dry, by placing the paper on a drawer of powdered chalk to absorb more of the moisture, and then placed before the fire. When dry, calcine it in a crucible in a charcoal fire, and throw it red hot into cold water; then rinse it in boiling water, and dry at the bottom of the bason before a fire; what remains is a beautiful black oxide of copper.

Green Oxide of Copper.

Take a saturated solution of copper in aquafortis, and precipitate it with subcarbonate of potash; then wash it several times in boiling water, filter and dry it.

White

White Oxide of Tin.

Into a small wooden box with a sliding cover, chalked over on the inside, pour melted tin from a ladle, and shake the box till the tin becomes finely granulated; then wash it and dry it, and put it into a Florence oil flask, and pour over it strong nitrous acid, which rapidly converts it to a white powder. When a sufficient quantity of this is obtained, it should be well washed in several boiling waters, poured out into a bason, and dried before the fire;—it then produces a very white oxide of tin.

Black Oxide of Cobalt.

Take good metallic cobalt*, and dissolve it to saturation in nitric acid diluted with a little water, in a flask placed in sand over the fire; then pour it in a large bason, and having added a quantity of water, pour in a solution of subcarbonate of soda, as long as any precipitate falls down: when settled pour off the water, and wash the powder in several hot waters, filter it and dry it. When dry, mix it in a biscuit-ware mortar with a pestle of the same, with three times its weight of dry nitre; place it in a warm crucible, and drop in an ignited piece of charcoal: some slight explosions will then take place, and when these have ceased, make the calx red hot; this, after being washed and dried, produces the best oxide of cobalt for enamel, and capable of making and compounding various colours.

Fluxes.

Take great care to mix all the ingredients accurately together in a biscuit-ware mortar, with a pestle of the same, and to have them all pounded as fine as possible. Let the crucibles be made warm before the fluxes are put into them, (by placing them on the fire with the open end downwards,) which will prevent most of the accidents which happen by their breaking in the fire.

The best furnace for making fluxes, or for any other process that requires great or continual heat, is a common German stove about 18 or 20 inches inside, lined all round from the grating to the top, (except the aperture at the door in the front for the occasional introduction of a muffle,) with one row of fire-bricks set with loam; the iron-pipe chimney projects from the back part near the top. The top or cover of the furnace, to be loose like a lid, and removable by handles; in the centre of it a circular hole is cut out, which is also fitted with a cover through which the top of the crucible may be lifted off, and its contents be stirred up with a bar of steel. A small piece of fire-brick is placed on the grate, for the crucibles to stand on, and the fuel should be charcoal and coke mixed, or charcoal alone.

* In the choice of cobalt, that which when dissolved in nitric acid gives the purest and deepest red solution generally makes the finest colours.

placed

Flux, No. 1.	Red lead 8 parts by weight		Flux, No. 5.	Flint glass	6
	*Calcined borax	$1\frac{1}{2}$		Flux, No. 2,	4
	Flint powder	2		Red lead	8
	Flint glass	6	No. 6.	Flux, No. 2,	10
No. 2.	Flint glass	10		Red lead	4
	White arsenic	1		Flint powder	$1\frac{1}{4}$
	Nitre	1	No. 7.	Flux, No. 4	6
No. 3.	Red lead	1		Colcothar	1
	Flint glass	3	No. 8.	Red lead	6
No. 4.	Red lead	$9\frac{1}{2}$		Borax not calcined	4
	Borax not calc.	$5\frac{1}{2}$		Flint powder	2
	Flint glass	8			

After the fluxes have been melted, they should be poured on a flag-stone wet with a sponge; or into a large pan of clean water, then dried, and finely pounded in a biscuit-ware mortar for use.

Yellow Enamels.

Red lead	8
Oxide of antimony	1
White oxide of tin	1

Mix the ingredients well in a biscuit-ware mortar, and having put them on a piece of Dutch tile in the muffle, make it gradually red hot, and suffer it to cool.

Take of this mixture ..	1
Of flux, No. 4	$1\frac{1}{2}$

Grind them in water for use.

By varying the proportions of red lead and of antimony, different shades of colour may be obtained.

Another Yellow.

Take three parts by weight of sheet lead and one part of block-tin, melt them together in a ladle or a flat shovel, and skim off the top in proportion as it oxidates; when a sufficient quantity is obtained, place it in the muffle in a gentle heat, to reverberate and entirely calcine any remaining particles:

Of which take	$7\frac{1}{2}$
Oxide of antimony	1
Litharge	1

Mix these well together, and give them a red heat in a muffle to bind them together, but not to melt. Use the same flux as for the other yellow.

Orange.

Take red lead ..	12	Oxide of antimony ..	4
Red sulphate of iron	1	Flint powder ..	3

* Borax calcined to a dry white calx in a crucible, only a third part of which should be filled at once, on account of the borax swelling so much as it gets hot.

well

well mixed in the mortar, and calcined so as to stick together, but avoid melting.

Take of the above	1
Flux, No. 7	2½

Grind for use.

Dark Red.

Take sulphate of iron calcined dark	1
Flux, No. 7	3

Grind for use.

Light Red.

Take red sulphate of iron	1
Flux, No. 1.	3
White lead	1½

Grind for use.

Red Brown.

Take brown sulphate of iron calcined dark	1
Flux, No. 1	3

Grind for use.

Vandyke Brown.

Take Flux, No. 4	3
Iron filings	1

melted together in a crucible, and drawn out with curling tongs *; with so much metal it will not pour out freely.

Take of the above	5
Black oxide of cobalt	1

Grind for use.

Another Brown.

Take manganese	2¼
Red lead	8½
Flint powder	4

calcined to stick together.

Of this mixture take	1½
Of Flux, No. 4, and iron filings melted as above	1½
Flux, No. 4	1

Grind for use.

Black for painting and mixing with other Colours.

Take of umber, broken into small bits and calcined to a yellow heat in a crucible, till quite black, then washed in boiling water and dried, ...	10
Black oxide of cobalt	10

* In this and in other cases where a muffle is not in readiness, an earthen crucible washed with flint powder inside, or with dry flint powder rubbed inside 1-8th of an inch thick, may be used, and the materials when partially melted, so as to stick together, are completely taken out without loss.

Blue

Blue flint glass	..	..	..	10½
Raw borax	..	..	..	7½
Red lead ..	..	..	..	12
Calcine these well together, and take of it				2
Flux, No. 4, ..	..	..	..	1

Grind in water for use.

Blacks are compounded in other proportions of these ingredients, and manganese is sometimes substituted in the place of umber.

Another Black.

Take umber calcined black		..	1
Black oxide of cobalt	..	..	1½
Black oxide of copper	..	..	½
Flux, No. 4 ..	..	..	3

Grind these in water, and when dried place them on a piece of Dutch tile, (washed over with flint-powder ground in water,) in a muffle in a charcoal fire, and calcine them so as to stick together; then add to it one-half of Flux, No. 4. Both these blacks, if too soft, are hardened by adding a little black oxide of cobalt.

Black for shading and drawing under the Greens.

Take manganese	..	..	5
Royal smalt ..	..	..	1

ground fine in water, and calcined to a high degree of heat in a muffle.

A beautiful Black, for solid grounds or inlaying, but does not mix generally.

Take black oxide of copper	..	1
Flux, No. 4	..	2

Grind in water for use.

A Frit for transparent Greens.

Take flint powder ..	3	Red lead	7½
Flux, No. 2	3	Raw borax	2½
Green pot-metal glass	1½	Green oxide of copper	1¼

Melt them in a crucible, pour out the mass, and pound it in an earthenware mortar.

Green.

Take of the green frit	3
Of yellow enamel colour made up as before directed	1½

If too soft add Naples yellow.
Grind in water for use.

Another Green.

Take of green frit	..	.. ..	5

Flux,

Flux, No. 2	½
Flux, No. 6	2½

Grind in water for use.

Greens in painting on enamel, are formed of various shades by mixing blue and yellow, or blue and orange, &c. in different proportions.

Blue.

Take of black oxide of cobalt ..	4
Flint powder	9
Nitre	13

Mix them well in an earthenware mortar, and heat them in the crucible, in a strong fire of coke and charcoal, till perfectly melted*; then pound the mass, wash it in cold water, and dry it.

Of this take	1
Flux, No. 5	1

Grind in water for use.

Another Blue.

Take black oxide of cobalt ..	1
Raw borax	1

Melt them together.

Of this mixture take	2
Blue pot-metal glass	10
Red lead	½

Melt them in a very strong fire. If either blue is too soft, add a little royal smalt; if too hard, a little flux made of blue glass 2, borax 1.

Purple.

Take fine-grained gold from the refiners, and dissolve it to saturation in an aqua regia made of strongest

Nitric acid ..	1	by measure.
Muriatic acid ..	3	
Distilled water ..	3	

Make the solution in a clean Florence oil flask, placed on sand near the fire. Pour melted tin into cold water, and take of the clean parts of this tin 1

Aqua regia, diluted with water in the same proportion as above 4

Place the acid and the tin in a large bason covered with a plate, in a temperate heat; when the tin is all dissolved, add

Strong red fuming nitrous acid ..	1
Tin	1

And instantly cover the bason with the plate, to prevent the

* If these preparations are not sufficiently fluid in the melted state to pour out of the crucible, the colour will stick to a piece of steel bar when it is warm, and may be drawn out; and sometimes the blues are made in crucibles lined with flint powder as before mentioned.

fumes

fumes from escaping. After standing twenty-four hours, a little distilled water should be poured into the bason. The solution of tin may then be put into a clean phial for use, adding to it a few grains of tin; examine it after four or five days, when the solution, if carefully made, will be of a fine and clear dark colour, and fit to make the purple with. Then of the solution of gold take sufficient to colour distilled water of a faint straw yellow, and drop gradually into it the solution of tin, and a most beautiful purple precipitate will immediately be formed, which must be thrown as it is made into a large vessel, and two or three pieces of the melted tin should be put at the bottom.

Mix the solution of tin with that of the gold in this manner till the last added drops occasion no turbidness in the liquor; the precipitate is then to be washed in several hot waters, filtered on the blotting paper and canvass, and while in a moist and soft state, mixed with flux, No. 4, finely powdered.

The proportion of flux to the purple precipitate is always various, and is judged of by the mass being of a good rich and dark colour, as the ingredients are ground together on the plate glass. Care must be taken to grind this colour before it gets dry.

Twenty-four grains of gold made into a precipitate in this manner will take two ounces of flux, and this may be a rule to the inexperienced practitioner.

Rose Colour.

To a saturated solution of gold in aqua regia (containing twenty-four grains of gold), diluted with 100 times its bulk of warm distilled water, having 20 grains of alum* dissolved in it, add caustic ammonia drop by drop as long as any precipitate is thrown down, which wash in several hot waters.

To 24 grains of gold, precipitated in this manner, put

Flux, No. 4	..	..	..	2 oz.
Flux, No. 3	..	..	..	2 oz.

Mix them together wet, and grind them on a plate glass, adding, by a leaf at a time, sixteen leaves of leaf silver; when the whole is ground fine, let it be dried on the glass, scraped off, and put in a bottle for use.

This rose colour grinds of a gray or slate colour, but after being ground, if placed in a muffle and exposed to a gentle heat, it will turn to a red; but it is fit for use in either state.

If too yellow, add a little purple; and if too purple, add more leaf silver.

Another Rose Colour.

Take purple made as before directed			..	1 oz.
Flux, No. 3 ..	..	..	..	4
Muriate of silver	..	..	..	10 gr.

* The rose colour is sometimes made without any alum.

The

The latter ingredient is prepared by dissolving silver in aquafortis, and precipitating it with common salt.

Grind in water for use; if too purple, add more muriate of silver.

Opake White.

Hartshorn shavings burnt in a crucible, in a charcoal fire, till perfectly white, ... 1

Flux, No. 1 1

Grind in water for use.

Venetian white cake enamel .. 1

Flux, No. 8 .. .. .. $\frac{1}{4}$

Grind in water, then calcine them together in a muffle.

Flux, No. 2, pounded and washed, then dried and calcined in a muffle.

It would not be difficult to exhibit a multitude of specimens of different tints, and fill a volume with descriptions of them, by combining these original enamel colours in various proportions: this, however, may safely be left to the taste and experience of the artist. My object has been to avoid every thing superfluous, and at the same time to explain the processes adapted to immediate practice in terms not liable to be mistaken.

Sir,—When I had the honour of explaining my Treatise on Enamel Colours to the gentlemen forming the Committee of Chemistry, I was requested by them to produce the method of *staining glass*, with a view of adding it as a second part to the Treatise on Enamel Colours.

I have inserted the most valuable information which I possess on this subject in the paper that accompanies this letter, and request that you will present it in my name to the Society of Arts. I am, sir,

Your most obedient humble servant,

No. 3, Taylors-Buildings, May 6, 1817. ROBERT WYNN.

To A. Aikin, Esq. Sec.

The Art of staining Glass.

In *coloured glass*, the whole body of the material is tinged throughout by means of some colouring ingredient uniformly diffused through, or dissolved in, the substance of the glass.

In *enameling*, the colours, being ground up with an easily vitrifiable flux, are laid on the surface of metal, or porcelain, or glass, and are then exposed to such a degree of heat as shall just melt the enamel, and then fix it on the surface of the substance on which it has been applied.

In *staining glass*, the colouring ingredients are mixed with water,

water, or some other fluid vehicle, by means of which they are spread over the surface of a plate of glass, and when dry, are exposed to such a degree of heat as by experience has been found to be sufficient. The colour is then rubbed off from the surface of the glass, to which it does not adhere, and those parts of the plate which have been thus covered are found to have acquired a permanent and transparent tinge or stain, doubtless from some particles of the colour having been absorbed, and fixed in the pores of the glass.

In all the compositions for staining glass, silver, in some form or other, enters as an essential ingredient; I shall therefore begin by describing the different preparations of silver that I make use of.

Take two or three ounces of pure nitric acid, dilute it with three times its bulk of distilled water, put it into a Florence flask, or any other convenient glass vessel, and add to it refined silver, by small pieces at a time, till the acid, though kept at a warm temperature, refuses to dissolve any more. After standing quiet for some hours, pour off the clear liquor into a clean ground stoppered phial, and label it Nitrate of silver.

Preparations of Silver.

No. 1. Dissolve common salt in water, and add nitrate of silver, drop by drop, till it ceases to occasion any precipitate; there will thus be obtained a heavy white curd-like substance, which must be well washed in hot water and dried; by exposure to light, it becomes of a dull purple colour. It is known by the name of muriate of silver, or *luna cornea*.

No. 2. Dissolve subcarbonate of soda in water, and add nitrate of silver, as above described. The white precipitate thus obtained, when washed and dried, is ready for use. It is called carbonate of silver.

No. 3. Dissolve subcarbonate of potash in water, and proceed precisely as directed for No. 2. The white powder thus obtained is also carbonate of silver.

No. 4. Dissolve phosphate of soda in water, and proceed as already mentioned. The precipitate thus obtained is of a yellow colour, and is called phosphate of silver.

No. 5. Take any quantity of pure silver rolled out into thin plates, and put it into a crucible, together with some sulphur. When the crucible has been a short time on the fire, the sulphur will first melt, and then will gradually burn away with a blue flame. When the flame has ceased, add some more sulphur, and proceed as before; then take the silver out, and heat it red in a muffle; it will now be white, and very brittle, and, after having been reduced to powder in a mortar, is fit for use.

No. 6. Take any quantity of a dilute solution of nitrate of silver, and put into it a stick of metallic tin, warm it a little, and the silver will be precipitated in the form of metallic leaves on the surface of the tin. Scrape it off, wash it in warm water, dry it, and grind it in a mortar.

No. 7. Take any quantity of nitrate of silver, and put into it a piece of copper plate; then proceed precisely as in No. 6.

The foregoing preparations of silver mixed with other ingredients, in the proportions about to be described, compose all the varieties of pigment which are requisite for staining glass.

Yellow.

Take silver, No. 2	1	parts.
Yellow lake	1	

Mix the ingredients, and grind them well with oil of turpentine mixed with the thick oil of turpentine; lay it on thin.

Take silver, No. 1	1	parts.
White clay, precipitated from a solution of alum by subcarbonate of soda	3	
Oxalate of iron, prepared by precipitating a clear solution of sulphate of iron by oxalate of potash	3	
Oxide of zinc	2	

Let the silver be ground first in water with the oxide of zinc, and then with the other ingredients. This is intended for floating on thick.

Take silver, No. 3 ..	1	parts.
Yellow lake ..	1	

Grind them in spirit of turpentine and oil, and lay the mixture on very thin.

Take silver, No. 4 ..	1	parts.
Yellow lake ..	1	
White clay ..	½	

Grind them in spirit of turpentine and oil, and lay the mixture on thin.

Orange.

Take silver, No. 6	1	parts.
Venetian red and yellow ochre, equal parts, washed in water, and calcined red ..	2	

Grind the ingredients in spirit of turpentine, with thick oil of ditto, and lay the mixture on thin.

Take silver, No. 7	1	parts.
Venetian red and yellow ochre	1	

Grind in turpentine and oil, &c. as the foregoing. If entire panes of glass are to be tinged orange, the proportion of ochre may be greatly increased. The depth of the tinge depends in some measure on the heat of the furnace, and on the time that the glass is exposed to it, which, though easily learned by experience, cannot be made the object of precise rules.

Take

Red.

Take silver, No. 5, ...	1	parts.
Brown oxide of iron prepared by heating scales of iron, then quenching them in water, reducing them to a fine powder, and lastly calcining it in a muffle	1	

Grind the ingredients with turpentine and oil, and lay the mixture on thick.

Take of antimonial silver, prepared by melting together one part of silver, and two ditto of crude antimony, and pulverising the mass	1	parts.
Colcothar	1	

Grind the ingredients in turpentine and oil, and lay the mixture on thick.

Take antimonial silver, prepared as above, ..	1	parts.
Venetian red, and yellow ochre, of each ..	1	

Grind, &c. as before mentioned.

When whole panes of glass are to be tinged, the proportions of ochre or of colcothar may be much increased, and the ingredients should be ground in water.

Of laying on the Colour.

The method practised by most stainers of glass is to draw the outline in Indian ink, or in a brown colour, ground with turpentine and oil, and then to float on the colour thick, having previously ground it with water. But in this way of proceeding it is very subject either to flow over or to come short of the outline, and thus render the skill of the draftsman of little effect.

My method is to draw the pattern in Indian ink, and having ground the colour as fine as possible in spirits of turpentine, brought to a proper consistence with thick oil of turpentine, to add a little oil of spike lavender, and to cover the outline entirely with this composition.

When it has become dry, I work out the colour with the point of a stick and a knife from those parts that are not intended to be stained, and am thus enabled to execute the most delicate ornaments and most intricate designs, with exactness and precision.

If the colour is required to be laid on so thick that the outline would not be visible through it, let the colour be first laid on as smoothly as possible, and when it has become dry draw the outline upon it with vermilion water-colour, and work out the design as before.

Besides the precision acquired by the above method, it enables the artist to apply different shades in the same design; whereas the old method of floating only communicates an uniform tint to the whole pattern.

The artist should contrive to charge his furnace with pieces the colour of which is ground in the same vehicle, and not to mix in the same burning some colours ground in turpentine and others ground in water. The pieces must also be very carefully dried, and must be placed in the furnace when this latter is moderately warm.

To gild Glass.

Take of fine gold in grains .. .. 1 } parts.
of pure mercury .. .. .. 8 }

Warm the mercury and then add the gold, previously making it red hot. When the gold is perfectly dissolved, pour the mixture into cold water and wash it well. Then press out the superfluous mercury through linen or soft leather, and the mercury which runs through (as it retains some gold) should be reserved for the next opportunity.

The amalgam which remains in the leather is to be digested in warm aquafortis, which will take up the mercury, but will leave the gold in the form of an extremely fine powder. This powder, when washed and dried, must be rubbed up with one-third of its weight of mercury; then mix one grain of this amalgam with three grains of gold flux (see the former part of this paper, p. 271), which is to be applied in the usual manner.

*** In the foregoing communication borax is mentioned as an ingredient in the composition of the fluxes. It does give them very easy fusion, but we should fail in our duty did we neglect to caution artists against a profuse use of this flux. It can hardly be employed in any quantity whatever without danger to the durability of the work; having a great tendency to effloresce in the atmosphere. Indeed this can hardly, if at all, be prevented where borax enters into the composition of colours used for painting on glass.—Edit.

XLVI. *On the Resilience of Materials; with Experiments.* By *Mr.* Thomas Tredgold.

To Mr. Tilloch.

Sir,—Previous to making the experiments on the resilience of timber, the results of which are given in your Number for last month*, I had endeavoured to anticipate the results by the usual mode of calculation, and proceeded to make some rough essays, in order to try the apparatus and prepare for more correct experiments; as it was desirable to let the weight fall from a height that would only just produce fracture, without having to make successive trials, which always weaken the pieces considerably.

In these trials I soon discovered that the calculations I had made would be of no use to me whatever, although I had to-

* Phil. Mag. vol. li. p. 216.

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lerably correct data to go upon; and that the theory is imperfect, in as far as it does not include the whole of the principal circumstances affecting the case in question.

In the first place, I propose to lay before you the investigation according to which the heights of the falls were computed. 2dly, Some additional experiments; and then conclude with some observations on the probable cause of the difference between the theoretical and experimental results.

I. Put s = the strength, or weight that would break a bar supported at both ends; a = the deflexion at the time of fracture; x = any variable degree of deflexion; W = the weight; h = the height of the fall; w = the weight of the bar; and $\sqrt{4gh}$ = the velocity with which W strikes the bar.

Then $a : x :: S : \frac{Sx}{a}$ = the motive force at any deflexion x; consequently $\frac{Sx}{a(W+w)}$ = the accelerative force there.

By mechanics $-v\dot{v} = 2gf\dot{s} = \frac{2gSx\dot{x}}{a(W+w)}$; of which the fluents are $-v^2 = \frac{2gSx^2}{a(W+w)}$; but when $x = o$, $v^2 = 4gh$; therefore $4gh - v^2 = \frac{2gSx^2}{a(W+w)}$. Also, when $x = a$, $v = o$; consequently $4gh = \frac{2gSa}{W+w}$, or $h = \frac{Sa}{2(W+w)}$ *.

It is shown by writers on the resistance of solids, that at the time of fracture S is inversely as l; l being the length, and that a is directly as l^2. Hence, all other things being equal, we have h is as l.

The resilience of bodies has been considered by Emerson, in his *Fluxions*, sect. 3, prop. 21, p. 404; and by Dr. Thomas Young in his *Nat. Phil.* vol. ii. art. 337; and their conclusions are the same as those above stated.

Experiments on the Resilience of Timber.

II. As Memel or Riga timber, when it is straight-grained, and the portions of the annual rings vertical, almost always breaks short or without splinters, it afforded an opportunity of trying each piece thrice; and the same was done with some of the others, when the splinters did not extend too far along he piece.

To avoid referring to the results given in your last Number †,

* The motive force ought to be $\frac{Sx}{a} - (W+w)$, but for the sake of simplicity the quantity $(W+w)$ is neglected, as when it is only very small in respect to S, it does not materially affect the result. Were it included, the correct solution would be $h = \frac{\frac{1}{2}Sa - a(W+w)}{W+w} = \frac{Sa}{2(W+w)} - a$: that is, less than the quantity above, which is itself less than it ought to be.

† Phil. Mag. vol. li. p. 216.

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the height that broke the pieces which would admit of a second trial, are here repeated.

Those trials made with the same piece are marked with the same letter. And the same weight of seven pounds was used in all the trials.

Kind of Wood.	Specific gravity.	Position of the annual rings	Distance between the supports in Inches.	Depth in Inches.	Breadth in Inches.	Height from which the Weight fell.		Effects.
						By Experiment Inches.	Calculated Inches.	
Oak (A.)	·743	vertical.	30	1	1	48	35	Broke.
Oak (A.)	·743	vertical.	11	1	1	54 60		No effect. Broke.
Dry Larch (B.)	·506	nearly vertical.	30	1	0·8	54	43	Broke.
Dry Larch (B.)	·506	the same	11	1	0.8	54 60		No effect. Broke.
Riga (C.)	·484	vertical.	30	1	1	36 42	21	No effect. Scarcely broke.
Riga (C.)	·484	vertical.	12	1	1	42 48		No effect. Scarcely broke.
Riga (C.)	·484	horizont.	12	1	1	48 54		No effect. Broke.
Memel (D.) ..	552	vertical.	30	1	1	36 42	31	No effect. Broke.
Memel (D.) ..	·552	vertical.	12	1	1	42 45		No effect. Broke.
Memel (D.) ..	·552	vertical.	12	1	1	45		Scarcely broke.

Observations.

III. From the preceding table it appears that the height is not directly as the length, as we find a shorter length actually requires more height than a longer one; and it is to this difference chiefly that I am desirous of drawing the attention of those who have more leisure to make experiments, and more ability to pursue the investigation. The difficulties which attend making experiments on the laws of percussion are well-known, and various means have been devised to avoid the irregularities which occur in such experiments, but with little success.

These irregularities evidently originate in the methods of measuring the effects; at least, it is more probable than that there is any defect in the theory of falling bodies. Camus, who published a considerable number of experiments on the effects of percussive forces, found that the nature of the bodies struck, and of the supports on which they rested, had much influence on the effects. Also in other experiments, made for the purpose of assigning

signing the degree of pressure equivalent to the stroke of a pile engine, the effects were not found to agree with the laws of falling bodies. M. de Cessart endeavoured to compare the percussive and pressive forces, by reducing equal cones of lead to the same thickness by the blow of a ram, and by pressure. M. Rondelet tried to accomplish the same object by means of the dynamometer of M. Regnier*; neither of these gentlemen found the effects to be proportional to the heights of the falls †; consequently we must either suppose this law to be incorrect, or that the motion is partly destroyed by the re-action of the supports. Now the experiments of Camus tend to prove the latter supposition to be correct; but the theory given in the first part of this letter does not suppose any part of the velocity to be destroyed by being transmitted to the supports, which appears to be the cause of the difference between it and the results of the experiments, and therefore requires to be considered anew, as it is not, in its present state, sufficiently correct for any practical purpose.

That a part of the velocity is destroyed by the supports is easy to conceive, because, I believe it may be shown, that the velocity of the motion transmitted through the length of the bar, will be greater than that with which the point struck will descend. I am, sir, yours, &c.

Grove End, April 14, 1818. THOMAS TREDGOLD.

XLVII. *On the Expeditions to the North Pole. By M.* MALTE BRUN ‡.

SOME English writers have already indulged themselves in illusory conjectures on this enterprise; they describe Greenland as bursting from the icy barrier which surrounds it. They repeat the traditionary reports of the last century, respecting some whale-ships said to have reached the pole, and even to have passed on the other side of it. They presume to doubt the existence of land to the north of Baffin's bay, although Baffin was the best informed and most judicious seaman England could boast of; and they even flatter themselves, that commerce will be carried on in a direct route from London to Canton, by the pole, which makes a distance of only 2,600 nautical leagues, whilst that by the Cape of Good Hope is 5,500 leagues.

Two men, however, of great authority, do not participate in these exaggerated anticipations.—Mr. Scoresby, an experienced

* Rondelet's *Art de Bâtir,* tome iii. p. 22—32.

† These experiments were certainly not all applicable to the case in question; that is, to establish the relation between the pressive and percussive forces, as far as regards driving piles.

‡ From the *Journal des Débats.*

Greenland captain, thinks that the polar seas are blocked by ice, and proposes a journ y to the pole by means of sledges drawn by rein-deer. Captain Burney, who accompanied the immortal Cook, has published a pamphlet, of which we have received a copy, and in which he proves clearly that there exists a large extent of land to the north of Behring's strait: he goes too far in supposing that this land probably unites the two continents of Asia and America; but it is very certain that his arguments are sufficient to contradict the opinion of the existence of an open sea.

The gratitude which the English government merits from every friend to science, ought not to prevent us from examining upon what foundation their hopes of success in this enterprise rest. It is even an advantageous justice due to the commanders of the expedition, to point out to them beforehand the immense obstacles against which they have to contend.

Fixed and floating ices may be considered as the first of these difficulties. Admitting for a moment the non-existence of a polar continent, that Greenland, New Siberia, that the land to the north of Behring's strait, and the land seen and coasted by Baffin, are in reality but four islands, (as the maps of the sixteenth century appear to represent them,) yet it is very probable, that the narrow seas which separate them are constantly choked with ice. Captain Scoresby observed the ice form itself in the open sea, at more than twenty leagues from the coast of Spitzbergen; and masses of ice arise from the bed of the sea. Thus, then, the principal argument in favour of an open sea is considerably weakened; it will be in vain for them to rely on the removal of a barrier of ice, shaken by an earthquake, or broken by the strength of the currents. The sea itself freezes, notwithstanding the assertions of Mr. D. Barrington. This truth once proved, eternal variations in the state of the polar seas may be expected.

From the year 1660 to 1680, the most sanguine hopes were entertained of penetrating to the pole; yet every authenticated effort to this effect proves all exertion to have been terminated by fixed ice, which arrested the navigators' further pursuit; or by floating masses of it, which threatening to overwhelm them, compelled them to desist from their courageous enterprise. Captain Wood, who confidently believed the possibility of a passage to the north, was opposed by a continent of ice, which united Nova Zembla, Spitzbergen, and Greenland.—Captain Souter, on the contrary, continued his route to 82 deg. 6 min. in a calm open channel; but the ice, on each side, beginning to connect, the dread of being blocked up obliged him to abandon his purpose. The adventurous Baffin was able once to navigate the sea which bears his name; but it has since been frequently found that this sea was filled with numerous islands, some of which were

100 leagues

100 leagues in circumference, and nearly 400 feet high. Perhaps James's island, noted in several charts, was itself one of these masses of ice. Captain Wafer frankly confesses that he mistook these fixed masses of about 500 feet in height for real islands. Even the floating ice is often covered with large stones, and trees torn up by the roots, which appearance produces the idea of land. It is very uncertain whether the Dutch discovered, to the east of Spitzbergen, a tract of land, or only a mass of ice: in one of their voyages to the north of Nova Zembla, they found a bank of blueish ice, covered with soil, on which birds had built their nests. Half a century ago, two islands of ice were seen to fix themselves in the bay of Disco: the Dutch whale-ships visited and assigned them a name. The same circumstance took place about Iceland.

Mr. Scoresby informs us, that the water of the sea of Spitzbergen contains only $5\frac{3}{4}$ ounces of salt in a gallon of 231 cubical inches: its weight is to pure water as 1,0260, and it freezes in a cold of two degrees by a thermometer *centigrade*. Thus it is probable, that in the polar nights, that is to say, during our winter, the arctic seas are covered with ice. The history of different voyages furnishes us with proof of this;—but let us resume the physical discussion.

The masses of floating ice arising from the water of the sea, and which is distinguished by its porous contexture (owing to the mixture of the volatile qualities of plants), sink 4-5ths of their thickness into the salt water, the latter being in a freezing state. The masses formed of fresh water have a greater density and more transparency. Mr. Scoresby made burning-glasses from pieces of it, and lighted the pipes of the astonished sailors. These bodies sink 15-16ths of their thickness below the surface. It appears certain, that the bays and straits of the polar seas (not generally very deep) are often obstructed by these masses, resting at the bottom. The floating ices present obstacles not less redoubtable.—The concussion of the masses produces a tremendous noise, which warns the navigator with what facility his vessel would be dashed to pieces, if he were placed between two of these floating islands. It is even asserted that the wood carried off by the currents kindles by the violent collision caused by the motion of the ice; and flame and smoke arise amidst the gloom of eternal winter. Pieces of floating wood have been often found burnt at the extremities. In winter the intense cold continually occasions these mountains of ice to split asunder, and at each moment may be heard the explosion of these masses which separate in enormous chasms. In spring, the motions of the ice chiefly consist in the upsetting of those masses, which lose their equilibrium, because some parts dissolve more rapidly than others. In all seasons the

broken

broken ice accumulates in the passages or gulfs, and opposes equally the attempts of individuals who expose themselves on foot, and the progress of the vessel, whose motion becomes paralysed.

If to all these considerations be added, that the ordinary course of the ices from the pole depends upon two constant and eternal causes, the seasons and the currents, the removal of the obstacles is only local and momentary; and it will be allowed that the polar seas will never afford a commercial route. Immense benefit may result to the fisheries from the discoveries which they hope to effect.

The principal argument which has been made use of, to show that the great changes in the position of the polar ice must open a passage through those dreary regions, is the pretended physical revolution, which, it is supposed, has changed the face of East Greenland. A flourishing colony, say they,—a colony embracing several towns and convents, and containing a considerable population,—is seen all at once shut up from the rest of the world, by a vast barrier of ice. Beside this terrible catastrophe, probably every thing suddenly perished there; men, animals, vegetables, every living thing perhaps has been attacked at the same moment with a mortal cold. If, in our time, this barrier be removed, we shall surely find this mummy of a nation, this frozen Herculaneum; nay, who knows but that some remains may be traced of the ancient Scandinavian colony? or who can say, but that, in this spot, so long inaccessible, a people may be found, who shall have preserved the language, the manners, and the catholicism of the North, as they were in the fifteenth century?

This romantic prospect, however, vanishes before a critical examination of historical facts, gathered in the *Sagas*, works which have been much read since the discovery of Greenland. Nothing in their annals, though preserved in the bosom of their families, proves that Greenland ever enjoyed a milder climate. The establishments of the islanders there, were never more consolidated than those of the Danes on the western side, or of the British near Hudson's Bay. The voyages thither were not so frequent or so expeditious as has been supposed: these voyages sometimes occupied five years. In 1383, a vessel which arrived in Norway, brought there the first news of the death of the bishop of Greenland, who had died six years before. There were not many enterprising enough to undertake these voyages, and hence Greenland became the country of prodigies—the scene of the most wonderful events. For instance, Forfæus, a certain Norwegian, went over the ice from Norway to Greenland. In the latter country, he saw great forests, whose trees produced acorns as large as apples, and where he hunted large sea-bears! The sea there was inhabited by immense giants and giantesses: and there were to be seen mountains of ice, as lofty and as solid as those which impeded the entrance of the Argonautic expedition into the Black Sea!

These are the poetic tales of the old Scandinavians, who, like Jason and Hercules, sought out danger, and defied every peril. But history is more circumspect. The Iceland book, entitled "The Mirror of Kings," gives a just idea of this country. It shows, that ancient Greenland differed in nothing from modern Greenland. The coast, even in summer, was surrounded by enormous mountains of ice, such as the Norwegians had never seen in their own country. The colonists established there, knew nothing of bread, nor were they skilled in agriculture. They exchanged the teeth and furs of some animals for wood, which they wanted for firing, and for constructing their houses. The coast was uninhabited, except in those places where there was an abundance of fish: the interior of the country, occupied by mountains and vales covered with snow and ice, was as difficult of access as it is at this day. The number of the colonists did not exceed that of a large parish in Norway, nor would they have had a bishop, but for their great distance from the mother country. The Scandinavian colony in Greenland was divided into two cantons,—one west, which had but four churches, the other east, where there were two towns, or rather villages. This division gave rise to a great error in geography, from which it is supposed, that the eastern canton of Old Greenland occupied the coast opposite to Iceland; and those unknown regions have been pompously described as *Austurbygol*, in East Greenland: imaginary gulfs and promontories have been traced there; but this geographical system has been overturned by modern accuracy.

The first navigators, on going from Iceland to Greenland, steered south-west, shunning a coast surrounded with ice; and they doubled the point of *Hvarf*, which is situated on the continent, but has before it an island with a very large mountain, which they call *Chemise Blanche*. Following this route on the modern map, it is to be inferred, that the promontory in question is Cape Farewell, the known southern point of Greenland. The Icelanders then sailed north-west to arrive at the colony.

Other navigators have proved, that the point of *Hvarf* is the southern extremity of Greenland, from which it must follow, that East Greenland is only a portion, in fact, the most eastern and southern part of the west coast. The name of *Greenland* is justified only by the verdure of some tufts of grass and a few flowers, which show themselves in June, towards the south.

Higher

Higher up, nothing is to be seen but ice; and ice so accumulated and impenetrable, as to repel the boldest pirates. The current, whose continual direction is south-west, floats towards this coast, extending for ten degrees of latitude the sheets of ice which come from Spitzbergen. This has been the case for two centuries; and hence it is extremely probable, that this coast was never accessible. It is not here that the ice has lately disappeared; it is higher north, where the ruins of the old Norman hamlets and churches have been discovered. These ruins bear traces of violent destruction by a hostile force. Perhaps, also, the plague, which ravaged Europe in the fourteenth century, was extended to Greenland. The commerce with this colony having become a droit of the queen of Norway, the monopoly accelerated the decline of the establishment. But a bull of pope Nicolas V. proves that the destruction of the colony was caused by a foreign invasion; a fleet, nobody knows whence, attacked and devastated the country; all was annihilated by fire and sword. This fleet probably belonged to Prince Zichno or Sinclair, lord of the isles of the Orcades and of Finland, whose two brothers Nicolo and Antonio Zeni decried the expedition as piratical.

These explanations, which make the wonders of East Greenland disappear, have not been received without examination, and without opposition. In a memoir printed in the Danish language, and which is deposited in the Royal Library at Paris, a lieutenant of marine, M. de Wormskiold, attempts to prove that old East Greenland must exist on the eastern coast between the 62d and 64th parallels of latitude. These arguments are taken from the "Voyage of Danell," a book not deemed authentic.

Denmark has left nothing untried to decide the problem. Many attempts have been made by that power, both by sea and land. In 1788, Lieutenants Egede and Bothe sailed up the eastern coast as far as 63 degrees parallel: the ice prevented them from going further. The coast presented the most dismal prospect.

Nothing discovered in modern times proves those changes in the climates, those great physical catastrophes, with which some writers, full of imagination, have endeavoured to animate the picture of the polar regions.

The idea of being winter-bound in the midst of the glacial regions, frightens the imaginations of those who have read the relations of Berendt and Heemskerk: but these two Dutch mariners had foreseen, had prepared nothing; shipwreck consigned them, without defence, to the horrors of a polar winter. The English navigators have calculated the dangers, and provided the means for withstanding them; they do not appear to doubt the possibility of supporting the cold, even under the pole:—perhaps nature may furnish them with some facilities which they do not expect.

expect. The polar countries, such as Iceland and Greenland, abound in warm, and even boiling, springs. Who has not read the descriptions of the Geyser, that marvellous boiling water-spout, which surpasses in magnitude all the *jets d'eau* which art has produced? But the œconomical use which may be made of these singular fountains, is not so generally known. The historian Snorron, the Herodotus of the North, and a long time judge or president of the republic of Iceland, made a bath for a hundred persons to be constructed, which still exists; and the water of which is furnished from a natural source. Some monks, who settled in Greenland in the middle age, went a step further; they heated their monastery with the vapour of boiling springs. The following is the curious account of it given by the brothers Zeni.

"There is," he says, "in this place (the monastery of Saint Thomas) a spring of boiling water, with which the monks heat the church, the refectory, and their cells; when it arrives at the kitchen, the water is still so hot, that they have no need of fire to prepare their meats. To make bread, it is sufficient to put the paste into copper-vessels, and to hold these in the water; the bread becomes baked in this manner, as if it were in an oven. He found also in this monastery, small gardens in full vegetation in winter; the monks irrigate them with this water, and by this means grow flowers, ripen fruits, and rear different sorts of plants, which vegetate as well as if they were favoured with a temperate climate. The rude savages, who inhabit these countries, astonished at effects which they regard as supernatural, take the monks for gods; and carry them all sorts of presents, such as birds, meat, and various other things."

Although the situation of this monastery cannot now be traced, the relation is too circumstantial to permit of our supposing it to be an imposture, though, perhaps, there may be some confusion in respect of places. "The monks," continues the narrative, "employ no other materials for the building of their monastery, than what are supplied by the neighbouring volcano; they take, for this purpose, the stones which are ejected in the form of scoria from the crater of the mountain; and while they are yet hot throw water upon them, by which means they are entirely dissolved, and converted into an excellent lime. The scoria, when it is cold, serves in place of stones to form very solid walls and arches; for when once cooled, it cannot be broken but by an instrument of iron. The arches made with this scoria are so light, that they do not require any support. The want of rain is never felt as an inconvenience in this country; for the first snow which falls remains frozen for the space of nine months, the time which winter lasts. The people live upon wild birds and fish.

"The

"The hot water of the volcano, falling into a large haven, prevents the sea from freezing, in consequence of which, so great a quantity of birds and fish are attracted to this place, that the religious draw from them as much as is necessary for their own subsistence, and for that of a great number of the inhabitants of the country, whom they constantly employ in building, in hunting, in fishing, and various other occupations."

The narrative treats afterwards of the barter trade, which these monks carry on with Norway and Iceland.

"During winter a great number of ships are always to be found here, which cannot get away in consequence of the sea being completely frozen, and therefore wait the return of spring."

The old chart, designed upon wood, which exhibits the course of the navigation of the brothers Zeni, represents Greenland under a figure approaching that which is now given to it, and under the double name of *Grolandia* and *Engroenland.*

The monastery of St. Thomas is placed there at three degrees northward of the island; a position which answers to that of *Point Charn,* where the whalers have met with floating pieces of hollow stone. It is also about this place that Volkart Boon, a whaler of Sleswick, discovered in 1761 a large gulf.

The details which the Zeni give with respect to the houses and canoes of the savage inhabitants of the country, coincide exactly with all that we know of the present Esquimaux of Greenland.

"The canoes of the fishermen of *Engroenland* have the form of a weaver's shuttle; they are made of the bones of marine animals, covered with several plies of fish-skins sewn together. These canoes are so impermeable and so solid, that in the greatest tempests, the persons who are in them content themselves with remaining at their ease, without caring where the winds or waves may bear them, well persuaded that their canoes run no risk of being broken or submerged; should it even happen that they are thrown upon a rock, they sustain no damage."

It would be a singular event to see the English re-discover the monastery of Saint Thomas, or some similar locality. If in the fourteenth century, some *poor preaching friars* were able to create a commodious habitation and a smiling garden in the midst of eternal ice, what may not the enlightened industry of the nineteenth century achieve? Were a small colony of learned men to sojourn for some years in a similar place, it would enrich science with precious discoveries. Magnetism, universal gravitation, the aurora borealis, and many other phænomena, could in no place be observed with more advantage.

[To be continued.]

XLVIII. *On*

XLVIII. *On extinguishing of Fires in Buildings. By Mr.* JOHN MOORE.

SIR,—OBSERVING the destruction of property by fire, and the fright and inconvenience to families when it occurs in dwelling-houses, with sometimes loss of lives;—and after taking a survey of the progress of the arts, I am surprised, that recourse is not commonly had to the mixing of some ingredient with the water employed, (as there are many known,) for the more immediate extinguishing of that destructive element. The importance of the subject is so considerable, that I think it ought to have the most serious attention.

To the uninsured, a means of speedy extinction would be a happy resource, and to the public a great acquisition, provided the expense be but trifling. Now in order to stimulate others towards the obtaining so desirable an object, I take the liberty to state to you the ideas that have occurred to me, hoping that improvements on them, or the selection of some more effectual means, will be the result;—therefore, without further introduction, I beg to submit to your consideration what I conceive would be serviceable.

I would have every fire-engine provided with a few sacks of ground clay in powder; the clay to be ground after it is dry and then sifted, in order that no large fragments of it may lodge between the valves, so as to prevent the working of the engine. I doubt not but you will observe, that the greater the quantity of clay and water which passes through the pipes to the fire, so much the sooner the fire must be extinguished; because the clay contained by the water will form a crust, and act like an extinguisher; by which means the flames will not only be prevented from extending their destructive progress, but may, by a judicious application of this clay water, be easily brought under. For clay being uninflammable, wherever it falls in sufficient quantity, it will cut off the communication between the fire and air, and thereby exclude the accession of oxygen to support the flame, which will consequently go out.

Alum is also an excellent ingredient to mix with water; because it has no tendency to inflame, and will also form an extinguishing cap or crust like clay, with which I have no doubt most of your readers are well acquainted: but if any of them should not, let them throw a piece of alum on any common fire, and they will be convinced of the truth of the observation. There is, however, one objection to the employment of alum, namely, the expense; and this is likely to keep it out of use, though its efficacy were much greater.

But the best substance of any for this purpose, is, in my opinion, burnt lime, exposed to the atmosphere that it may absorb moisture,

moisture, and thereby fall to powder. This, after sifting and being mixed with water, when thrown on fire will be found almost instantly to extinguish the flame. Indeed it has come under my notice more than once, that water impregnated with only the quantity of lime that it is capable of holding in solution, always had a very increased effect in extinguishing fire; for, at a fire that recently occurred, it was observed, that if any burning piece of wood was extinguished thereby, it would not rekindle. Since such was the effect of lime-water, which contains so small a quantity of lime, will it not immediately put out flame, when the lime is thrown in a larger body with the water? and will not each engine be enabled to throw its water a much greater distance, as its density will be much increased by the mixture of either of the foregoing substances?

If the dust of the turnpike roads was collected, and sifted from its grosser particles and kept for use, it would be found of great benefit; because, most stones that are used on the roads being of a limestone nature, the dust of them when thrown on the fire will become lime, and consequently have much the same effect. There is moreover a considerable advantage in the ease with which it may be procured*.

To show the utility of mixing something uninflammable with the water, I need only mention, that, at a fire at which I once assisted, it was observed, that one of the engines operated much more powerfully than either of the others; and wherever its water came, the flames appeared to be almost instantaneously subdued, whilst the other engines often seemed rather to be increasing than diminishing them. Upon inquiry I found that this efficient engine was supplied with the waste water that was spilled in the street, which was afterwards taken up in buckets, water and dirt together, and thrown into this engine. Is it not therefore reasonable to conclude, that the superiority of it was from the mud being for the most part uninflammable?

Besides making each engine carry a reasonable quantity of clay, &c. it might be adviseable, that each watch-house or other convenient places should be provided with a sack or two. Were this done, no fire could possibly take place in any part of a city, without some clay, &c. being at hand, always in a state fit for use†.

I am, &c.

Bristol, Feb. 21, 1818. JOHN MOORE.

* Where lime forms the principal ingredient in the materials employed for making and repairing the highways; the road-dust, as suggested by the author, might answer very well; but where siliceous ingredients form a portion of the materials, such dust would grind the pump-work of the engines to pieces in a very short time. EDIT.

† Mr. Moore's communication also contained some hints for extinguishing

XLIX. On

XLIX. *On the Preparation and Use of Copal Varnish as a Vehicle preferable to Oil for the Purposes of the Painter.* *By Mr.* Cornelius Varley.

To Mr. Tilloch.

Sir,— I beg, through the medium of your valuable Magazine, to lay before artists the method of preparing pure copal varnish in spirits of turpentine *, without heat. It is quite transparent, colourless, durable, protects the colours, and does not change in the least. This is the purest material I have yet found, and is the only vehicle I use to paint with.

Take the cleanest and whitest lumps of copal; beat them small, and pick out all the impurities. Pound them to a fine mass in a glass or Wedgwood's mortar; then pour in colourless spirits of turpentine to about one-third higher than the copal, and work up the whole quite fine; in half an hour work it up again till fine (if left too long, it will get so tough as not to be rubbed up again); and in an hour work it up again, and once or twice more in the course of the day. The next morning it may be poured off into a bottle for use; but as it is thicker or thinner according to the quantity of turpentine and the heat of the weather, it should be tried as follows, before bottling it up: Dip a palette-knife in, and dry it by the fire as quick as you can without burning it; and if, when cold, it is found to have left on the knife a fair coat of varnish, it is strong enough: otherwise, work it up again, and let it stand some time longer. After taking off this first supply, pour on a fresh quantity of turpentine, and rub it up several times during two or three days;—try it by the palette-knife as before; and when strong enough, pour the liquid off into the same bottle with the first. A third quantity of spirits might be added, which would make the remaining copal appear as dry as crumb of bread; but a much longer time than before would be required for the solution.

To paint with this varnish, use powder colours ground quite fine; or else grind them in spirits of turpentine, and add as much of the varnish as will bind them well. Keep them in bottles, and mix your tints in saucers for use; and as they thicken by

ing fires in ships; but as the idea had already been anticipated in our xxist vol. p. 97, to which our readers can turn, it was unnecessary to insert them here. He also suggests that ships might be rendered more buoyant by making them *air-tight*, and forcing in air by means of an air-pump, which would elevate them to a higher level in the water, and consequently might sometimes save them when they have got upon a bank.

* The common spirits of turpentine seems to dissolve copal best; but when that cannot be had colourless, I use the rectified spirits.

 drying,

drying, thin them with pure turpentine. I purposely make the varnish so thin as not to shine when the paint is dry; but if it work too dry, moisten with a little more varnish till right; and if it become too thick so as to work gummy, add powder colour and turpentine till it is brought to a proper consistence. I keep these saucers of colour clean in a small chest of drawers, by which I have a great many in a little compass; and the colours are continually set for use, as they only require moistening in the morning with turpentine. I do not however always wait to soften them in this way, but put fresh colour upon the old, out of the bottles.

The colours work freest, and dry quickest, when they do not shine, and will remain exactly as you put them on; but if there is too much varnish, and the canvass is painted frequently over in the same day, the sharp touches will spread a little; a fault which must be avoided by using no more varnish than just enough to shine, and by drying the part more perfectly before painting on it. In cold weather, the drying may be effected by the fire, or by holding a hot iron before the canvass; in cold weather, you must not expose the painting to a hot fire directly afterwards, as it would melt and run, but let it dry two or three hours, and then bring it to the fire to finish. The picture may be varnished with the same varnish made thicker (if you wish to be sure of the purity of the materials), and when varnished, dry it as soon as convenient by gradually approaching the fire, avoiding a heat that would melt the copal. After this perfect drying, no dirt or smoke will ever stick to the picture; but it may be dusted or washed quite clean with pure water, and will never change.

The next varnish I generally employ is made in the following manner: Take the sediment of the first varnish or fresh copal beat small; cover it to about twice its height with turpentine in which camphor is dissolved, (an ounce to a quart,) cork it up, shake it well, and put it by for a year, at the end of which it will be dissolved. The first portion that is dissolved thickens the turpentine, and enables it to dissolve the remainder; but if the turpentine is too much diluted, the remainder will never dissolve, so that it is easier to make thick camphor varnish than thin. It must not, however, be made too thick at first; but when that which is put by is thick enough, pour it off for use, and add fresh camphor and turpentine to the remainder, and put it by again till dissolved, which it will be in the course of time. If you want that which is put by before it is quite dissolved, it may then be put in a saucepan of cold water with the cork loosened, and made to boil for an hour, stirring it well up two or three times towards the end, and that will finish the dissolution.

I paint small pictures on the finest mill-boards glued to a deal frame,

frame, and I take care to prevent the larger pictures from swagging, by not allowing the canvass to be tightened afterwards, but cutting the picture off the frame on which it was painted, and then pasting it to very tight canvass protected from swagging by pannels. This mode of preventing cracking is of real advantage; for all pictures will imbibe the smoke of London, and become tinged in proportion to the softness which is had recourse to for this purpose.

A third varnish is quickly made by oil of spike-lavender, which is very good for drawings or prints, but will not do for pictures, as it will dissolve the paint underneath and run down while drying. I remain, sir, Your most obedient servant,

42 Newman-street, May 17, 1816. CORNELIUS VARLEY.

L. *On the Saltness of the Atlantic Ocean.* *By M.* GAY-LUSSAC*.

M. LAMARCHE, a distinguished officer of marine, collected the water of the sea at different latitudes during a voyage from Rio Janeiro to France in the year 1816, and on his return to Paris he brought it to me in order to submit it to some experiments. The water had been taken from the surface of the sea, and preserved in glass bottles closed with stop corks, and for the most part tarred.

I at first proposed to myself to examine the nature and the proportions of the saline substances which they contained; but I found myself relieved from the necessity of such examination, by the analysis of the water of the Frith of Forth by Mr. John Murray, who appears to have made it with much care, and from which it results that this water contains in 100 parts

Muriate of Soda	2·180
——— Magnesia	0·486
——— Lime	0·078
Sulphate of Soda	0·350

I contented myself with determining the specific weight and quantity of the saline matters of each specimen. The experiments were made in my laboratory by M. Despretz with all possible care. The specific weight was taken, by weighing successively the same vessel, first empty, then full of distilled water, and afterwards full of sea-water at the constant temperature of 8°.

The total quantity of saline matters may be determined by analysis, in the manner adopted by Mr. Murray; but it is more simple and more exact to determine it by evaporation. This operation is done very conveniently by putting the water in a phial,

* From the *Annales de Chimie* for December 1817.

 and

and continually agitating it while upon the fire, with the neck inclined at an angle of about 45°. The residue gives exactly the weight of the saline matters. It is easy to estimate the quantity of hydrochloric acid proceeding from the decomposition of a part of the hydrochlorate of magnesia contained in the sea-water, by collecting the magnesia which remains, when the residue after the evaporation is dissolved in water; since the relation in which these two bodies enter into combination is well known. The quantity of magnesia furnished by each residue being too small to be estimated with precision, all the residues were collected into one; and after separating the magnesia from the whole mass, it was divided proportionally to the weight of each residue. As it is very probable that this base exists in sea-water in the state of chlorate of magnesia, the weight of each residue was corrected by subtracting from it that of the oxygen contained in the magnesia obtained, and adding to it the weight of chlorate saturated by the corresponding quantity of magnesia. The results have been collected in the following table. The water which is designed by *Calais* in the column of latitudes was taken up by myself in the midst of the channel between Dover and Calais. The saline residues proceed from 100 parts of sea-water.

Latitude.	Longitude.	Density.	Saline Residue
Calais	.. ..	1·0278	3·48
35° North	17° West	1·0290	3·67
31 59′	23 53′	1·0294	3·63
29 4	25 1	.. ..	3·66
21 0	28 25	1·0288	3·75
9 59	19 50	1·0272	3·48
6 0	19 55	1·0278	3·77
3 2	21 20	1·0275	3·57
0 0	23 0	1·0283	3·67
5 2 S.	22 36	1·0289	3·68
8 1	5 16	1·0286	3·70
12 59	26 56	1·0294	3·76
15 3	24 8	1·0284	3·57
17 1	28 4	1·0291	3·71
20 21	37 5	1·0297	3·75
23 55	43 4	1·0293	3 61
Mean ..	.. ..	1·0286	3·65

The densities, in spite of every care being taken, differed frequently, and in an irregular manner, from each other. The quantity

quantity of saline matters presented also slight variations, which did not always correspond to the variations of density; but it is possible this discordance may have proceeded from the saline residues not being calcined exactly in the same degree. On taking the mean of the densities, and that of the saline residues, the former was found to be 1·0286, and second 3·65.

If we may be permitted to draw a conclusion from these experiments, the densities prove that at the latitude of Calais and of 10° N. the saltness of the sea is almost at its minimum; that it is strongest at 35° and 32° N. lat.; that it goes on afterwards diminishing until near the equator, and that it increases in the other hemisphere, where at the latitude of from 17° to 24° it is the same as at 35° and 32° N. lat.

According to the saline residues, the saltness is at its *minimum* at the latitude of Calais, and that of 10° N. It augments afterwards, although in an irregular manner; and appears a little stronger in the southern and in the northern hemispheres. By thus comparing the densities with the saline residues, it seems that the saltness of the sea is less in the canal of Manche and at 10° of latitude N. than any where else; and that it is somewhat greater in the southern than in the northern hemisphere. But before drawing any general conclusion, it may be proper to compare the preceding results with those obtained by other chemists or by voyagers.

According to the recent analysis of Mr. Murray, the water of the Frith of Forth only contains 3·094 of saline matters; but this result is certainly too weak, and cannot perhaps be applied to waters taken more at large. If it were exact, it must be concluded from it that the saltness of this frith is modified by the rivers which fall into it. Lord Mulgrave, in latitudes further north of 60, 74, and 80 degrees, has found that the water of the sea taken at a depth of 120 metres contains 3·40, 3·60 and 3·54 of saline matters. The water collected by Pages in different parallels from 45° N. to 50° S. contained nearly four centimes of saline matters. Bergman has found 3·60 in water taken near the Canaries; and MM. Bouillon Lagrange and Vogel 3·60, and 3·80 in water in the latitude of Dieppe and Bayonne, and 4·10 in the Mediterranean opposite Marseilles.

It is not surprising that the results obtained by different persons should differ, since they may not have employed the same means of analysis; but all of them unite in proving that the water of the ocean contains at least three centimes and a half of saline matters. This result agrees also with the average of the extreme degrees of saltness collected by M. Humboldt.

It is not easy to decide, whether the degree of saltness varies with the latitude. The densities of the water collected by M. Lamarche

marche do not accord with any general rule. The density of the water at 32 and 35 degrees N. is as strong as that of the water of the tropics. M. Humboldt, whose attention was directed in his celebrated voyage to all the great phænomena of nature, has observed, after comparing the results of his experiments with those obtained by other voyagers, "that the experiments hitherto published do not justify any opinion being entertained that the sea is salter under the equator than under 30 and 44 degrees of latitude."—(*Relat. Hist.* i. 74.)—According to Pages, the saltness of the sea is constant and equal at four centimes from 45° N. to 50° S. However, according to the experiments of Bladh on the specific weight of sea water, it would appear that the water is salter towards the tropics than even under the equator. Some similar experiments of Mr. John Davy, which have been published in the Philosophical Transactions for 1817, prove that from 30° to 35° on one side as well as the other of the equator, the specific weight and consequently the saltness is exactly the same, but that it is a little stronger under the northern than under the southern tropic. And lastly, M. Humboldt, by the use of an excellent aërometer constructed by Dollond, thought he could discern that the water is less salt between the tropics than from the coasts of Spain to Teneriffe.

From this discordance between the results, which may depend on the process of analysis, or on local causes; from the circumstance especially that the results of different voyagers range often in contrary directions, we may conclude that the saltness of the great ocean, if it is not the same every where, presents at least very slight variations. But every incertitude which remains on this subject, will doubtless be removed on the return of M. Freycinet, who is to bring home, in vessels closed with emery, water taken in a great number of places in the two hemispheres.

Looking in the mean time to the subject in a theoretic point of view, it is easy to be convinced, that in general there can be but little difference between the saltness of the sea at one place and at another, if we except some local causes, and particularly the river waters, which may occasion a good deal of variation.

LI. *On the Practicability of a direct Passage over the North Pole*.*

Hull, Feb. 27, 1818.

Mr. Hydrographer,—When the public attention is so generally directed to the expedition now fitting out to explore the Arctic regions, it is to be supposed that individuals who take a

* From the Naval Chronicle for March 1818.

lively

lively interest in the objects it has in view, will estimate, in their own way, the probabilities, and the obstacles, that appear to them to weigh for, and against, its success.

The appearance of an article in the Quarterly Review this month, on this subject, has led me to consider some of the matters there stated, and to inquire into the solidity of some of the writer's notions on this interesting topic. In the first place—The disappearance of a whole, or a great part of the impenetrable barrier of ice which had shut up a large portion of the eastern coast of Old Greenland, seems to be authenticated by persons entitled to credit; and it *may be*, that this "accumulated barrier of ice, probably by its own weight and magnitude, and the action of the current together, at length burst its fetters." But at the same time it may be observed, that, if this barrier possessed the same strength and compactness as it did during the many hundred years it is said to have held its post in defiance of it, it would have remained there still. We must, therefore, look for some other cause to account for a diminution of its strength; and this cause might perhaps be, winters unusually mild, without frost sufficient to keep its parts so consolidated as heretofore. Accounting thus for a reduction of its strength, the same current it had so long resisted might separate it. The removal of this ice being "cotemporaneous with the period when the western declination of the magnetic needle became stationary," is certainly a "remarkable coincidence."

That there may be some connexion between this disappearance of large masses of ice, and the power of magnetic attraction, is probable enough, from this coincidence. Yet all we can gather is, that the power of magnetic attraction increased with an accumulation of ice, and became stationary about the period of its removal from particular places. Whether there exists any combination of causes—whether the connexion is between the ice and the grand focal point of magnetic attraction, which some philosophers suppose to be situated in the earth, or whether it is between the ice, and electricity in the atmosphere, or the *aurora borealis*, or all these together, can as yet be only matter of mere conjecture; as are opinions of the cause of the *aurora borealis* itself.

Beccaria conjectures, "that there is a constant circulation of electric fluid from north to south, and that the *aurora borealis* may be this matter performing its circulation in such a state of the atmosphere as rendered it visible, or approaching nearer than usual to the earth." Dr. Halley imagines "that the *aurora borealis* is produced by a kind of subtle matter freely pervading the pores of the earth; and which entering near the southern pole, passes out again with some force into the æther at the same distance from the northern.

Franklin supposes, "the electric fire discharged into the polar regions from vaporised air raised from the ocean between the tropics, accounts for the *aurora borealis*; and that it appears first, where it is first in motion; namely, in the most northern part, though the fire really proceeds northward." Father Boscovich determined the height of an *aurora borealis*, and found it 825 miles. Mr. Bergman, from a mean of thirty computations, made the average height of the *aurora borealis* 469 miles; but Euler supposes the height to be 7000 miles; and Marian also assigns to them a very elevated region. Thus discordant and various are the conjectures of philosophers on this matter.

At all events, in whatever way the supposed connexion may be between the removal of the ice and these phænomena, it "seems not unfair to infer, that the departure of the immense mountains and fields of ice, which for so many centuries have *covered* the arctic seas, may have had some effect in stopping the barrier of the western declination of the needle." But we may *as fairly* draw the *same* inference from a similar cause, though probably of much *less extent;* and all we *can* know, till the whole of the arctic regions are explored, is, the departure of perhaps a very small portion only, of "these immense mountains and fields of ice," which had collected in the *vicinity of Greenland.* What may still remain in the arctic seas we are yet to learn; and concerning which, like every thing else where facts and local experience are wanting, our opinions can only be formed on some theory built on fixed, and generally received principles.

The fact, however, of the disappearance of *some* large mountains and fields of ice from *part* of the arctic regions being admitted, the writer of the article in question then "inquires whether any, and what advantages, may arise out of an event, which for the first time has occurred, at least to so great an extent, during the last four hundred years;" and among the objects most interesting mentions these: 1st. The influence which the removal of "so large a body of ice may have on our climate."

On the benefits we should derive from an amelioration of our climate, there can be but one opinion. And that our summer seasons have been colder than usual, in the latter years for instance, are from the causes he assigns, few will doubt. But the effect produced may not continue. For though the principal cause of the chillness of our climate, compared with what it appears to have been centuries ago, may be removed *for the present*, yet the grand primary cause which produced the ice, whose approximation deterioriated our climate, it is to be presumed,

will

will continue to operate; and what has happened by the established general law of nature may happen again. Therefore, though it may be *hoped*, it certainly would be "unreasonable to *presume*" that merely on account of the present accidental removal of some portion of ice," our summer climate (and winter too, when the wind blows from the western quarter,) may henceforth improve." Though, no doubt, it will improve, *if* the ice does not again collect in the places from whence it has lately been dislodged. But surely we have more *reason* to fear it *may*, because it has done so before, than to presume we shall "henceforth" have no more huge ice-bergs drifting down to the southward in the wind's eye of our island, and that *therefore* our climate may improve. For, whilst the universe continues to be governed by the unerring and unalterable general laws of God, mountains and fields of ice will doubtless continue to be formed in the polar regions of the north; and whenever the winters are successively severe there, they must accumulate; and, no doubt, find their way to the southward as they have done.

The 2d object is "the opportunity which the *local* disappearance of the ice affords of inquiring into the fate of the long lost colony, on the eastern coast of Old Greenland." It must be admitted to be favourable for this object, so highly interesting to humanity and science, as well as to curiosity; and should the east coast of Greenland continue to be free from ice, as it is said to have been last year, it is probable it may be attained.

The 3d object, viz. "the facility it offers of correcting the very defective geography of the arctic regions in our western hemisphere. Of attempting the circumnavigation of Greenland. A direct passage over the pole. And the more circuitous one along the northern coast of America into the Pacific." Certainly "any event that tends to encourage the attempt to amend the very defective geography of the Arctic regions, more especially on the side of America, may be hailed as an important occurrence." But let us see whether, what may be only a *local* and very partial removal of ice, collected in the vicinity of Greenland, is likely to facilitate *more* than an examination of its eastern coast, or, at most, its circumnavigation, and, perhaps, of exploring the coast of America some distance to the north-west of Cumberland Island, if not to its north-eastern extremity. It is very true, that "several circumstances may be adduced in support of the opinion, that Greenland is either an island, or an archipelago of islands;" and none stronger than, the perpetual current stated to set down "to the southward, along the eastern coast of America, and the western shores of Greenland."

But this current, though affording "a strong presumption" that between Davis's Strait and the great polar basin there is

some

some communication; yet surely it does not authorize us to presume that there is "an uninterrupted communication." On the contrary, it seems probable, that there may be islands or shoals between the north-west coast of Greenland and the north-east coast of America, among which smaller masses of ice, trees, and whales too, as well as the current, may find a passage down Davis's Strait from the polar basin; but which may be, and probably are, so blocked up generally by mountains and large fields of ice, as to present an impassable barrier *for ships*.

On account of the current, it is certainly fair to presume, that the northern part of Davis's Strait is misnamed in the charts as "a bay;" for if it were one, it certainly "would be very difficult to explain, how a current that runs to the southward perpetually, with a velocity of four and sometimes of *five miles* an hour, could originate in the bottom of it!" *If* there is an uninterrupted communication, that is, if there is no land, no shoals in the whole space between Greenland and America, it appears very probable that greater quantities of ice would pass through that space with a current of *such velocity;* and less find its way round Greenland. But we must endeavour, first, to decide in our own minds, as well as we can, how and where the ice in the polar regions is formed. In what direction it is *probably* impelled by the winds and currents. How these winds *probably* prevail in summer, and winter, and how the current *probably* sets underneath, as well as at the surface of the water. For, notwithstanding the writer of the article I am examining, apprehends it will be "found, that the currents of the ocean, where no land intervenes, are entirely superficial;" and though he says, "it would be difficult to explain the perpetual egress of a current, from the polar basin into the Atlantic, without admitting a supply through the only remaining opening (Behring's Strait,) into that basin, to answer the demand of the current," I yet firmly believe there must be a continual *under* flow of water in the ocean, as well as superficial currents. Otherwise, that "universal motion of the great deep," which he, and all must allow, cannot be satisfactorily accounted for. How then, it may be asked, are these lower currents to be accounted for? The question is much easier to be put, than solved to the satisfaction of others. But I will endeavour to explain the ideas I have on the subject as well as I can; and that, too, with all the diffidence of one who knows that, though conjectures *may*, perhaps, be well-founded, their truth depends on experiment.

The conjectures I venture to offer, are founded, however, on the known and acknowledged properties of heat and cold. Heat is known to be the general cause of the expansion of air, and cold the cause of its compression. Heat rarefies, and cold condenses.

The

The influence of the sun, in rarefying the atmosphere to the greatest degree, between the tropics, together with the earth's rotation on its axis from west to east, would produce a constant wind from east to west, all round the globe, *if* no land intervened; because, the points of greatest rarefaction being successively westward; and those eastward of each other, parting successively, as the sun sets in their horizons, with part of the heat received in his passage over them, the motion of the atmosphere nearest the surface of the water, must necessarily be from east to west; following the apparent motion of the sun. We find this proved by fact, on those portions of the globe where the general law is not obstructed by causes of an opposite nature, arising from terrene influence; viz. in the great Pacific ocean, between America and the coast of New Holland, and also in the open sea, between Africa and America. The central medium line of greatest rarefaction, is the equator; but, according to the sun's declination, north or south, it will be more to the northward or southward. The air, thus rarefied in the lower regions of the atmosphere surrounding the earth, and comprised within the limits of the sun's path between the tropics, must be continually ascending into the higher, and thence, north of the equator, advancing towards the north pole; and south of the line, towards the south pole, till, *somewhere* in its passage, it acquires that degree of condensation by cold, which compels it again to return in the lower strata, to the point of greatest rarefaction, to undergo the same process. This seems to be the grand *general* law of nature's operation upon the atmosphere; that by "universal motion, it may be preserved in a state of purity." Let us now inquire, whether this same law is not equally applicable to that universal motion of the great deep which must be equally necessary to *its* purity, and therefore we certainly may presume, does take place on *some general* principle. We indeed already know, that the waters of the ocean in the Pacific, and in the Atlantic between the tropics where least obstructed by land, move at and near the surface, in a similar direction, nearly and generally, to that of the wind. When obstructed by lands, they take the various turnings and windings, which the forms and trendings of those lands, and other local causes, impose on them.

If it be allowed, that the influence of "the sun, in rarefying the atmosphere to the greatest degree between the tropics, together with the earth's rotation on its axis from west to east, would produce (if no land intervened) a constant wind from east to west," may we not suppose, *if* the same causes operate similarly, but proportionably, on the waters of the ocean, that they must produce a similar effect, and oblige them to take a like direction? That is from east to west, at and near the surface

face

face all round the globe, within the limits of the sun's declination. If this general effect be admitted, then, on the ground it rests, we may presume that, if there was a passage through the isthmus of Darien for the immense body of water which continually flows from east to west, into the Caribbean sea and gulf of Mexico, what is called the gulf-stream would no longer exist. And as it seems probable, that the surface of the water must be somewhat higher on the eastern side of America, *thereabouts*, than on the other, owing to the land's obstruction to the natural course of the great equinoctial current, and the necessity imposed on it, to find vent through the gulf of Florida into the Atlantic, it is not unreasonable to conclude, that, if this accumulation of water was at liberty to flow through the continent of America into the Pacific Ocean, the surface of the sea on this side would be lower than it now is; and parts of land, now under water, would be exposed to view. This effect would however be injurious to commerce with the West Indies, for it would render the homeward-bound passage more difficult. Instead of a constant weather current to assist ships, it is pretty certain there would be a lee one from the N.E. along the east coast of Florida, and its influence would most probably be felt far up to the N.E. from whence the current of colder water would flow nearer the surface than it now can, covered superficially as it may be supposed to be by the warmer gulf-stream. The high degree of temperature which this great body of water acquires by the sun's constant action upon it, being slowly reduced during its *propelled* progress to the N.E., it is probable that it may advance even beyond the Banks of Newfoundland before it is reduced to the colder temperature of the fluid below it, which must be flowing from the northern regions of condensation towards the points of greatest rarefaction and evaporation between the tropics, to supply the place of that which the heat is, as constantly, rarefying and evaporating; and so sending back in the upper strata of the atmosphere to the colder regions. The gulf-stream thus propelled by lateral pressure up towards the Banks of Newfoundland, is seldom found to affect a ship beyond those banks; at the same time, it is possible, that some of it may advance further to the northward, before that reduction is effected in its temperature which gives it a tendency to the southward; for many articles, the produce of tropical climes, and some known to have been from the West Indies, have been cast ashore on the coast of Europe. Some of these places being to the N.E. of Newfoundland, it is difficult to believe these articles could have been driven thither by the winds and the swell of the sea *only*. For, these prevailing as much from N.W. as S.W. would give them about an east direction; and if they were immersed sufficiently

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to feel the force of the great *under flow* of cold fluid from the north, which brings the ice bergs down to 39° or 40° of latitude, they would move in an *east southerly* direction. It seems therefore reasonable to suppose, that there may still be the remains of a northerly movement of water, at and very near the surface, to cause bodies floating there, to make course, as some *have done*, to the north even of E.N.E. from Newfoundland. The great body of the gulf-stream is, however, much reduced in temperature about the Banks of Newfoundland; and in proportion as it feels the cold of the great under-flow from the north, it is turned gradually to the eastward and southward past the Western Islands. Whether any part of it reaches the coasts of England, France, Portugal or Spain, is a point much disputed. It is possible, however, that it may, diverging as it appears to do to the eastward and southward, some of the fluid that composed it *may* find its way to the northward of Cape Finisterre, and add something to the great body of water which the western swell heaves into the Bay of Biscay; and proceeding to the northward along the coast of France, sets over from Ushant beyond Cape Clear; till meeting in that quarter with a fluid below, of a colder degree than its own, perhaps gradually joins the polar stream to the southward according to its depth and temperature. Some of the waters of the gulf stream it is possible (though hardly that), may assist in supplying the water expended by evaporation in the Mediterranean, whose surface, therefore, it is presumed, must be lower than that of the Atlantic, as the constant current setting into it, seems to prove. Some philosophers, indeed, suppose that the quantity of water continually admitted through the Gut of Gibraltar into the Mediterranean, is greater than can be expended by evaporation; and that therefore, there must be a *counter* current setting out *underneath*. To establish this opinion, it seems necessary first to prove that the temperature of the Mediterranean is lower generally than that of the Atlantic which flows into into it. For, if it is higher, (as is more probable,) the surplus (if there was any, and allowing their surfaces to be equal) would, I presume, run out at the surface, and the supply be received in underneath; which is contrary to fact—though I have supposed it barely possible, that some of the gulf stream *may* cross the Atlantic, I by no means say that it is so. On the contrary, it is little felt by ships far to the eastward of the Azores; but in the vicinity of those islands, the S.E. portion of it gradually turns to the southward, and as it advances in that direction, soon feeling the impulse again of the grand equinoctial current, is compelled to partake of its western motion. Thus forming a sort of circular eddy, which may be comprised between the latitude

tude of about 18° or 19° north, and the parallel of the Western Islands; and from about the longitude of 29° to 43° west; within which limits the gulf weed is found floating on the surface, where I suppose it originates, lives its appointed time, and decays like any other vegetable production; and I believe it is rarely or never met with beyond these limits.

Though I have admitted the *bare possibility* that some of the gulf stream may enter the strait of Gibraltar, I cannot agree with the writer of the article in the Quarterly Review, when he says (speaking of the gulf stream) "that it is of sufficient force and quantity to make its influence be felt in the distant strait of Gibraltar." Thus implying (if I understand him right) that this "force and quantity," of the gulf stream, are the primary *causes* of the constant current into the strait. On the contrary, thinking as I do that the causes of this constant flow of water into the Mediterranean, are of a purely local nature, connected exclusively with that sea; I therefore think it most probable, that if the great equinoctial current flowed (as I presume it would, were there a sufficient passage,) through the continent of America into the Pacific, and consequently *annihilated* the present gulf stream, there would be the *very same* constant flow of water into the Mediterranean that there is now, as long as the sun's power continued, and the localities exclusively belonging to that sea remained the same. In short, I am of opinion that the waters of the Atlantic (approximate to the strait of Gibraltar), feel the influence of purely Mediterranean causes; and that neither the "force" nor "quantity" of the gulf stream has any effect whatever in *causing* the current that runs into the Mediterranean. It is well known by experience, that this current is strongest with easterly gales in the hottest weather, *with wind* at the same time, and is diminished during the prevalence of the westerly winds; and is weaker in winter generally than in summer. But to return.—The winds, and surface currents in the PacificOcean, are influenced generally in a similar way by the sun's power as those between Africa and America, making however due allowance for the difference of the formation and positions of intervening lands, for these obstruct the uniform general tendency of the winds and currents from east to west: therefore, from the east coast of New Holland, to the east coast of Africa, and within the limits of the sun's declination, the winds and currents become periodical and changeable, according to his place. But it would be leading us too far out of the way, to attempt to trace the currents in the Indian seas, influenced as they are so variously and oppositely, in their direction and velocity, at different seasons, by the monsoons, and the bodies of land within their

their limits. Suffice it to say, what more particularly applies to the North Pacific, and will lead us again to the Arctic regions.

Having said that the air is rarefied and raised in the atmosphere, and that the greatest degree of evaporation is effected between the west coast of Africa and the east coast of America; and that north of the line, the fluid is so returned *towards* the North Pole, and being condensed *somewhere* in its passage by cold, it perhaps supplies with water some of the rivers which discharge into the seas of the temperate zone, or into the polar ocean; and whether, falling in rain, hail, or snow upon the earth or not, it ultimately finds its way into the ocean; and according to the temperature proportionate to its depth, the water takes a direction towards the regions of equatorial heat, is again raised by that heat to the surface, and again evaporated. For experiments in the *ocean* have proved, that when the temperature of the *atmosphere exceeds* that of the surface of the sea, the superficial water is generally warmer than that at certain depths *beneath* it, (I say generally, because in *soundings*, and confined waters, local causes effect many *exceptions* to this *general* rule), and in all probability, the greater the depth the colder the fluid in *that case*. And as we know, that when the air (or water) receives an increase of heat, its parts will be put in motion towards that heat, it follows that the colder water throughout its whole depth, must have a tendency to flow towards the point of greatest heat; and be continually rising towards the surface in the equatorial regions. This probably is the routine of the general movement of the atmosphere, and the waters of the ocean, between Europe, Africa, and America, from the Arctic regions to the equator; and it seems no less probable, that in the Pacific, they are subject to the same general laws. For there also the great equatorial current is in constant motion to the westward, and like the gulf stream, and from causes too in *some* points similar, it gradually turns to the northward, when it approaches the lands to the northward of New Guinea, and the Philippine Islands; and being perhaps at the same time influenced by currents setting in a different direction, more particularly during the prevalence of the S.W. monsoon in the India and China seas.

Near the coast of Japan, the current has been found to set N.E. by N. at the rate of five knots an hour. At eighteen leagues distance, about three knots in the same direction, but at a greater distance from the land, it inclined more to the eastward; and at sixty leagues from the land, it set E.N.E. three miles an hour, then (like the gulf stream), inclining gradually to the southward; so that at the distance of 120 leagues from the coast of Nipon, its direction was S.E. and its rate not more than a knot an hour. From this current setting generally to the N.E. along the coast of Japan

more

more or less strong, probably, according to the season of the year, it appears that the motion of the air and waters between the west coast of America, and the coast of New Holland, and all the lands to the northward towards Behring's strait, is *similar* to that north of the line, between Africa, Europe, and America. It is therefore presumable, that though a *superficial* current may run into Behring's strait, there must also be one running *out* of it *underneath*, if the principles this theory rests on, are *correct*.

The writer of the article I am examining is of opinion, that "the constant *circular* motion, and interchange of waters between the Pacific and the Atlantic," *must* be by Behring's strait, otherwise "it would be difficult to explain the perpetual egress of a current from the polar basin into the Atlantic, without admitting a supply through the only remaining opening into that basin to answer the demand of the current."

He considers the principal object to a free communication between the Pacific and the Polar basin, to arise from "Capt. Cook having found little or no current to the northward of Behring's strait;" and answers that objection at once, by instancing "the small current perceptible in a milldam, though the waters below may be rushing *out* with the greatest violence under the floodgate;" meaning, if I do not mistake him, that though little or no current was found at the surface, there *was one below* "rushing (in) with the greatest violence" from the Atlantic to the Polar basin.

I admit the probability of a surface current into the strait, for the reasons already given, and believe there may be one, because it is mentioned thus in Cook's voyage:—"We were now convinced that we had been under the influence of a strong current setting to the north, that had caused an error in our latitude at noon of twenty miles. In passing this strait last year, we experienced the same effect. On the 12th of July, when within the strait, in latitude 69° 37′, and half-way between the two continents, the current was found to set N.W. at the rate of one knot."

This proves there was a surface current, though a small one, both at the entrance, and to the north of the strait.—But what have we to found the supposition on, that the waters may be "rushing out,"—that is—*in*, with the greatest "violence under the floodgate," which is compared to "the impenetrable barrier of ice which stopped the progress of Cook's successors?"—The author of the article in question supposes, that "if the Polar basin should prove to be free from land about the Pole, it will also be free of ice," and that this may be the case, is not improbable, in the *summer* season; not however because of the non-existence of land, but for other reasons which shall be explained by and by. He also supposes that "the barrier of ice which

which stopped the progress of Cook's successors," was moveable, or no where touched the bottom.

The writer of Cook's voyage was of the same opinion about the ice nearest the ship, though it rested on a foundation that perhaps might not equally apply to the largest masses of ice further to the northward, and *not seen*. His words are—"We had twice traversed the sea in lines nearly parallel with the run we had just made; and in the first of those traverses we were not able to penetrate so far north by eight or ten leagues as in the second; and that in the last we had again found an united body of ice, generally about five leagues *to the southward* of its position in the preceding run.—As this proves, that the large compact fields of ice which *we saw* were moveable or diminishing; at the same time, it does not leave any well-founded expectation of advancing much further, in the most favourable season."

Though this proves that the floating ice *seen*, shifted its position, both to the northward and to the *southward*, but *chiefly* the *latter*, as will be seen further proved—Yet, it does not prove that the larger masses to the northward, which they did *not see*, might not be immoveable, by grounding at the bottom; *if* the water became *shoaler* in that direction, as our navigators appeared to find it was, as far as they advanced.—Now should there have been any such immoveable masses of ice to the northward, it would in some degree account *why* the current, which the writer in the Review supposes to set with such "violence" from the Atlantic, should not have carried the *ice* away with it towards the pole, where there *may be none*.—But, if the whole of this ice was moveable, it proves, that whether there was a small current setting to the northward, or not, and whether at the surface, and underneath, or both, there *must* have been a much *stronger* current *from* the *northward*, or something else, which still *more powerfully* impelled the ice to the southward in defiance of the other, as well as of the wind, which appears to have been generally from the S.W. when strongest. It is said in Cook's voyage, "it may be observed, that in the year 1778 we did not meet with the ice till we advanced to the latitude of 70°, on August 17th, and that then, we found it in compact bodies, extending as far as the eye could reach, and of which, a *part* or the whole was moveable; since *by its drifting down upon us*, we narrowly escaped being hemmed in between it and the land. On the Asiatic side, they encountered large extensive fields of ice, and were sure to meet with it about the latitude of 70° quite across, whenever they attempted to stand to the northward. On the 26th of August, they were obstructed by it in 69°$\frac{1}{4}$ in such quantities, as made it impossible to pass either to the north or west. In the second attempt they could do little more, for they were never able

able to approach the continent of Asia higher than 67°, nor that of America in any part, excepting a few leagues between the latitude of 68° and 68° 20′ N.—But in the last attempt, they were obstructed by ice *three degrees further to the southward,* and their endeavours to push further to the northward, were principally confined to the mid space between the two coasts."

Now all this does not seem to *favour* the supposition of a current "rushing in" from the Pacific through Behring's strait, with such velocity as it may be fairly supposed a body of water *would have,* of sufficient quantity to supply the southerly current, "setting perpetually into the Atlantic on both sides of Greenland, not only when the ice is melting, but when the sea is freezing."

Indeed, if we do but consider for a moment, the *quantity* of water that may be supposed to flow through so extensive a space as Davis's Strait, "with a velocity of four, and sometimes of five miles an hour," and then add to that the amazing quantity setting as constantly to the southward in the still greater space to the eastward of Greenland and Spitzbergen, it does certainly appear to be improbable, nay impossible, that a current of at least equal, or even of double velocity, and occupying the full extent of the breadth and depth of Behring's Strait, would be at all adequate to answer the demand; much less so trifling a current as we are warranted by facts to believe there is. For in Cook's voyage, the remarks on this matter are thus summed up: "We again tried the currents, and found them unequal, but *never exceeding* one mile an hour. By comparing the reckoning with the observations, we also found the currents to set different ways, yet more from the S.W. than any other quarter. But whatever the direction might be, their effect was *so trifling,* that no conclusion respecting the existence of a passage to the northward could be drawn from them." It is presumed, that all the currents here spoken of were *superficial.* But even admitting they extended quite across the strait, and flowed the same way throughout its whole depth; still it seems quite beyond the bounds of possibility, that the quantity of water so admitted, and with a rate of flow so "trifling," could be sufficient for the supply of the currents constantly setting to the southward, through the other two openings into the Atlantic.

[To be continued.]

LII. *On the first Principles of Music.* *By* A CORRESPONDENT.

To Mr. Tilloch.

SIR,—EVERY lover of music must derive great pleasure from pursuing the chain of reasoning elicited by your very ingenious correspondent

correspondent H. Upington, Esq.; and I feel anxious to apprize that gentleman, as well as your musical readers, that a very plain and apparently correct illustration of his principles has made its appearance some time ago in a little publication called "The Piano-Forte Pocket Companion," which professes to investigate the science and practice of music, not by drawing rules for vocal melody from the mathematical measures of the monochord, or the mere artificial arrangement of intervals as expressed by a keyed instrument, but by investigating the laws of melody, and even harmony, from plain and simple facts connected with the *voice*, the *ear*, and the *mind*, seeking in them for the *first principles of music*, finding a counterpart of these principles in the monochord and other sonorous bodies, and applying them to artificial imitative instruments.

Mr. Upington, at page 40 of your Magazine for January last, observes that, what the nature or manner of transmission of those particles may be, which, after being thrown off by the vibrating body, arrive at the ear, has not been hitherto discovered; —and yet that our organ of perception has been gifted by the Creator with the faculty of comparing, through the medium of the ear, the relative magnitude or number, or perhaps the peculiar arrangement, of these particles, equally as the proportions of a picture through the medium of the eye, appears too reasonable to dispute.

Most certainly; and Mr. Upington's difficulty is precisely that which has hitherto puzzled all writers on music. But that difficulty is done away by the little work alluded to; and a theory offered, which, if it is correct, bids fair to explain every musical difficulty that has yet appeared in the science.

Much light seems likely also to burst upon the science from a radical distinction drawn between the *major* and *minor*; the one being stated to be *naturally* an *ascending*, and the other a *descending* octave—a distinction which at once explains why the *artificial ascending* diatonic minor octave requires flats and sharps which are rejected in *descent*.

This distinction also seems capable of explaining many of the points noticed by Mr. Upington; whilst others, especially in regard to the intervals both in melody and harmony, are demonstrated to have their origin in the musical conformation of *man* himself, and not in the artificial arrangement of the key-board.

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LIII. *Notices respecting New Books.*

An Essay on the Principles and Construction of Military Bridges, and the Passage of Rivers in Military Operations. By Sir Howard Douglas, *Bart. Inspector-General of the Royal Military College at Farnham.*

This is a work of very considerable importance to engineers. The author informs us, that it was written several years ago, for the use of the senior department of the college over which he presides; and upon his embarkation for foreign service in 1808, it being left in one of the offices, to which it was sent, accompanied by a proposal for extending its utility, it became mislaid, and was never afterwards recovered. Anxious to prevent its meeting the public eye hereafter, without reference to the purpose for which it was designed, the author has now published it, with many additions derived from his own active services since that period.

The long protracted, and strongly contested war which we have lately carried on, has given our officers great experience in all military operations; and the present work may be considered as the result of much attention to the practical part of the subject. Its utility is not wholly confined to the hostile art; many parts of it being equally applicable to agriculture, and the conveniences of rural life: and we should hail the appearance of many other treatises of a similar nature from the same source, possessing the result of an equal quantity of experience.

The continual motion of armies, and the frequent necessity of crossing rivers of various sizes on short notices, render a ready knowledge of this art of the utmost importance to those who direct their movements. The safety of the troops, or the success of an expedition, most commonly depends on the celerity with which bridges can be made, by means of such materials as the country affords. There are many instances on record of whole armies being destroyed, and large tracts of country desolated, for want of immediate communications across rivers. The advantages, therefore, of a thorough acquaintance with the nature of those materials which are generally employed, and the best way of combining and using them, to engineers, and indeed to soldiers in general, cannot but be evident. To inform young officers who are unacquainted with the construction of military bridges, and to refresh the memory of those who have previously learned and forgotten, is the object of this publication: and it cannot but be pleasing to the country, to observe the author thus usefully occupied in those researches which so immediately concern the service for which the young gentlemen under his care are destined; whilst the influence which his rank commands, will tend strongly to in-

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duce them to follow his example, and store their minds with information, that some day may be of the greatest consequence to the interest of their country.

The number of different ways in which bridges may be made is very great, and the materials of which they may be formed very numerous: therefore, by thus collecting together into one volume so many resources, and arranging them in succession under various heads, the author appears to have adapted them for almost all the cases and all the situations that can occur in the usual march of an army. Its portable size, and the facility with which it may be referred to when occasion demands, must render it of service to those who are employed in defending the cause of their country. All the best writers on the art of war, both among the ancients and the moderns, have been searched for hints with which to enrich it; and some useful matter respecting the mathematical principles of hydrostatics, and the flowing of water, as well as the theory of floating bodies in general, has been collected from the most able writers. These auxiliaries, joined to the author's own knowledge, derived from long and active service on the continent, render the work valuable, and well-worthy the attention of all those who have occasion to build these useful means of communication, whether for the purposes of war or peace.

It is divided into seven sections: The first treats on the principles and effects of the motion of water in rivers; on the figure of their beds; and on the formation and nature of their sinuosities. It contains likewise the explanations and theorems that are necessary for the further prosecution of the subject, extracted from the able writings of Gulielmini, du Buat, and Bossut. It concludes with some useful remarks respecting the choice of places, and the best direction to take in fording rivers.

The second section contains the dimensions and weight of pontoons, accompanied by problems respecting the depth to which they sink with a given weight, and the weight corresponding to given immersions; also a table, showing these at sight. He then gives the practical mode of forming pontoon bridges, and some important hints, concerning the precautions necessary to be taken to prevent their being injured by any thing floating down the stream, by accident, or by design from the enemy. The whole is illustrated by real examples, collected from military memoirs, or the result of the author's own experience.

Section the third contains the mode of making bridges, either of boats collected on the rivers, near the seat of war, or of portable bateaux, made expressly for the purpose; together with instances of the use of light bateaux and row-boats in the passage of the Limat and the Linth in 1799, and of the Rhine in 1800.

The fourth section contains the theory and practice of flying

 bridges,

bridges with about twenty pages of good practical observations on forcing the passage of rivers.

The fifth section contains the method of making bridges on rafts of timber, on casks, on air-tight cases, and on inflated skins. The different chapters are interspersed with a variety of theorems and details, of considerable use in practice.

The sixth section describes the construction of carriage-bridges and rope-bridges. In this is given Colonel Sturgeon's celebrated rope-bridge across the broken arch of the bridge of Alcantara, which appears to have been a very complete one.

The last or seventh section contains the construction of bridges on trestles; on piles; on trusses, and other principles of carpentry. Among the latter, is an account of a wooden arch of 250 feet span, across Portsmouth-river in North America, which may serve as a good pattern, when the beams that can be obtained in the country are short, or when portable pieces of no great length are carried with the army. It appears rather too complicated for hasty military purposes; but may be advantageously applied as a permanent bridge over a river of no great width. Its construction affords good hints to builders in general. The chapter concludes with practical modes of passing small rivers by felling trees across them in such directions, that they may combine with each other in forming a communication.

A tolerably copious index is subjoined; and the plates, which are thirteen in number, are well engraved, and illustrative.

To an experienced engineer, many other expedients and methods will occur, besides those which he meets with on perusing this little work; but, probably, the author thought with good reason, that they would have increased the size beyond portability, and the expense beyond the reach of general purchasers. The plan and execution of the book is however good, and may be easily improved and extended, if the public should require other editions, of which there is little doubt when it becomes sufficiently known.

Chemical Amusement, comprising a Series of curious and instructive Experiments in Chemistry, which are easily performed and unattended by Danger; the third Edition with Plates, and considerably enlarged. By FREDRICK ACCUM, *Operative Chemist, Lecturer on Practical Chemistry, on Mineralogy, M.R.I.A. F.L.S. M.A.S. R.S.A. Berlin, &c.*

This work, of which a third edition has appeared, has been written with a view to blend chemical science with rational amusement. To the student it may serve as a very useful manual for performing a variety of curious and instructive experiments, well calculated for illustrating at a cheap rate, and in a pleasing manner, the most striking facts the science of chemistry has to offer.

offer. The treatise is written in a concise and perspicuous manner. But as the knowledge of mere facts does not constitute a science, the author has added his *rationale* to each individual experiment, so as to interest the mind, and to enable the young chemist to contemplate the phænomena with advantage, and their relation and consequence. All the experiments are well selected, and well calculated to unfold the wonderful changes which various bodies are susceptible of in their mutual action upon each other. The following experiments, among many others, cannot fail to diffuse mirth and surprise through a friendly circle unacquainted with chemical science. *To cause water to boil by the application of cold, and to cause it to boil by the application of heat*, p. 1.—*To set a combustible body on fire by the contact of cold water*, p. 14.—*Heat and cold produced by the same body at the same time and at the same temperature*, p. 29.—*Three metals, when brought into contact with each other, take fire spontaneously*, p. 46.—*Illustration of the art of calico printing*, p. 253.—*Of soap boiling*, p. 260.—*Of bleaching*, 262, &c. The plates of the work are executed with great neatness and fidelity; they exhibit the most essential chemical apparatus for carrying on experiments in the small way; and the whole of the work is singularly well adapted to answer the purpose for which it is intended, namely; to *blend chemical science* with rational *amusement*.

Whittle and Laurie have announced the publication, in two parts, of a general description of, and directions for, the coasts of Brazil, from Maranham in the north, to Rio de Janeiro and Santos in the south; accompanied with three large and elegant charts of the coast and harbours, from the surveys of Lieut. Hewett, R.N. and others; and in which, from original observations, the enormous errors of all preceding charts and directions for these coasts have been obviated.

An essay on a species of mosaic pavement, formed of right-angled triangles of different colours; with the method of calculating the number of their combinations: illustrated by a series of engravings. By N. J. Larkin.

The first volume of the Transactions of the Royal Geological Society of Cornwall, will, it is expected, be ready for publication in the course of May.

LIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 23. A PAPER was read by Mr. Pond, Astronomer Royal, on the Parallax of *Alpha Aquila*, in which he states the result

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of his observations to differ considerably from those of Dr. Brinckley.

A communication from Dr. Granville to Sir Everard Home, of a singular Case of Malformation of the Uterus, was also read.

Part of a paper from Dr. A. Ure, On the Elastic Force of Vapour, communicated by Dr. Wollaston, was read, and the conclusion deferred till next meeting.

April 30. The remainder of Dr. Ure's paper was read.

ROYAL INSTITUTE OF FRANCE.

April 24th. The Annual Public Sitting of the Four Academies took place this day.

M. De Rossel, President of the Academy of Sciences, presided.

M. Biot, of the Royal Academy of Sciences, read a notice upon the operations undertaken for determining the Figure of the Earth, in the course of which he took marked notice of the eminent exertions which Sir Joseph Banks, "the Nestor of the learned world," has made for the promotion of this object of philosophical inquiry.

M Quatremere de Quincy, perpetual Secretary of the Academy of Fine Arts, read a dissertation on the elementary principle of imitation in the fine arts, and the primary cause of the pleasure which they afford us. The theory he maintained is not new; he wished to demonstrate that the design of the fine arts, like the belles lettres, is not to reproduce the objects of nature, but to reproduce some one of the affinities which exist among these objects.

M. Abel Remusat, of the Academy of Inscriptions and Belles Lettres, read an article on the wandering nations of Upper Asia, extracted from a work entitled *Recherches sur les Langues Tartares*. The most original observation for which it was distinguished is, that the Goths originally issued from Tartary; in proof of which he affirms, that near Mount Altai inscriptions have been found in Runic characters similar to those of Scandinavia.

LV. *Intelligence and Miscellaneous Articles.*

IMPROVEMENT IN THE PURIFICATION OF COAL-GAS.

It is sufficiently known, that the production of carburetted hydrogen obtained from coal, and its fitness for the purpose of illumination, varies much according to the circumstances under which the gas is obtained, and the means employed for purifying it. To deprive coal-gas of that portion of sulphuretted hydrogen, with which it is always more or less contaminated, it has hitherto been made

made to act on quicklime, either in a dry state, or combined with water in particular vessels, so constructed as to bring a large surface of the lime into contact with the gas. This method must naturally be very imperfect, on account of the feeble action of sulphuretted hydrogen upon lime. In proof of this statement, the gas supplied to this metropolis, need only be examined in the following manner. Collect a four-ounce phial full of the gas, in a wash-hand bason, or other vessel full of water, in the usual manner, and then plunge into it a slip of paper moistened with a solution of nitrate of silver, or super-acetate of lead. The paper will instantly acquire a brown colour.

A new method of getting rid of the sulphuretted hydrogen gas has been lately resorted to with success; and the facility, cheapness, and expedition with which this process may be employed in the large way, give reason to believe that it will be highly beneficial to the manufacturer of coal-gas in general. The process consists in passing crude coal-gas, as it is disengaged from coal, through a heated iron cylinder, or other vessel, containing fragments of metallic iron, (the waste clippings of tinned iron will do very well) or any oxide of iron at a minimum of oxidation; for example, clay iron-stone, so disposed as to present as large a surface as possible: by this means the sulphuretted hydrogen becomes decomposed by the metallic iron, and the gas is obtained in a pure state. This iron, if in a state of a metal, acquires by this process a crystalline structure, and affords abundance of sulphuretted hydrogen by the affusion of dilute sulphuric or muriatic acid, a proof that it is converted into a sulphuret;—a quantity of sulphuric and sulphureous acid is likewise collected at the extremity of the vessel. The gas thus treated affords no disagreeable odour during combustion, and its purity is attested by its not acting upon the solutions of lead, silver, or any of the white metals.

WATER-SPOUTS.

The following observations of Capt. Thomas Lynn, commander of the E. I. Company's ship Barkworth, afford a striking corroboration of the statement of the ingenious writer in our last number, Mr. J. H. viz. that the particles of water ascend upward from the sea, in the phænomenon called a water-spout.

"Barkworth, Dec. 11, 1816, in lat. 4° N. long. 129° E. (having passed through the Siao channel yesterday) at 11 A.M. the officer of the watch, Mr. Dudman, came down and informed me there had been a *whale blowing* close to the ship for several minutes, and that it was continuing to do so. I then, from curiosity, went upon deck, and was surprised to find it was the vortex of a water-spout, within one hundred yards of the ship, on the windward quarter:—ordered a gun to be got ready, by which time

time it had passed under the stern, within thirty yards of the ship; which afforded us an excellent opportunity of observing this wonderful phænomenon.

"The space it occupied upon the sea was apparently about thirty feet in circumference, and the water so much agitated in the centre, as to be quite frothy, *ascending* in a spiral form in *visible particles* like rain, and making a rushing noise about as loud as the blowing of a whale continued, and communicating with a spout* depending from a black cloud over head, gradually passing to leeward, and disappearing about a mile off."

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for March 1818, it appears that during that month, the following was the work performed by the engines reported with each bushel of coals.

	Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
25 common engines averaged	21,898,644	various.
Woolf's at Wheal Vor ..	29,511,211	17·2 lib.
Ditto Wh. Abraham ..	30,445,224	16·8
Ditto ditto	26,978,054	5·45
Dalcouth engine	40,499,113	10·5
Wheal Abraham ditto ..	35,715,298	10·9
United Mines engine ..	31,427,373	13·6
Treskirby ditto	41,867,601	10·6
Wheal Chance ditto ..	33,594,548	8·9

PETALITE.—SELENIUM.

M. Ardvison, assistant to M. Berzelius, has discovered a new alkali, in a stone known to mineralogists by the name of *petalite*. The new alkali is named *lithion*. The experiments have been repeated and confirmed by Vauquelin. Berzelius has also discovered a new metal in sulphur, to which he has given the name of *selenium*. As it occurs only in the proportion of 13 grains in 500 lbs. it is not surprising it should hitherto have escaped the notice of chemists. This new metal has not yet been obtained in France.

MAGNETIC NEEDLE.

In the meeting of the Royal Society of Sciences at Copenhagen, on the 27th of March, Chevalier Vlengel read an essay containing observations on the magnetic needle, from which it seems probable, that its western variation has already been at its maximum.

* We could not perceive the communication with the spout, the particles being too minute for the eye to discern much above the sea, but we had no doubt of the fact.

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To Sir Thomas Cochrane, knight, commonly called Lord Cochrane, for a manufacture of lamps for streets which effectuate and regulate the combustion of a certain purified essential oil or spirit obtained from different ligneous, carbonaceous, or bituminous substances usually called spirit of tar, or oil of tar; and also working or making a manufacture being an arrangement or arrangements of parts of lamps, whereby all other lamps in which flame is inclosed as in street-lamps within glass vessels or cases, capable of transmitting light and protecting the flame from the wind and weather, are adapted to the production of a clear light by the combustion or decomposition of the said purified oil or spirit therein, and the use of the said purified essential oil or spirit in such lamps.—8th April 1818.—Allowed 6 months to enroll.

To John James Alexander M'Carthy, of No. 4, Spring Garden, Westminster, in the county of Middlesex, gentleman, for his method or methods of applying granite or other material in the making, constructing, or forming pavements, pitching, and covering for streets, roads, ways, and places.—8th April.—6 months.

To William Annesley, of Belfast, architect, for certain improvements in the constructing ships, boats, and other vessels. 8th April.—6 months.

To William Hopkinson, of High Holborn, in the county of Middlesex, coach-maker, for his machine or apparatus to prevent the wheels of waggons, carts, coaches, and all other carriages from coming off by accident, and which he intends to denominate or call a Wheel-detainer.—8th April.—2 months.

To George Whitham, of Sheffield, manufacturer of spindles, for his machinery for grinding, glazing, and dressing small cotton and woollen spindles, for spinning on jenney bills and mull and other kinds of machine for fine work.—8th April.—2 months.

To William Booth, of Eckington, in the county of Derby, turner in wood, for his method or process of making by a certain machine or machines, wooden clogs for pattens, wooden clogs or soles for shoes, and a description of wooden clogs commonly called or known by the name of the Devonshire clogs, or by whatsoever other name or names, description or descriptions the same several clogs or soles are commonly called, known, described, or distinguished.—8th April.—2 months.

To William Church, late of the New Coffee-House, Sweeting's Alley, Cornhill, in the city of London, but now of Clifton-street, Finsbury-square, in the county of Middlesex, gent., for his improvements in the steam-engine.—8th April.—4 months.

To

To Robert Clayton, of Nelson-street, in the county of Dublin, artist, for his method of depositing or inserting certain metals or a mixture of metals in wood, ivory, bone, horn, paper, and pottery ware, whereby the old and tedious process of inlaying may be superseded, and the same effects be permanently produced in a shorter time and at a less expense than by any other process now in use.—16th April.—6 months.

ASTRONOMICAL PHÆNOMENA, MAY 1818.

D. H. M.		D. H. M.	
3. 5.40	☾ ο ♓	14. 5.44	☾ ν ♍
4.	☉ eclipsed visible	15. .	♂ μ ♋ * 16′S
6.12.3	☾ ☿	16. 8.40	☾ γ ♍
7. .	☾ in apogee	16.22.54	☾ θ ♍
7. .	♃ 763 Mayer * 3′ S.	17. .	♀ 99 ♉ * 16′N
7. .	♂ 82 ♊ * 15′ N.	18 5.43	☾ λ ♍
9. .	♀ 51 ♉ * 5′ S	18.18 50	☾ α ♎
9. .	♂ 84 ♊ * 17 S	19 22 39	☾ δ ♏
10. 6.11	☾ ♊	20. .	☾ in perigee
10.21.55	☾ ψ^3 ♋	21. 5 48	☉ enters ♊
10. .	♀ 56 ♉ * 2′S	22.17.51	☾ τ ♐
11. .	♀ χ^2 ♉ * 11 N	24. .	♀ 121 ♉ * 9′S
11. .	♀ χ^1 ♉ * 17 N	25. 5. 1	☾ ε ♓
13. 5.24	☾ η ♌	28. 3.57	☾ 29 ♓
13. .	♂ 7 ♋ * 3′S	30.11.29	☾ ο ♓
14. .	♀ τ ♉ * 14′N		

NEW COMET.

To Mr. Tilloch.

The slow motion in declination, and that nearly perpendicular, and descending toward the Ecliptic, ought apparently to have made the new comet very easily discernible. I thought that I saw it on the 20th; but very repeated efforts to find it since in the absence of the moon, and at a very late hour, have been all unsuccessful.

I thought at first there was some mistake in stating the Perihelion in orbita to be 287°; but on investigation, I found the number right. And this would place the descending node about 306° R.A. and give a striking resemblanee to the long-expected comet of 1661. ——— C. L.

Errata in Mr. Capel Lofft's paper in our last number. Page 204, line 3, *read* revolution: ibid. line 21, *read* for W. HUME's —MR. HIGGINS: ibid. line 26, *for* iron *read* gaseous. Page 205, line 11, *for* equiunial *read* equicrural: ibid. line 19, *for* plane *read* place: ibid. line 20, *for* rota *read* rate: ibid. line 36, *read* Betelguese and Procyon: ibid. line 40, *read* with the nebulæ at all near it, and *dele* the "not."

Meteorological Journal kept at Walthamstow, Essex, from March 15 *to April* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twlve and One P.M.]

Date.	Therm.	Barom.	Wind.
15	42 47	29·50	SE.—Some slight showers; gray windy day; rain after 4 P.M.; rainy and windy. Moon first quarter.
16	37 49	29·30	W—NW.—Clear, and *cumuli;* some showers; wind and sun; *cumuli* and *nimbus*; evening bright star and moon-light, and light *cumuli.*
17	47 50	29·61	NW.—Clear; *cirrostratus,* and wind; cloudy and windy; some slight showers; light, but cloudy.
18	43 53	30·09	S.—Gray and some wind; a gray windy day; light, but cloudy.
19	47 53	29·90	SW.—Gray morn; some slight showers; a gray day; cloudy and windy.
20	47 51	29·61	W—NW.—Showers and wind; sunshine at 9 A.M.; fine day; windy; moon- and star-light.
21	34 48	29·90	SW.—White frost, and clear; very fine day; some showers and sun; clear moon-light.
22	47 50	29·60	SW—NW—W—NW.—Storms of rain and hail, and wind; great showers and sun; night rain and wind. Full moon.
23	47 49	29·23	SW—W—NW.—Violent rain and wind; [large elm trees torn up by the roots by the storm;] showers and sun; moon-light.
24	37 50	29·60	SW.—Cloudy; sun and wind; a storm at 1 P.M.; fine day; bright star-light.
25	37 50	29·65	SW—NW.—Clear, and *cirrostratus*; great rain in the night; clear and *cumuli*; wind and showers; star-light and windy; frost in the night; and then rain and wind.
26	39 39	29·60	SE—NE—NW.—Very rainy till after 3 P.M.; after 3, showery; cloudy windy night.
27	34 44	29·90	NW.—Sun and wind; fine day; some slight hail; star-light.
28	35 49	30·21	SW—S.—Calm and hazy; fine day; cloudy.
29	42 53	30·10	SE—S.—Clear; *cirrostratus,* and wind; fine gray day; star-light, and windy. Moon last quarter.
30	34 56	30·10	E—W.—Clear, and *cirrostratus;* calm; fine day; star-light.

March

Date.	Therm.	Barom.	Wind.
March			
31	38	30·30	N—NE.—*Cirrostratus*; sun, and wind; *cumuli*, and wind; fine day; star-light.
April			
1	38 49	30·30	NE.—Sun and wind; very fine day; clear star-light and windy.
2	38 49	30·20	NE.—Clear, and *cirrostratus*; fine day; sun and wind; dark night early; then star-light, and *aurora borealis*.
3	43 46	30·30	NE.—Windy and cloudy, and very cold; fine day; dark night.
4	36 46	30·30	N—NE—SE.—Sunshine; very fine day; clear star-light.
5	36 52	30·11	SE.—*Cirrostratus*; white frost early; calm, and thick ice; very fine day; dark night. Full moon.
6	49 52	29·31	SW—NW.—Small rain till near 9 A.M.; fine day; stormy wind; a cloudy dark night.
7	41 41	29·70	SE—E—SW.—Rain and wind; very rainy day, and but little wind; at night violent rain.
8	50 64	29·45	SW—S.—*Cirrostratus*, and gleams of sun; clouds, sun, and wind; rain after 1 P.M. till about 5 P.M.; night cloudy and windy.
9	51 55	29·35	NS.—Showers, and some sun; fine day; windy, and moon-light.
10	47 56	29·65	SE.—Rain; very rainy day; dark and windy.
14	47 60	29·30	S—N.—Sun and *cumuli*; sunshine; slight rain; very rainy after; stormy; wind, and very dark.
12	36 50	29·90	N—NW.—Sun and wind; and clear sky; sun, clouds, and wind; some hail storms; clear night.
13	36 52	30·05	S.—Hazy and white frost; fine day; sun and *cirrostratus*; cloudy and windy. Moon first quarter.
14	46 53	29·65	E—SE.—Fine morn; clouds and gleams of sun, from 8 P.M.; a fine *halo* round the moon, slightly tinged with red; bright moon-light till near 10 P.M.
15	47 58	29·65	E.—Fine morn; clear and *cumuli*; a very fine day; bright moon-light.

Errata in last month's Phil. Mag.,—*for* 29·90, *read* 28·90; 8th March, *for* moon first quarter 13th March, *read* moon first quarter 15th March.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Mar. 15	8	46·	29·47	Rain
16	9	44·	29·54	Cloudy
17	10	46·	29·86	Ditto
18	11	51·	30·06	Ditto
19	12	52·5	29·97	Ditto
20	13	50·	29·90	Fine
21	14	45·	29·84	Cloudy
22	full	41·5	29·68	Fair
23	16	38·	29·39	Rain—blows hard
24	17	46·5	29·73	Fair—rain and hail in the evening
25	18	43·	29·58	Fine
26	19	38·	29·51	Rain
27	20	43·	30·25	Fine
28	21	48·	30·43	Ditto
29	22	49·	30·39	Cloudy
30	23	50·	30·36	Ditto
31	24	45·	30·50	Ditto
April 1	25	45·	30·47	Ditto
2	26	47·	30·52	Ditto
3	27	44·	30·57	Ditto
4	28	47·	30·49	Fine
5	new	50·	29·31	Rain
6	1	41·	29·83	Ditto
7	2	58·	29·51	Cloudy—rain A.M.
8	3	54·	29·35	Ditto
9	4	45·	29·63	Rain
10	5	42·	29·40	Cloudy
11	6	43·	30·11	Ditto—frost and hail in the morning
12	7	50·	30·09	Fine
13	8	51·	29·85	Cloudy

The quantity of rain fallen has been very great, though the number of rainy days is comparatively small.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For April, 1818.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	40	47	39	30·05	36	Fair
28	38	47	40	·20	30	Cloudy
29	42	53	37	·10	49	Fair
30	39	55	40	·11	39	Fair
31	40	49	41	·22	30	Sleet showers
April 1	42	48	40	·13	36	Fair
2	40	47	39	·17	39	Fair
3	39	48	39	·20	47	Fair
4	38	46	40	·29	46	Fair
5	47	53	46	29·78	40	Fair
6	48	54	44	·32	33	Cloudy
7	44	43	43	·50	0	Rain
8	55	55	50	·40	0	Rain
9	54	57	47	·28	36	Stormy
10	47	55	45	·40	0	Showery
11	43	43	39	·42	0	Rain
12	39	45	38	·88	26	Hail storms
13	43	50	43	·80	37	Fair
14	39	54	44	·62	46	Fair
15	43	55	43	·58	47	Fair
16	46	55	42	·29	48	Fair
17	44	53	43	·30	50	Fair
18	43	51	38	·39	51	Fair
19	40	47	40	·70	49	Fair
20	39	49	43	·70	39	Cloudy
21	44	54	46	·64	47	Fair
22	48	53	46	·56	0	Rain
23	47	47	44	·25	0	Rain
24	46	55	46	·25	24	Cloudy
25	47	56	49	·26	21	Showery
26	56	65	55	·40	41	Fair

N.B. The Barometer's height is taken at one o'clock.

LVI. *On the Question "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?"* *By* HENRY UPINGTON, *Esq.*

[Continued from p. 186.]

To Mr. Tilloch.

Blair's Hill, Cork, April 12, 1818.

SIR, — MY former letters, the last of which has been published in your Magazine for March, were almost exclusively confined to a cursory survey of harmony, and the particular analysis of our modern intervals. To these letters occasional reference may perhaps be requisite; but in themselves they are obviously insufficient for any final deduction with regard to our oratorical question. They merely point out—what very few persons could have supposed—the superiority of the *fourth*, as well as the excellent proportions of the minor 3d and minor 6th; and indicate the apparent danger of cultivating harmony, when *dignity*, which is decidedly hostile to every species of jingle, shall become the object of the speaker. In place therefore of obtruding any premature and perhaps ill-founded opinions upon the public, I have considered it an indispensable task to analyse the genius of our *speech* itself, as connected with tone, time, and forte; proceeding with all the necessary caution of a careful experimenter;—first, by calling to my assistance the most discriminating ear of all my acquaintance; and secondly, by selecting an eligible person as the subject for investigation.

Aware of the almost insurmountable difficulty of measuring the intervals of speech; and determined that no favourite theory whatever should influence my judgement, I availed myself the more readily of the abilities of my musical friend—having previously ascertained the accuracy of his ear by a satisfactory test*.

* *Experiment.* I divided the scale of my 30-inch monochord, described in your Magazine for November, agreeably to the various intervals of our octave, as found on our piano-fortes; that is—taking the open wire as the fundamental, I marked across the surface of the board, a certain number of lines, every one of which exactly corresponded with every semitone between that fundamental and the centre of the wire or octave. In this operation I was merely guided by the *ear* of my associate, strictly conforming to those distances which *he* considered perfect. Now, having interposed a screen between him and the monochord, I struck the open string or wire for half a dozen times, more or less as he deemed expedient; when promptly introducing the bridge at any previous line I thought proper, I sounded *that* interval, which he never failed to recognise: and what is more, whenever I committed an error by placing the bridge even one quarter of an inch short of, or beyond the destined mark (which I frequently did, to try him), he was equally certain whether that interval was too sharp or too flat, although its deviation from accuracy could not have exceeded the one-fifth of a semitone.

Being thus sufficiently prepared for my intended scrutiny, I directed my endeavours towards the obtaining of a suitable *speaker:*—and with all due deference to the several provincial habits of our English, Scotch, and Irish countrymen, the intonation of the well-educated London gentleman habituated from infancy to the higher circles, has always appeared, in my estimation, so decidedly superior—that he, and he only, who in his delivery approximated most closely to that character, could interest my attention.

I succeeded. A gentleman just suited to my purpose—whose articulation was extremely distinct—whose modulation was agreeable,—and yet, who neither could sing in tune, nor play upon any instrument, entered fully into my views; and consented not only to the examination of his ordinary language, but also to become the subject of any course of experiments that I should deem it necessary to pursue.

Examination of his ordinary Language.

OF SOUND.

Observation 1. Two species of sounds or syllables were manifest—the *sonorous* and *obtuse*; the latter, in ratio to the former, as perhaps about one to four. The sonorous syllables were obviously of the musical class, and could be sufficiently estimated to form a competent idea of their situation on the scale:—on the contrary, the obtuse syllables were altogether immeasurable, until persevering practice overcame the obstacle.

Remarks. The whole of our particles, viz. *a*, *the*, *of*, *to*, &c. were almost at all times (in the flow of conversation) of the obtuse or non-sonorous order; as also, rather frequently, a considerable number of more independent monosyllables, viz. *at*, *but*, *get*, *met*, &c., together with a variety of commencing, intermediate, and terminating syllables of dissyllables, trisyllables, and polysyllables, as *ob* in *obtain*, *suf* in *sufficient*, *per* in *perfection*, *der* in *consider*, *con* in *convenient*, *col* in *recollect*, *lerable* in *tolerable*, &c. &c.

It may indeed be alleged, without hesitation, that every syllable whose *vowel* is indistinctly uttered, opposes a decided obstacle to mensuration. Hence the necessity, in graceful and melodious delivery, of expressing those *vocal* elements with all the clearness and euphony of which, consistently with the decorous usages of speech, our language is susceptible; a practice highly extolled by MILTON in his letter to *Hartlib*, on Education.

OF TONE.

Observation 2. The musical intonations or notes were almost wholly

wholly of that description called *fixed tones* or monotones,—the *slide*, or variation of tone on the same syllable, being very seldom employed. The *vowel* called "*long i*" (or perhaps more properly *triphthong*, for besides its ordinary character, if prolonged it will terminate in *ee*,) must be considered an exception to this general observation; *this* letter appearing, at all times, to terminate somewhat higher than it began *. The *latter* character indeed, viz. *that* of terminating higher than the commencement, is equally applicable to almost every other slide that was expressed; — the opposite character or *downward* slide being scarcely ever affected during the course of this investigation, except in a few instances where the vowel or diphthong *u* became the terminating syllable of a period, as in *you, news, refuse*, &c.

No *circumflex* † whatever was observable.

Remarks. Dr. BURNEY in his "History of Music," with every appearance of correctness observes, that it is extremely difficult to preserve a *slide*, when speaking to any person at a considerable distance; and that on such occasions we naturally use our efforts to express ourselves in *fixed* tones. QUINTILIAN too, in his "Institutes of the Orator," makes a nearly similar remark; *he* positively asserting that the command of our inflexions is lost upon the higher keys ‡.

Some modern theorists, however, would insist that *fixed* tones are the peculiar characteristics of *song;* and that a series of *speaking* sounds cannot be accomplished but through the agency of *slides*.

A Mr. STEELE (I believe of London), the author of a work intitled "*Prosodia Rationalis*," but deserving rather the appellation of "*Prosody run mad*," was the origin of this egregious absurdity, which he deduced or rather fancied to have deduced from a very obscure passage of DIONYSIUS THRAX:—But of this hereafter.

The reality is, that *slides* must be considered as *graces*, and

* May we not thus account for the well-known difficulty of singing this triphthong in perfect tune?

† Dwelling on the base of an upward, or on the summit of a downward slide will be taken by the grosser ear for two species of the circumflex. Thus, _/ may be taken for \/, and ‾\ for /\ : errors into which I am persuaded that our grammarians have constantly fallen, when speaking of the Scottish and other intonations. — See *Sheridan, Walker*, &c.— This deception is principally attributable to the management of the *ictus*.

‡ Is it necessary to observe, that in calling to a distant object, we are compelled by necessity to the adoption of the higher keys or pitch? I have met with some professional musicians who imagined the reverse, until actual experiment convinced them of their mistake. Had they reflected on the wide difference, both in loudness and acuteness, between the braced and unbraced *drum*, the question had been instantly resolved.

oftentimes peculiarly expressive ones, with which every language may, and ought to be, occasionally adorned: but as a proof that they are not actually indispensable for the constitution of *speech,* in opposition to *song,* I shall proceed to experiment, by setting the following sentence in fixed tones or monotones; and this too without any deviation from the ordinary intervals of our musical scale.

Experiment.

[I would recommend to the amateur to defer the execution of this passage. By reading a little onward; he will find some necessary hints.]

Thus it would appear, that the essence of speech is dependent on the *series* of sounds, not on the individual character of those sounds with regard to slides or monotones: and consequently that by the modification of the series, speech may be converted into song, or song into speech, at the pleasure of the composer.

Preparatory to the execution of this passage, it will be necessary to offer some observations even to the intelligent amateur: for, strange as it is, among the various musical gentlemen to whom I have shown it, not more than two or three have been able to perform it, until the following indispensable requisites were laid before them.

Requisites for the Execution of the Experiment.

1. Of Tone.—Agreeably to Italian phrase (for which I am indebted to Dr. Burney) this passage must not be *vocalized,* but *syllabized:* the meaning of which is, that the mouth must not be unduly opened, nor must the larynx be almost solely occupied as in song; but that every organ be preserved, and engaged in that exact position in which it would be necessarily placed, were every syllable to be uttered in the ordinary way of language. Thus will be obtained that peculiar modification of sound which is the characteristic of speech.

2. Of Time.—Every idea of *musical* time, or musical pauses called rests, must be relinquished—forgotten: neither should any syllable whatever be contracted within, or extended beyond its usual dimensions in speech. Also,

A sufficient duration must be allotted to every *consonant;* by no means suffering the whole period of enunciation to be almost

most exclusively consumed by the *vowel*, as in song*; which, especially with the inarticulate singer, is the general practice. Without this necessary precaution, the closing syllable in particular, viz. "*man*," whose vowel is exceedingly short, will have a most unnatural effect.

3. Of Forte.—The sentence should be rather energetically delivered, as a public toast, for example; and sufficiently loud to be heard throughout the imaginary room. The due emphasis too, and no more, should be given to every emphatic and unemphatic syllable; while the genius of song which so frequently deranges this habitual order, must be studiously avoided.

These premises being fully understood and carefully attended, —nothing further remains than merely to suggest to the experimenter, that if, after giving every vowel its legitimate sound, and expressing every syllable without a *jerk*, should still the minutest vestige of *sing-song* be discoverable;—the substitution, in such instance, of a fixed quarter tone higher or lower, or some other unacknowledged interval, in place of the existing character, will effectually remove it.

To the musical gentleman who would feel desirous of trying the experiment, but who may consider the execution of so many solitary and independent intervals (for they are by no means associated, and governed by a key-note, as in song) too troublesome a task, I would recommend the playing of this little passage on the piano, and accompanying it with his voice in the best manner he can: then setting aside the instrument, let him endeavour to *speak* the passage without ever reflecting on the necessary notes; and he will be certain not only to express it in fixed tones or monotones, but likewise so very closely to the *actual intervals*, as will enable him to form a sufficiently just opinion of the subject.

Now, after all this lengthened discussion, lest any objection should arise in the mind of my intelligent reader, with respect to the unvaried range of notes, or repeats, which two or three times may appear within this sentence; or that he should wish for an

* Is it not upon this principle that the *stammerer* can *sing* with more facility than *speak?* And, to cure his impediment, should he not, for some time, be exercised in the opposite extreme; extending as much as possible, every prolongable consonant, and proportionably contracting every vowel? It is true, that by so doing he will incur the danger of cacophony, to which the over-extension of *f*, *m*, *r*, *s*, and other disagreeable consonants will subject him: but, *this* habit can afterwards be abandoned.

Having thus touched on the *stammerer*, will it be too digressive to observe, that changing his associations, with regard to *time*, may also prove beneficial? Let this time be of whatever species it may, it should be simple and slow; nor should the unfortunate person be suffered to protract his cure by hurrying over any of his consonants, for the despicable purpose of crowding them within a *bar*.

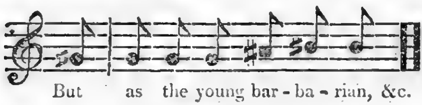
opinion as to the probable number of *slides* with which it may be adorned*—I shall hazard a few more, though perhaps unnecessary remarks, and terminate my letter.

First then, as to the range of notes or repeats,—I would request him to consider that, having a *different* object in view, I did not, by any means, attempt that variety of intervals which very musical speakers may possibly introduce: and yet I can hardly persuade myself that even the present intervals deserve, when applied to language, the epithet "*unvaried*†," especially when I compare them with one of the boldest and most expressive *recitative* compositions of the immortal HANDEL.

We are indebted to Dr. Burney for a copy of this production, taken from the last act of the Oratorio of Athalia. He considers it a *master-piece*, in which the genius of English speech is wonderfully preserved. It commences with "*But as the young Barbarian I caress'd,*" and terminates with "*I fainted and I fell.*"

The music runs thus:

Recitative.



Of this passage—the variations of interval require no comment: but with regard to the *slides* (which in regular music are represented by *slurs*);—throughout the four lines from which the above words are extracted, not a single instance of any such can be found, except in the solitary case of "*fainted*" or rather "*faint.*" Such was the conception of Handel.

In *speech*, however, as I should imagine, these graces, by way of compensation for its comparatively unmusical character, may be less sparingly employed. Our short vowels, it is true, when

* It will be seen, if my subsequent reasoning be well founded, that "Eng" in *England*," by somewhat extending the vowel, is the only admissible slide, (and *that*, most undoubtedly, an *upward* one) within the passage.

† There is a material difference between "*unvaried*" and "*unmusical.*" Speech is not very much inferior, in *variety*, to the simpler kinds of recitative, although less *musical* in its intervals. Recitative too, is governed by a key; speech by none. Regular *time*, likewise, to a considerable extent, is granted to recitative, while, to speech, even this privilege is denied.

legitimately

legitimately sounded, will hardly admit them *; and, therefore, in the previous example, if the word "*fainted*" which Handel has adopted for the slide, be chosen; the first syllable, as being long and emphatic, should claim, no doubt, the prerogative.

To conclude: In *speaking* this passage, if my judgement be correct, the *ba* in *barbarian* is nevertheless the only syllable, within the quoted portion, susceptible of a slide, and *that* too an *upward* one; which gives the speaker, on all such occasions, a suitable command;—whereas the very reverse must happen whenever the *downward* slide is attempted: from which I would naturally infer, that if, for expression's sake, the downward slide be effected on "*faint* †," the voice of the speaker will be dangerously precipitated, and the succeeding syllable "*ed*" be rendered very nearly, if not wholly, inaudible.

Such are the fatal effects of sinking through the scale, *during the enunciation* of any syllable: and therefore the resistance of this practice, or judicious *sustentation* of the voice, cannot, in my opinion, be too strongly recommended to the orator; for on this sustentation, as the intelligent have uniformly observed, the majesty of every language must depend.

[To be continued.]

LVII. *On the Geological History of Loch Lomond.* By GAVIN INGLIS, *Esq.*

To Mr. Tilloch.

DEAR SIR,—I REMEMBER mentioning to you long ago, a curious coincidence in the names of places upon the rivers Leven in Fife, and Leven in Dumbartonshire. In both counties their names are derived from the Gaelic, and descriptive of their local appearance and situation: but however the rivers and lakes may coincide in name and other circumstances, in their present and former geological position there is a prominent change.

The lake in Dumbartonshire is now elevated twenty-two feet above its ancient level, which is an exception to the general fact —that lakes, by the impetuosity of their discharging streams, in process of time wear down their channels till they lower themselves to the level of some opposing rock, or finally, to the level of the sea, laying their former beds dry, or leaving marshes in their place. It appears perfectly evident, that the waves of the Clyde have once beat against the bottom of Dumbuck,—that the delightful road, after passing westward by Dunglass Castle, along the base of the impending rocks, and the space between

* From the very great difficulty of execution, as well as contemptible effect.

† In speech, a well sustained *diminuendo* is sufficiently expressive.

 that

that and the Clyde, has been formed by the debris from the elevations on the north, and the alluvial deposits from the river on the south; that the tide at a former period has flown to the head of Loch Lomond, as it now does to that of Loch Long and Gair Loch; and that at that period Dumbarton Castle must have been surrounded and standing in deep water, rearing its basaltic mass above the circumambient element, like the Bass or Isla;—that the flat ground round the castle, that on which Dumbarton now stands, with all the low grounds east of the Leven, extending from Dumbuck to Stonefield, with the crofts of Dalquhurn, Cordale Dillychyp, Bonhill, &c. with all the lower grounds round that part of the lake where the Leven escapes from its bosom, debouching towards the sea, owe their present elevation above the Clyde, to the stones, gravel, and other alluvial matter brought down and deposited by the river Endrick, which discharges itself into the mouth of the present loch. So long as the abyss of the lake continued to receive the deposits of the Endrick, no alteration could take place on the surface of its waters; which must have then extended in one sheet of beautiful expanse, from the Clyde to the head of the present lake, and from Dumbuck on the east, to the head-land on the west of Dumbarton bridge. But after the accumulated matter from the Endrick rose to the elevation of the high water level, the lake getting brimfull every flood tide, there would then, as far as the slender state of the primitive but increasing barrier could oppose resistance to the weight and pressure of the collected waters, be a torrent occasioned at every ebb, which would carry along with it the mud, clay, sand, gravel and stones, till the succeeding tide catching the waters of the Leven like a thief loaded with the spoils of the Endrick in its progress round the south head-land, would throw the Leven with its alluvial treasures on the opposite shore, where its stream would be impeded by the hill of Dumbuck; and in rounding westward to join the Clyde, the castle rock would oppose its stately resistance, to prevent the robbery and stilling the waters, or give time for the lighter alluvials to deposit and settle down; so that the first formation of alluvial soil must have been in that water. The continual gatherings, the wreck of other soils brought down by the Endrick, increased by every autumnal torrent and winter flood, would collect and heap up from time to time, till the waters thrown back upon the lake would accumulate such weight as to force the augmented, but still loose and unconsolidated materials of its dam, forward into the vale below; till the rushing water coming to its level, would lose its pressure and allow the heavier matter to settle down, still carrying the lighter substances on towards the Clyde, and lodging them behind the castle rock. This in process of time would bring that part to

the

the level of the highest tide. The new-made soil would soon be furnished with the seeds and many of the roots of the various plants indigenous within the whole range of the lake, the Clyde, and all the tributary streams. Reeds, rushes, and other vegetables would soon cover the surface, and add rapidly to its elevation; in cases of autumnal floods filtering the water as it passed to the sea, and retaining the slush, not only of the river, but in a more eminent degree that of the land-rills and hill-burns, raised by occasional thunder-plumps and other heavy rains, winter freshes and storms, which must have brought down great quantities of soil and debris from their kindred heights, and given birth to the gradual and gentle inclination towards the rising ground, which now hides from human eyes all marks of their origin and former state. After the quantity of matter at the mouth of the lake had equalled the ordinary level of the highest tides, and acquired a solidity beyond the elevated pressure of the water during the recess of the sea, there were still great efforts to be made to bring the surface of the lake to twenty-two feet of altitude, and the whole vale of Leven, from the castle to the lake, to a gently inclined plane. This must have been the work of time, and a succession of heapings and levelings before the loose materials had gained that compact solidity to present an effectual resistance; even after the dam had acquired a consistency and durability sufficient to repel the effects of any ordinary flood, the grand finishing strokes given to this fairy-land must have been the effects of a succession of winter freshes, after severe and lengthened storms and frosts. The vast surface of the lake being covered with ice of great strength and thickness, and swollen by the mountain torrents above its usual height, the accumulated ice would of itself become an elevated and powerful obstruction to the discharge of the surplus waters; till overcome by the increasing weight, it would sweep from bank to brae, tearing up and leveling every thing before it, carrying from round the edges of the lake and the islands, and still more from its newly-formed dam, great quantities of stones, gravel, fragments of rock. Even blocks of great size firmly fixed in the ice, might in this way have been floated down, till grounding somewhere, they remained, or dropped into the hollows by the melting or bursting of the crushing ice dashed against the rocks of either shore.

The shoals of herrings that abound in Loch Lomond have been adduced as a proof, that the obstruction of the original communication between the lake and the sea must have been the effect of some sudden convulsion: but that idea proceeds from the propensity that many people have to account for every change by something strange and marvellous, without deigning to look into or inquire after the unceasing progress of nature in her most legitimate

timate and simple agencies. These operations, from their simplicity, although great and momentous in their results, often escape the observations of man, till, after a long succession of ages, changes are effected, the cause and origin of which are for ever hid from mortal research. The parent-shoal of these herrings may have passed from the sea into the lake at a very early period, when the waters of the latter might be less elevated above the Clyde, perhaps aided by the circumstances of a flood in the river and a spring-tide from the sea. This might throw the waters so far back, as to render the pressure of the current from the lake little beyond the run of a brisk ebb.

The herrings once fairly in the lake, might not so easily escape from their new element by the narrow and single outlet where the water runs with some force. In the regular migrations of herrings from the bays and shoals to the depths of the ocean, in their endeavours to escape from the still waters of the lake, arriving at where the current acquires its greatest force, they might instinctively spring back, from the dread of impending danger or any sudden impulse, and seek safety in the still waters and depths of the lake, where, in time, they have become as completely naturalized to the fresh, as they were originally to the salt water.

[Loch Leven in a future paper.]

March 23, 1818. G. Inglis.

LVIII. *On the Practicability of a direct Passage over the North Pole.*

[Continued from p. 306.]

Judging from such facts as are before us, that a part, and but a very small part, of the demand to supply the southern current, comes in from the Pacific through Behring's Strait,—it is necessary to inquire, From what sources, then, is all the water so flowing out of the Polar regions to the southward derived?

I have supposed them to be produced (at least the "motion of the great deep," generally) by evaporation in the equatorial regions of heat, and by cold returned, in various ways, in the atmosphere by land and by sea, into all the northern regions, even as far as the Pole. For though "the way of the Almighty," as thr psalmist says, "is on the sea, and his path in the deep waters," yet it is also as surely in the clouds of heaven. And though "his footsteps are not known" certainly, yet it is permitted us humbly to endeavour to trace them.

Whether or not there is any increase of water from the melting of the ice in the Polar sea so as to cause a current to the south, appears to be not very material; and perhaps has little to do in increasing

increasing or diminishing the *general* quantity of water in the "polar basin." In all probability, it remains *nearly the same* at all times, whether there is more or less ice; that is, taking the ice and water together as an aggregate body to make up that quantity. I agree with the writer in the Review, that "those who could suppose the melting of the ice to afford such a supply, would betray a degree of ignorance," greater perhaps than that of not being aware "of the very little influence which an Arctic summer exerts on fields of ice, perpetually surrounded as they are with a chilly, and mostly with a freezing, atmosphere, created by themselves." However, there is no subject, perhaps, on which opinions have been more at variance, than on the melting of the ice in the Polar regions, as well as where, and how it is formed. St. Pierre went so far as to suppose it was the cause of the tides. But he does not appear to have been a plain "matter of fact man," but of fancy and imagination.

Others think the ice does not melt at all, or at least very little, even in summer. If ice, when once formed (be it how it may) round and along the coasts of those regions, *does not* melt at all, there must be a constant increase so long as that ice is "surrounded by a freezing atmosphere created by itself," which we are told it "mostly is" even in summer; and if so, we may fairly presume it *always* is in winter. At this rate, with the exception of what may make its escape to the southward through Davis's Strait, and to the eastward of Greenland, it would necessarily be always advancing towards the Pole (admitting the land to be the place of its first formation) and close over it; unless we can find some probable cause counteracting this effect of *perpetual frost*. And perhaps we are warranted in supposing that there exists some such cause. Indeed it seems more than probable, that the process of *freezing and melting* may be going on in the Arctic regions on the *same* body of ice (if of magnitude to be *sufficiently* immersed) at the *same* time, and *perhaps* in the winter as well as the summer.

Water is a compound of ice and caloric. The temperature of ice is 32°; and while surrounded by a temperature *equal*, it will remain ice. But whenever the temperature of the atmosphere exceeds 32°, and continues so long enough for the body of ice to receive a *sufficiency* of caloric to effect its dissolution, it will do so. It is probable that the temperature of the atmosphere, even in the Arctic regions, in summer, will sometimes exceed 32°, and the more, perhaps, the nearer the Pole; and whenever it does *sufficiently*, the effect on ice is obvious. This seems sufficient to be said on the *probability* of ice *above water* melting in the Arctic regions in summer, if the temperature of the atmosphere ever *sufficiently* exceeds 32°. In the winter, as the temperature

perature of the atmosphere must be constantly below that, of course the freezing above the surface of the sea will be as constant, though the surface of the sea itself probably will not freeze at a temperature much below 30°, even in a motionless state. The *same body* of ice while freezing *above water*, that is increasing in size and extent by snow and hail, and the salt water freezing in washing over it, may perhaps at the *same time* be *melting under water;* and this process will be probably accelerated according to the magnitude of the mass, and the depth of its immersion. For when the atmosphere is colder than the surface of the sea, the water will (in proportion perhaps to its depth) be found warmer by some degrees, than it is at the surface; and though few experiments have yet been made to establish this fact, yet sufficient to warrant this conclusion. Thus, even in summer, if the temperature of the atmosphere should be 32°, and the surface of the sea, *clear of land and soundings*, three or four degrees higher, that of the water below would probably be much higher still; so that the portion of a large mass of ice *above the surface* of the sea would remain ice and augment; and the other portion of it *below* being immersed in a temperature exceeding the point of congelation, would probably be melting and decreasing.

The well attested facts, of large bodies of ice having been seen to capsize or turn bottom up, prove, that their centre of gravity is altered by an increase of their bulk above, or a diminution of it below, according to the excess of either effect. Upon the whole, however, it seems probable, that in the Arctic regions, the process of freezing in the atmosphere, exceeds that of melting under water, particularly on those smaller masses of ice which are immersed least; and therefore that there must be a *general increase* of ice in the polar basin, from the Pole (*if* the ice originates *there*) towards the lands surrounding the basin; or from those lands (if the ice first forms there) up towards the Pole. On this question, too, opinions have been most various. Every circumstance seems to weigh against the opinion of its greatest formation being about the Pole, except one, and that is, because the sea-water there will most probably be found to contain the least salt. I am disposed to believe, that it must also in the winter be colder at the surface of the sea, near the Pole, than any where else. Yet, on the whole, it seems most probable, that the ice is originally formed in the rivers, and along the shores of all the lands surrounding the Polar basin. Being afterwards detached from those lands, and driven to *sea* by the winds and currents in masses of more or less extent of surface, but no great thickness; it *there* accumulates by the falling of snow and hail,

and

and by the salt water freezing upon it, into immense mountains and fields.

In the part of the Polar basin further to the southward, where it is bounded by the land, it is to be presumed the general prevailing winds are from S.W. to N.W. particularly the former, in bad weather: Northerly and easterly when most settled and fine. And if so, it is equally to be supposed there will be a generally prevailing current from the westward to the eastward, partaking at the same time of that general tendency of the fluid to move *southward* from the Pole, which I imagine it will be found to have, from the coldness of its temperature. These two general combined impulses operating upon moveable bodies floating on the surface of the Arctic seas, must impel them in an *east-southerly* direction all round the globe; being, in fact, that "circumvolving current" which the writer in the Review mentions as carrying "fir, larch, aspen, and other trees," the produce of "both Asia and America, from the polar basin through the outlet into the Northern Ocean." But a page or two further on, he has "annexed a diagram, constructed on the *plane* of the *Pole*, to assist the reader in the explanation of the notions he entertains on this interesting subject," but which is rather *puzzling*: for whoever turns over the leaf with an idea of his own, or adopting that of the writer, that there really *is* a "circumvolving current" from west to east in the Arctic regions, will be surprised, when he casts his eyes on the diagram, to observe *two different* courses denoted by arrows.

The first shows the probable direction of the ice bergs, from New Siberia, and is nearly as follows:—N.E. by N. to the latitude of 80° from thence about E.; then S.E. S.E. by E. and again S.E. into Davis's Strait. This first route, if it were not for the north-easterly direction of the first arrow, accords pretty well with the notion we had formed of a "circumvolving current." But the second, which the ice fields are conjectured probably to take from the western part of New Siberia, viz. about N.W.W. then about S.W. to the N.E. part of Greenland, is so *contrary* to the former, and so completely *opposed* to the direction of the circumvolving "current which carried the trees of both Asia and America into Davis's Strait," that it is difficult to account for so very obvious a discordance, except by supposing him to have considered the diagram he looked on, to represent *indeed* a *plane* surface, instead of a globular one, as it is. The case then stands thus—If there *is* a current such as the second for the ice fields, a circumvolving current *cannot exist*; and if there *is* a circumvolving current, it is quite impossible that the ice fields *can* take the "probable direction" assigned them in the diagram.

Having

Having presumed that there *is* a "circumvolving current" in the Arctic sea from W. to E. but *southerly withal*, it leads me to inquire into the probable effect of it, and the winds together, upon floating masses of ice.

In the first place (let the ice be formed where it may), its *general* direction will in all probability be from W. to E. with a tendency at the same time to set to the *southward*, too strong to be much counteracted by the force of any winds from *that* quarter; its bulk *under* being greater than that *above* the surface.

If we cast our eyes on a chart of the north polar regions, constructed on the plane of the equator, no opening is seen for the egress of ice to the southward, out of the Polar basin, from Norway and Lapland to the eastward, along the whole coast of Asia, till we come to Behring's Strait. Through this strait it does not appear at all probable that much of the ice can pass, on account of its comparative small extent, and the depth of water, perhaps, being insufficient to float the bodies of greatest magnitude. There may also be a "*trifling*" superficial current, as I suppose; or one "of the greatest violence," as the writer of the article in the Quarterly Review supposes, running in from the Pacific, to oppose its passage through this strait.

From Behring's Strait, then, to the eastward, all along the north coast of America, we find no opening for the ice to escape, till we get to Davis's Strait. Through this strait, if there *is* "an uninterrupted communication," it is not unfair to presume that immense quantities would be carried by a current "running perpetually with a velocity, as it is stated, of four and sometimes of five knots an hour! I am, however, inclined to think that either from the interruption of lands, or shoals, between Greenland and America, a comparatively small quantity passes from the Polar basin through Davis's Strait, and that *much* of the ice, as well as the current, may have Hudson's Bay for their origin. *If* any obstruction *does* exist to the free egress of ice through Davis's Strait, the consequence must be a vast accumulation of it in a mass, more or less consolidated, from about Nova Zembla all the way to the eastward, as far as Greenland; and extending northward from every part of the coasts of Asia and America, at least *to the parallel* of latitude in which the *north point of Greenland may lie*.

For whatever masses of ice cannot pass through Davis's Straits, must be pressed continually by others brought from the westward and northward by the circumvolving current, along the north part of the more connected ice. If its progress through Davis's Strait to the southward was not *somehow* impeded, it would pass through. If impeded (let the impediment be what it may) in its course to the southward, it is yet still more im-

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peded in its progress to the eastward, by the west side of Greenland; and therefore must accumulate against this solid barrier, *as far at least* to the northward as Greenland extends. Then, and not till then, can ice of any comparative quantity drive further to the eastward, or find any passage down to the southward.

All the ice furthest to the northward of Greenland is then at liberty to move on towards Spitzbergen, whilst the ice that may be in motion closest in with the land, when rounding the N.E. point of Greenland, will take a turn to the southward, and *in towards* the coast *withal*, because it will be within the influence of an eddy that must necessarily be produced in the stream of waters passing *nearest* to the *N.E. part* of that land. There it must collect; and if it consolidates, extend to the shores of Iceland, or even to Spitzbergen; or else "burst its fetters," as it is said to have done lately, and drift away to the southward into the Atlantic.

This is sufficient to account for the ice between Greenland and Spitzbergen having a general movement to the S.W.; and there is the same reason to suppose that the ice nearest to the N.E. and E. coast of Spitzbergen has also a similar movement. But it will not warrant the conclusion of there being a current in the *same* direction, at any considerable distance to the *northward* and *eastward* of Spitzbergen. On the contrary, it seems most probable that any masses of ice found in that direction to the northward of 82° or 83°, will be more within the influence of the *general* circumvolving current, and therefore make an east-southerly drift towards Nova Zembla, and perhaps clear of its N.E. point.

Greenland and Spitzbergen being situated so much further to the northward than any other known land in the Arctic regions, form an impenetrable barrier against the movement to the eastward of any ice but what may be to the *northward* of them both.

Much of this northernmost surplus ice finding its way to the southward, is one reason why it seems very likely, that, ice in the greatest quantity, and most compact, will be found from about Nova Zembla all along the coast of Asia and America, and extending to the northward as far *generally* as the north point of Greenland; and that *perhaps* less and less ice will be found to the northward of that parallel, as the Pole is approached; that is, adopting the opinion that the ice is first produced on the lands surrounding, and accumulated afterwards at sea, so as to extend its surface from those lands, northerly, till it reaches the parallel of the north point of which the surplus ice *must* round before it *can* pass into the Atlantic, *if* Davis's Strait *is* obstructed.

Greenland

Greenland and Spitzbergen forming so powerful a bar to the progress of the ice with the circumvolving current to the eastward, renders it extremely probable that there is always less ice between Nova Zembla and Spitzbergen, than any where else in the *same parallel*, and perhaps still less the nearer the Pole in summer.

Whether the ice during the *winter* encompasses the Pole or not, can only be matter of conjecture; and, in all probability, the fact will never be decided by man.

In that season, if the cold is intense in proportion to the nearness of the Pole, it is possible the ice *may* advance to it. But yet, as it is more probably drifted out of the Polar basin, as fast perhaps as it collects to the northward of Greenland, it seems more reasonable to presume, that it seldom reaches much beyond the latitudes of 82° or 83°, in any consolidated or very extensive bodies all the year round. On this ground for one, rests the opinion I hold, in common with the writer of the article in question, of the probability of the vicinity of the Pole being free of ice in the summer; not, however, as a *consequence* of there being no land there, but whether there shall be any land or not. For I have supposed it likely, that the temperature of the atmosphere in the Arctic regions in summer, may sometimes exceed 32°; and the more, perhaps, the nearer the Pole is approached.

First, because there may be less ice, for the reasons I have given. And if there *is* ice, there will *probably* be a warmer atmospheric temperature to dissolve it at the Pole itself, than any where else to the southward of it, as far as 75° or 80°. Because, when the sun's rays first strike the Pole, they will be felt there incessantly for six months; but with what force and effect we have yet to learn. On all other parallels, in proportion to their distance from the Pole, the duration of the sun's influence will be shorter. And though the sun's power during the periods they feel it may perhaps be greater than at the Pole; yet, being interrupted while he is below the horizon, it is perhaps probable, on the whole, that the greatest effect of the sun's heat may be at the Pole, as there he is above the horizon for six months; in the latitude of 84°, about five months; and in 78½°, about four months only, at a time.

We are next to inquire, what "facility the late disappearance of the ice from the east coast of Greenland offers; 1st, for attempting a direct passage over the Pole; and 2d, the more circuitous one along the northern coast of America into the Pacific."

As to the 1st, according to the view I have taken of the subject, it appears to me, that the facility this event offers for attempting a direct passage over the Pole, would be very *nearly the same*, whether more or less ice is collected, not only on the *eastern*

eastern coast of *old Greenland*, but even all around it, and even between it and Iceland and towards Spitzbergen. That is, provided the attempt is to be made, as it is to be hoped it will be, to the *eastward of Spitzbergen;* because, for the reasons I have offered, it is probable the least quantity of ice will be found there *clear of the land;* at all events, whatever masses may be found there, they will in all probability be of less magnitude, and more detached from each other; because the space for them to move in is least confined. If any of the vessels fitting out are destined to take this route, the probability is, that if they advance beyond the latitude of 82° or 83° N. the ice will less and less impede their progress to the Pole; and to reach it will perhaps be the least difficult part of the enterprise.

To the northward of 82° or 83° up to the Pole, it is likely that the weather in the summer will be for the most part fine, but hazy generally—thick fogs will be very frequent. The winds are likely to be moderate, shifting often round from north to east, by south and west, to north again; but prevailing *chiefly* from the eastward.

If our polar navigator is furnished with time-keepers set to Greenwich mean time, or with known errors relative to that time, whose *rates* of going have been correctly ascertained, and which rates will *not* be affected by cold, electricity, magnetism, and other yet unknown causes perhaps existing there, which may operate on the materials of their construction; it is very true, the "time at Greenwich" will always be known: but should this compass become useless to him, this Greenwich time alone will not enable him to steer his proper course unless he sees the sun, whose bearing he must have now and then to regulate it by. Here then at the Pole difficulties *may* occur, that the more "undivided attention" of the navigator can not alone enable him to surmount. For at the Pole being left entirely *perhaps* without any guide, though he may chance to steer a true south course, he cannot possibly know to what point on the globe it may happen to lead him, until he is enabled to see the sun, which in all probability will be very generally obscured.

If he passes the Pole without any great difficulty, and finds the true south course he has steered, to be by chance, on or near the 170″ west meridian, and so leading him towards Behring Strait, he will, in all probability, soon get to the southward as far as 80°, or perhaps 78°, where it is *as probable* he will find his further progress stopt by ice, perhaps impenetrable.

From this part of the expedition, therefore, I see no very reasonable ground for entertaining "lively hopes," that a practicable passage for *ships* will be discovered into the Pacific, though there

there does not seem to be the least doubt of there being one for water and fish.

As to the second, viz. "the more circuitous passage along the north coast of America into the Pacific," the prospect of success is still more unfavourable than the other, because the navigators are destined in the first place "to struggle against the ice currents and tides in Davis's Strait, and on the east coast of America," which the writer of the article I have been examining, tells us himself, "are of course never free from mountains and patches of ice," and to which he attributes "the failure in every attempt, either to make" this (very) passage or to "ascertain its impracticability," "so that the highest point the former navigators ever reached, is the Arctic circle, or at most the 67th parallel."—But, even allowing that the present adventurers do reach the N.E. point of America, and discover a passage through what appears to be "gratuitously called" Baffin's Bay, they will then have to make no less than 100 degrees of Westing, most *probably*, through immense masses of ice, fixed, or moving with the circumvolving current as well as the winds, both prevailing in a general direction from west to east *against them.*

If there is any ground to hope, that a practicable passage for ships can be discovered between the Pacific and the Atlantic, along the north coast of America, the chances are that it will be done (if ever it is) *from* Behring's Strait to the *eastward*; and, therefore, it is much more likely to be accomplished (if at all) by the Russian officers, said to be making the attempt this year, than by ours; because, *most* of the obstacles opposed to the progress of our navigators from east to west, will be in *favour* of the Russians the other way, at least as far as the effects of winds and currents go. And though (as the writer in the Quarterly Review observes) it might "be somewhat mortifying, (to national pride and vanity, I suppose) if a naval power, but of yesterday, should complete a discovery in the nineteenth century, which was so happily commenced by Englishmen in the sixteenth," yet it is to be hoped, it will be no less *gratifying* to those who are disposed to estimate as highly, some of the still better feelings of our nature.

Notwithstanding the little reason we have upon the whole to *expect*, that *all* the objects of our expedition to explore the Arctic regions will be accomplished; yet it is *possible* some of them may: but at all events, I perfectly agree with the writer of the article I have taken the liberty so freely to canvass, "that the character of the several officers who have been appointed, and the men of science who are to embark on this grand enterprise, afford the strongest presumption, that whatever talents, intrepidity, and perseverance can accomplish, "will be effected:"

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at the same time, the *probable* obstacles they will have to encounter, should be fairly pointed out to their *full* extent, that the public may not be led, by a too flattering view of the matter, *to expect more* from the utmost exertions of these excellent officers, than known circumstances as well as conjectured probabilities would appear to justify. PHOCA.

LIX. *On Chemical Philosophy. By Mr.* MATTHEW ALLAN, *Lecturer.*

[Continued from p. 122.]

Essay III.

HAVING briefly stated my arrangement, and the reasons for the names and order of division which I have adopted in the mean time, for the purpose of conveying the views I entertain of the subject in a more definite and less equivocal form; we now come to the express object of my particular plan,—To apply these principles—to try their adaptation to facts and to nature. We come, if possible, to measure the dimensions of the trunk from which all the grand and all the numerous branches of science arise.

This Essay and the three which follow, are intended to include an abstract view of the principles which form the basis of the rest, and contain the principal ideas to be unfolded as we proceed through the whole.

To say that I conceive that attraction in general, gravitation, chemical affinity, electricity, galvanism, magnetism, caloric and light, arise from one power regulated by one law,—that their diversified effects are merely modifications which circumstances and substances produce on its actions—is easy. But to support this opinion by a clear explanation and exposition of facts, by pointing out what those circumstances are, and in what way they operate, is matter of some difficulty. I conceive it however to be a work which will tend to give us clear and simple views of each part of science, and of the whole combined, as one undivided, sublime and majestic fabric of nature.

Similar conjectures have been made by every philosopher of any consequence; without, however, as far as I know, any idea having been thrown out by which such apparent diversity might be explained: much less has any chain of reasoning been adopted to support such a view. Many expressions occur, it is true, by which we are given to understand that attraction and repulsion, &c. are merely effects: but these are so loose and so ill-defined, as to have been almost altogether overlooked by others. Many more again, on the other hand, have offered powerful arguments and objections against various assumptions on which modern science is is founded, without however submitting any thing better in their place.

"It is by attraction," says Newton, (by which he meant one grand power, without intending to convey any thing as to its nature,) "that we must expect to learn the manner of the changes, production , generations, corruptions, &c. of natural things, &c." Bacon too, who constantly recommends an inquiry into the physical causes and reasons of things, says, "that he who duly attends to the appetences and affections of matter (which both in the earth and in the heaven are exceeding powerful, indeed pervade the universe) will receive from what he sees passing on earth, clear information respecting the nature of the heavenly bodies; and contrariwise, from motions which he shall discover in the heavens, will learn many particulars relating to things below, which now lie concealed from us."

It is evident from this quotation, that Bacon considered attraction, or the general laws and principles of that power which moves and unites and regulates the different tendencies of matter (or as he expresses it, "the appetences and general tendencies or affections," which matter has to unite), to be the same, whether it acted on small portions or large masses of matter; whether on minute particles or on celestial masses; and that the actions of these two when viewed together, would tend very much to illustrate each other. We might quote without end passages to the same purpose from the ancients, as well as from all the moderns, Kepler, Copernicus, Fermat, Roberval, Galileo, &c. All of them had the same sublime views of the majestic simplicity of nature; and indeed with Dr. Hooke they prepared the way for the exertions of Sir Isaac Newton, who improved and extended the attraction of gravitation, and who first discovered or clearly stated the doctrine of attraction among minute particles, since called chemical affinity. He not only frequently intimates that the same power is applied to both particles and masses, but conjectures that the principles by which they are regulated are also the same, and that the apparent differences would vanish when they were better understood: "All those actions," says he, "by virtue of which the particles of similar and dissimilar bodies tend towards each other (and to which he applies the term attraction) are the same as that by which distant bodies tend towards each other."

"Again," says Lord Bacon, "the harmony of a system, each part supporting the other, is and ought to be the true and brief confutation of all minor sort of objections." "Again," says he, "it is the harmony of a philosophy in itself which giveth it light and credence; whereas if it be singled and broken, it will seem more foreign and dissonant." Again, in another place, "Generally

nerally," says he, "let this be a rule, That all partitions of knowledge be rather accepted as lines and separations, so that the *continuity and entireness* of knowledge be preserved:" for, says he "*the contrary hereof* hath made particular sciences to become barren and shallow and erroneous, while they have not been nourished and maintained from the common fountain."

Let it not be conceived that, in speaking of one grand power or agent in nature, I suppose that it is for us to understand its nature or essence, or even all its properties and effects. But what I contend for is; that I conceive, with Lord Bacon, "that it is unphilosophical to introduce a number of powers and agencies in creation, when one will better apply and answer the purpose." When clear conceptions of this one, and of the modifications which circumstances, substances, and time, &c. must produce on its actions, will not only better apply in explaining all the facts, but even particular anomalies and partial difficulties, which exist according to any other view;—when this one agency, in fact, enables us to ascend and descend another link in the grand chain of effects; it cannot be vanity to suppose this is truth, or at any rate a nearer approach towards it. Besides, what idea more sublime can be conceived, than that as by one power the present states and form and order of material existences were evolved; so also by the same power they are supported and preserved in that state, or changed from one state and stage of existence to another? The power which produces all this I have therefore defined as "that which produces all the motion and union of matter." Now, it is, I conceive, the object of CHEMICAL PHILOSOPHY to ascertain, or endeavour to ascertain, how this motion and union are produced: and I therefore consider this will be a more correct definition than the one usually given, "*that chemical philosophy is the science (or that part of our knowledge of nature,) the object of which is to investigate the movements and changes of the universe, and which endeavours to ascertain the nature and properties, together with the laws and principles and modifying circumstances of that* POWER *by which they are produced.*" The definition given of chemistry, I have already said, is partial and imperfect; and it is particularly so, according to these views; it in fact applies better to chemical affinity, as chemical affinity has in a great measure for its object the examination of the "*minute* and intricate changes of nature." Now *chemical science* is distinguished from this, as a whole is from its separate and distinct parts: it has to investigate the *general* operations and effects of this power; while chemical affinity treats only of some of those operations and effects:—the one is *general*, the other *particular*. These definitions cannot at present be either approved or condemned, until the *whole* of those we have to introduce are seen in connexion as one whole. I wish them, if possible, to be each descriptive of parts belonging to one whole; and to present the mind, when they are seen together, with a connected and combined view of the different objects of science, or of the operations, changes and phænomena of nature. An artist would not call separate limbs and features, put together regardless of proportion and harmony, a correct portrait; and still less do those definitions and views of different branches of science, which include too little or too much, give, however joined together, any correct representation of nature:—it would, however, be unfair to judge of the one or the other, in an unfinished form.

What then, it will be asked, is the nature of this power? and why in its actions does it produce such diversified effects and phænomena? Such questions require to be answered; for I have ventured to assert, "that I conceive one subtle and all-pervading power produces all the phænomena of nature and art; is the sole agent which creates or destroys, unites or separates, preserves or diversifies the forms of matter; that to its agency we owe not merely the subdivisions and convertibility of matter into solids, liquids and gases; their appearance in one state, and disappearance in another; their union with this substance, and their separation from that species of matter;—but also all their various degrees of density, of colour, of quality, of form, and of arrangement: that in fact it is the grand agent, the universal solvent, that energy which pervades the universe, and modifies all the powers and properties of matter; which in one quantity determinate in each species of matter, binds and unites it together, and is therefore its principle of aggregation or bond of union; which, increased in quantity, produces all its movements and changes, and is therefore the principle or power which alternately destroys or renews every state of its existence"—the GRAND POWER, which in different quantities, by its solvent and attractive properties, gives to and preserves matter in its various states and stages and forms of existence; which binds and unites its particles together, and by which also they are separated and carried from one point to another.

This power, therefore, I have defined as "that which produces all the motion and union of matter." This definition, without presuming to explain in full detail its essence, will serve to exhibit some idea, in the mean time, of its powers and its properties and its actions. It points it out as the grand agent, the cause alike of the inconceivably minute and infinitely extended movements and changes throughout nature. Some of these are presented to exercise our senses and advance our knowledge; the rest we conjecture by the powers of reasoning, the peculiar cha-

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racteristic of humanity, and no where so well exemplified as here, as in these boundless and seductive investigations of the wonders of space. The more extended effects, I am afraid, will require a more enlarged treatment than can be given in this form. The full explanation of these views cannot therefore at present be expected, nor even any thing like a complete answer to these questions:—What is the nature of this power? How can the same power produce such diversified effects? and, Why should its actions be attended with such diversified appearances? Yet they are questions of such high importance, that we may be excused if we should consider them at greater length than a mere abridged view would seem to warrant;—for what in science can be of more importance than clear views on the nature of this power? If we are imperfect in our conceptions here, all that follows will only tend to involve us in controversy and confusion: and it is evident that this must be the case, if it be true, that there are no separate and distinct causes for each of our artificial and arbitrary divisions of science. And is it not certain, that if this power has the properties and qualities which we have assigned it, then every effect and phænomenon can only be modifications which different circumstances and kinds of matter produce on its actions? That, in fact, if we have distinct views of its varied attractive relation to substances and of its solvent powers, by which in different quantities it changes or preserves their states of existence, then shall we not only know the effects which it produces, but perceive its principles or mode of operation, by which these effects are produced; and also, why certain phænomena attend their production? It is because I firmly believe this to be the case, that I have defined it, "the power which produces all the motion and union of matter;" a definition which is brief and comprehensive, which includes every other; and I conceive it ought not to do less, for it is the definition of the cause which includes every other cause,—if those phænomena and effects can with propriety be called causes, which, though they produce other phænomena and effects, merely result from or attend the various degrees of energy, or extent, or nature of those actions arising from differences in the quantity, intensity, and direction of the grand power, and the substances on which and circumstances in which it operates.

If then it be the grand power which in different quantities, by its solvent and attractive properties, gives to and preserves matter in its various states, stages, and forms of existence; and if during these changes it produces appearances as diversified; as these changes themselves differ in their nature, we shall not only perceive why one portion produces and preserves matter in one form, and an increased quantity changes this form; or why every substance varies in the relative quantity it requires to preserve it in one state, or change it into another; but also why, when we apply its concentrated actions (or heat, according to its common acceptation,) to substances; or when we mix bodies together, or change their relative position, this power effects their separation, motion, and deposition, or their solution and union; and if not soluble, moves them in one way and draws them in another; and all this invariably in the same order, and as invariably attended with the same appearances. And hence I have said that "it is not only the power which binds and unites the particles of matter together, but that also by which they are separated and carried from one point to another."

If then, I again repeat, different quantities are required for each species of matter, and if these quantities be relative as well as appropriate shares, then we perceive why every increase or diminution of this power, every change of position and arrangement, every solution of one substance in another, must alter these relative quantities, must take so much from one particle and give so much to another. It is thus one body is precipitated from it and another is dissolved in it; or if not soluble, they are drawn in one way and carried in another, in proportion as they have relatively to each other more or less of this power; so that "*all changes are mere exchanges of this power*," separating from one substance and entering into combination with another, passing from one and adhering to another:—Hence we have by these artificial means, as well as in nature, their separation, motion and deposition, or their solution and union, on their being moved from one point and carried towards another, &c.; and all this, as they are relatively to each other more or less soluble, or relatively require more or less for any given state of existence, is true, in those changes that are produced by simple mixture, as well as in those where heat, or the concentration of this power, is applied. The power then, I repeat, that accounts for minute changes, such as are in general exclusively considered under chemical affinity, is the same as that which produces the more obvious and striking changes; and they differ in effect and appearance, because the extent and rapidity as well as the substances on which, and circumstances in which, they are produced, are infinitely varied both in their quantity and intensity, their nature and their situation.

Essay IV.

Thus it is evident, that the power which we designate by the words *fire*, *heat*, or *caloric*, is very improperly restricted by modern philosophers to the mere solitary effects of heat and flame: for though these words *caloric*, *fire*, and *heat*, are all at present used by chemists and philosophers as expressive of the cause and the effect which that cause produces; yet in all their writings and

and lectures on this subject, they confine their attention so exclusively to the phænomena of heat and flame, as in the most unequivocal manner proves they conceive these features and appearances characterize its proper form and nature: that, in fact, caloric abstractly considered is something in its own nature hot and fiery.

This I conceive, as will be evident, is an erroneous and very imperfect conception of its nature and of its actions: and hence, on this subject there has been so much controversy and confusion of opinion. As, for instance, From whence is it derived? Whither does it proceed? Why is there frequently heat without light? Why, again, do they generally appear together? Whether are they in reality separate and distinct powers? Many contend they are mere properties of matter, and nothing in themselves. Some, that they are only the effects which the vibratory motion of the particles of matter produces. And what appears to me very curious, before any one of these questions is answered, almost all our modern philosophers have either been exerting themselves with all their might to prove, or have admitted as proved, that heat and light, while they have a repulsion for their own particles, possess not only an attraction for each other, but for all kinds of matter; and this is said even by some who at the same time affirm, that heat and light are nothing in themselves, mere properties of matter, or the mere vibration of its particles. Nor have these various opinions and contending theories at all diminished since the Lavoisierian theory of combustion; *proving* in my opinion most fully, that the views of the nature of this power and of its actions, are still erroneous and imperfect: and in fact it is now admitted, that this theory does not account for the heat and flame which appear in many instances; and I conceive it is very imperfect in its application to all of them.

Now, from the theory already briefly, and consequently from its brevity imperfectly, detailed as it is, it will, I conceive, appear evident, (and I trust still more so when we come to particular detail,) that heat and flame are the mere effects and phænomena which attend the more energetic operations of a power on which every motion, change, effect, and phænomenon, throughout nature depend: and though we do not apply the word caloric, as the cause of those changes which are more slowly produced, the power and the principles of its operation are the same, differing not in kind but in degree, and the nature and circumstances of its actions. Confounding the difference between that which may be the same in kind but very different in degree, is, on all subjects, the most frequent cause of error in our conceptions and our reasonings, and, what is worse, in our practice. Perceptible heat and visible flame are effects and appearances which, I repeat,

peat, depend on the nature and the degree of rapidity and extent of those alternate subversions and formations, transformations and transmutations, of the various forms of matter which one power produces. We are not, therefore, to confine the meaning of the word to the mere effects and appearances of heat and flame;—effects and appearances which are only the more striking features of a power on which every effect depends; the mere specific and determinate effect depending on the mode or degree or nature or circumstances in its operations—which in fact depends on the rapidity and extent and nature of the change produced, the rapidity and extent with which old combinations are subverted and new products formed by this power. The quantity of heat or flame (I say heat *or* flame, not *and* flame, for they are not in equal but inverse proportion) are in proportion to this. *It is during these actions of ignition and solution, of combination and consolidation of matter, that by its combination and concentration, disunion and radiation, its rapid movements to and from a point in this combined and condensed form, it produces, as well as these changes themselves, that also which is the most useful of all its effects, and that which is the most beautiful of all its phænomena,* HEAT *and* FLAME. Agreeably to these views, I define that part of our knowledge of nature, the object of which is to treat of heat and flame, or caloric, as that *which treats of some of those particular and more striking operations and effects of the* GRAND AGENT *on dissimilar kinds of matter, attended with heat and sometimes flame, and of observing and applying its actions in these states in order to ascertain its nature and produce other effects.*

I wish to be understood, that I do not conceive this power of nature is CALORIC, either abstractedly considered or according to the common acceptation of the word; but on the contrary, that heat is a mere effect, and that flame is a mere effect;—that heat and flame are *mere effects,* indicating some specific mode or degree in its operations. That this is the fact not merely as respects heat and light, but also electricity and galvanism; that it assumes either the form of electricity, galvanism, magnetism, caloric or light, when in its movements and changes which it produces on matter it combines with or separates from them in a more or less perfect manner, and in a greater or less degree, according to the nature of the substances on which, and the circumstances in which it operates. So true is this, that it may be reduced to the form of an axiom, "*that this power is either given out or absorbed in one form or the other in every change; and this in proportion to the extent, rapidity, concentration, and the nature of the change produced.*" From a partial view of this applied to caloric in its usual acceptation, Dr. Black

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formed this axiom, "*that whenever a body changes its states, it either combines with or separates from caloric.*" This has been found true to a much greater extent than was ascertained during the time of Dr. Black. His attention in forming this conclusion seems to have been chiefly directed to changes from the solid to the liquid, and from the liquid to the solid states. But it is now clearly shown, however slight the change in any one of these states, and however produced,—whether mechanically or chemically; whether by compression or the removal of pressure; in almost all instances of mixing substances together, and the solution of substances in liquids; and in all where the resulting density of the compound is not the mean between them in their separated states,—a proportionate quantity of this power in one form or another is either given out or absorbed: *substances give it out or demand it, in proportion as they are relatively to each other in excess, or defective.*

Is it not then most evident, that "*all changes are mere exchanges of this power,*" passing from one substance into another; separating from this, in order to dissolve and combine with some other species of matter? and in proportion to their solubility or insolubility they are united or separated from each other; and this in a more or less perfect and striking manner, in a greater or less degree, according to the nature of the substances, and the extent of the changes produced.

If then the science of chemistry endeavours to ascertain the laws and principles of that power which produces all the motion and union of matter, we have to explain what have been considered very different and opposite kinds of effects and phænomena; and this is still the production of the same power, and not, as has been considered, effects and phænomena belonging to another and opposite power, called repulsion. It therefore becomes necessary to state in what, according to my opinion, this difference consists. It depends, I conceive, on different degrees of the same power; which different degrees produce various effects, according to the circumstances in which, and the substances on which, it operates. In fact, in proportion to the quantity of this power and the time taken to act, and the solubility of the substances on which it acts, it tends more or less to change their state of existence. The quantity necessary to produce this change is equally necessary to support this change. Chemists have called this their relative capacity; as if water and steam differed in any way, and in the relative quantity of this power required for their respective states of existence. That water in the state called steam, occupies and suspends the active energies of this power, is evident from this fact,—that this gas or vapour does not scald more than air heated to that temperature; but when the

water

water is separated from it, the moment this power is liberated it instantly passes with all its energies and force into some other substances:—these actions on ourselves we call scalding, burning, heating, &c. So in making coal-gas, or any of the gases by heat, the pipes are hot only so long as vapour comes over, &c.

The atmosphere and all the permanent gases, I consider (and I trust in the subsequent Essays will prove satisfactorily) to be the same grand power holding substances in solution, by which its density is increased, its active properties and energies suspended or occupied in the exact proportion to its degree of saturation, or of their quantity in any given volume. Hence in elevated regions, with less oxygen and nitrogen and no moisture to act upon, its energies are not suspended; and thus its greediness for moisture occasions intense thirst, and evaporation unimpeded by pressure goes on with intense rapidity. I consider also that this atmosphere or the permanent gases are solutions of substances in this grand and universal solvent, and differ from the solution of water merely in this; that they are not separable from it at any known temperature; and that thus all above this point acts upon any other soluble substance, dissolves it, and mixes it with the atmosphere or any of these gases. Moisture in this way is dissolved in this power, and that in quantities in proportion to its excess; and consequently the quantity of moisture held in solution in our atmosphere varies with the temperature or uncombined state of this power, with season and with climate. Hence, when this excess or the temperature is increasing, and of course going before the saturated point, as in spring, we perceive the greatest difference between the spring and autumnal shower,—that vegetation goes on at a temperature in spring, at which it decays in autumn: and hence the difference of our feelings in one season and the other, and of our diseases; that at one season we have inflammatory and at the other putrid diseases; that a thorough knowledge of this principle, and of its influence, explains the diseases of climate and of situation. Hence also the explanation of many singular facts connected with magnetism and the polar regions (of which afterwards): that this power, not being separable from these permanent gases at any known temperature; that all above this unknown temperature, and under it, necessary to raise and suspend water, as at the polar regions, appears in this form of electricity, or in its most separate and pure state, (we can perceive it by the sense of seeing) as I consider its state in the form of electricity to be; and thus the northern lights change with the changes in temperature, and follow the parts where the largest collections of ice are formed, &c. All these and several other facts appear to me explained on these views. The power of the steam-engine depends

pends on rendering this power too dense to escape through the apparatus as it would do in its pure state. Its power is in proportion as its radiating energies are unoccupied, and yet sufficiently confinable not to pass through the vessel*; thence the powers of the air-machine lately discovered, (I conceive the word *discovered* is more correct than *invented:* the power is the discovery; the wheels, and pinions,&c. are the invention,) or, as it has been called, the rarefied air-machine. What is called the elasticity and compressibility of air is heat, or this power passing out and passing in: if altogether separated by compression from the substance held in solution, as in the German tinder-box, its elasticity instantly ceases. In short, not to dwell more on these points at present, it seems to me that certain appropriate quantities are inherent in all passive substances, and may therefore, probably, bind them together; and that all above this tends to dissolve and change their state of existence from the solid to the liquid, and from the liquid to the gaseous state; and if not soluble, to separate their parts or particles, more or less, according to circumstances. As for instance, a change of electricity will explode a small quantity of water, converting it into vapour; and if confined in a small tube, burst it with explosion, or cause pith-balls which are not soluble to diverge, &c.

The action of one quantity or proportion of this power, I therefore say, is necessary to the present relative order of things; and according to this order, and this state, is confined principally to the internal parts, and preserves and supports them in this relative and present form of existence. The other, or additional proportion, is the excess, and has the power more obviously of dissolving and uniting, or moving and separating particles and masses, carrying in one direction, and drawing in another. And in proportion to their solubility or insolubility they are united or separated from each other.

The first, or combined quantity, more particularly pervades passive substances throughout space. The last, the uncombined quantity, more particularly pervades space void of passive substances, and is the infinite medium or stupendous chain of connexion between them. This distinction, then, though it may in some measure be arbitrary and artificial; yet it comes near to facts, and the natural state of things; and it is one which I think will be found useful in assisting the memory, the only use

* This and every part of this view or theory of the subject was entertained in 1806, as may be proved by a correspondence with the late Dr. Fawcett, in Chester, if these letters are still in existence. I mention this, because many improvements in the steam-engine prove this view correct. To speak of the expansive power of steam, conveys to me a very imperfect and inadequate explanation of the mode of its action.

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of any such divisions. It will be found, too, convenient not only in rendering this theory of chemical philosophy, of electricity, galvanism, caloric, light, and chemical affinity, or chemical changes, more complete and more fully and easily comprehended; but also it will enable us with more facility to follow the application of these principles to astronomy, or mark those means by which one celestial mass influences another. It is from considerations such as these, that I have been induced to divide this power into two portions; by which it is not to be understood I mean any difference in the power, but in the quantity of that power and the circumstances attending the operation.

The one then, I call the *combined portion*, or that which is internal and inherent in passive substances; and the other I call the *uncombined portion*, or that which is in excess, and consequently is external and extraneous of passive substances.

The utility of this distinction, and its useful application to the phænomena of nature, will, I repeat, be perceived and acknowledged, as we proceed in our course to point out the heads and principles of chemical affinity; and I conceive I am warranted in adding, demonstrated in our application of them to electricity, galvanism, caloric and light, and still more so in proceeding to particular facts.

The existence of separate and distinct powers in nature is so far from appearing to me a maintainable position, that I cannot even form the most imperfect and vague idea of any such thing: but that one power should be modified in its effects and phænomena by circumstances and substances, is not only, *a priori*, a rational idea; but on a very slight investigation of nature it will be evident that such modifications must take place. We see it proved by every fact of nature and art, that there is some active grand and irresistible power and universal solvent; and if so, then, as it holds substances more or less in solution, it must in proportion to this be more or less attenuated or condensed, and consequently more or less rapid in its movements, and more or less energetic in its actions. All these and a variety of other things, infinitely varied in their nature and their proportions and combinations, must diversify the actions of this great agent, and produce phænomena and effects so infinitely varied and apparently widely different from each other, that, overlooking this view, we may cease to wonder that so many powers and agencies in creation have been introduced and enrolled on the list by modern philosophers; and which in my opinion threaten, by rendering science so complicated and confused, to extinguish that general love for it which must end in its monopoly by the few, and thus cease to produce its benign and liberal effects on the world. I contend that circumstances constitute the sole difference

ference of each; that all natural knowledge can only be the science of the *vis naturæ*; that whether we examine the bowels of our earth or the constitution and properties of our atmosphere, or ascend to the more elevated and sublime view of nature in the celestial circle of events, it is still a continuation of our investigations of the laws and principles and modifying circumstances of the same unlimited power of nature.

If it should appear that I repeat these ideas and vary the expression by which I convey them too diffusely, it arises from my anxiety to be fully understood; and I do this particularly from a conviction of its necessity—a conviction which has been produced by the difficulty I have experienced, in conversing on the subject for years past, in making these views fully comprehended, which I have been in the habit of doing almost incessantly, and with those most conversant with such things, some of whom seem to have imperfectly adopted them in their after writings. It is very different to have in our own minds the clearest perception of a subject, and the fullest conviction of its truth (which can only arise from having long confined our thoughts to such subject), and to be able to produce the same effect on another. Nor need we wonder at this, when we reflect on the amazing length of time and degree of labour it requires, even stimulated by an enthusiasm which cannot be transferred, to produce this effect on ourselves. I shall therefore be yet more particular.

By "*the combined portion,*" I mean that fixed internal and inherent quantity, of which I conceive every distinct species of matter has, in the same circumstances, its allotted portion, as that principle which gives, retains, and continues its form, properties, and state of existence.

By "*the uncombined portion,*" I mean that moving, external or extraneous or projectile or radiating quantity, which, in proportion to its abstraction or its excess, produces either the solution, deposition, or separation, motion, or change of place, of particles, or of substances.

Together and as one power they fix the place, regulate the form, and determinate the movements of matter, whether it respects the particles, or the aggregate masses of matter.

This science of attraction, then, not only investigates the force of that inherent or combined portion of this power, which is allotted to particles or to masses of matter, but it ascertains also the influence which its excess, or the radiating or uncombined quantity, has also upon them; or not only of that quantity which produces the union, but of that also which produces the motion of matter.

This science then has to ascertain the grand law by which this

this force and this influence are regulated, and mutually act on each other; and it is evident they act on each other with a force exactly in proportion as the one or the other is in excess or deficient. *In fact, there appears in nature a strong tendency to possess a relative and an appropriate share of this principle;* and whenever this distribution is disturbed or accumulated on one point, and abstracted from another, this power is at the same instant exerted. The one body is as ready to part with its excess as the other is to receive its deficiency, and this exertion to effect a proper distribution keeps up all the movements and changes of the universe. The quantity in that place which is overcharged, rushes to that which is undercharged, with a force bearing an exact ratio to the overcharge of one and the undercharge of the other, when nothing intervenes to oppose the action of this law. But as it is from substances intervening of different specific capacities and conducting powers, &c. that these commotions themselves take place, in its distribution this law is modified by an infinite number of circumstances, and of course produces, by the qualities of the particles, and these qualifying circumstances attending these agencies, all the various results which it is the object of chemical philosophy to investigate.—Whether these results are those of nature or those of art, and however varied the circumstances, the power and the primary law of that power are the same.

[To be continued.]

LX. *Description of Mr.* Henry Tritton's *improved Apparatus for Distillation.*

This apparatus, for which the inventor has obtained patents for the United Kingdom and the Colonies, is so simple, so efficacious, and at the same time so important in its results, that we are persuaded our readers will feel gratified in being made acquainted with it.

Mr. Tritton incloses the body of his still in an outer case filled with water in such a manner that no heat can get to the still, but through the medium of the water, and consequently no heat greater than the boiling point of water can reach its contents. Before commencing the distillation, the still and the other vessels connected with it are exhausted of air by means of an air-pump. A close condensing vessel is used, instead of the worm commonly employed, which is connected with a close receiver by means of a pipe; and in the pipe there is a stop-cock by which this communication may be closed as occasion requires. The receiver has on its top a stop-cock for the

the admission of the air when necessary, and both the receiver and the still have proper discharge cocks, and may have man-holes or cap-screws in any convenient situation for cleansing them, and any other useful appurtenances.

The construction is shown in Plate II. and will be easily understood from the following description:—A is the still; LL the outer case attached to it; B is the close condensing vessel; C is the close receiver, having an air-pump E on its top, and also a stop-cock D; H is the discharge cock of the receiver; I the discharge cock of the still; G is the stop-cock in the pipe connecting the close condensing vessel and the receiver. The condenser and receiver are also in cases, but surrounded with cold water.

In this improved apparatus, the produce in the receiver may at any period of the distillation be examined without interrupting the progress of the operation; for by turning off the stop-cock G, so as to close the communication between the close condenser and the receiver, the latter may be opened, without suffering air to get into the other vessels, so as to stop or impede the distillation. The advantages presented by this invention are many and important. By the removal of the pressure of the air from the surface of the liquid in the still, a very moderate heat suffices to effect distillation—far less than in the common process; and a great saving of fuel is effected, which (for the colonies especially) is a consideration very important.

To show the extent of this benefit, it may be stated, as the result of repeated experiments, that distillation in this improved apparatus takes place at a temperature full 80° below the common boiling point of 212°. By the regular application of this low degree of heat through the medium of the liquid in which the still is immersed, any empyreuma is entirely prevented, and the produce in the improved apparatus is rendered far superior to the best produce of common distillation. No accident of burning to the bottom can occur in this apparatus, nor can loss by evaporation, inevitable at the worm's end in the common mode, and sometimes great when too fierce a fire is kept up. The durability of the vessels is also promoted by this new construction and mode of operating; and as less heat is required for distillation with this apparatus, so less cold water is wanted for condensation, which in several of the West India islands, that have but a very scanty supply of that useful article, is a very desirable saving. These advantages have been proved by many decisive experiments, and the quality of the spirit obtained by distillation in this improved method has been pronounced unrivalled by gentlemen eminently competent as judges on the subject. Those who are interested in such improvements may see the apparatus at work at the premises of the patentee, No. 63, Whitechapel, London, who has the permission of the Honourable Commissioners of His Majesty's Excise to erect his improved apparatus for distilling, for the use of distillers, rectifiers, &c.

Copy of a Letter from Joseph Benwell, *Esq. (a Gentleman of long Experience, and of great practical Skill, in the English Malt Distillery,) addressed to Mr.* Henry Tritton, *on the Subject of his Patent improved Apparatus for Distilling.*

Henley, Oct. 9, 1817.

Dear Sir,—Having attended repeated trials made with your apparatus for producing a vacuum and distilling by the combination of the balneum with the air-pump, and having considered the principle thereof,—I feel much satisfaction in communicating my full conviction, that it is a mode by which a purer spirit will be extracted, than by any other that has been hitherto practised; that a considerably less proportion of fuel will be requisite, and that the operation may be performed with equal facility. I remain,

Dear sir, yours very sincerely,

Joseph Benwell.

To Henry Tritton, Esq.

Copy of a Letter from Mr. William Allen, *(Fellow of the Royal and Linnæan Societies,) addressed to Mr.* Henry Tritton, *on the Subject of his Patent improved Apparatus for Distilling.*

Plough Court, Lombard Street, Oct. 7, 1817.

My Dear Friend,—I consider that the plan for producing a vacuum, or even a partial vacuum, in the vessels destined to receive the products of distillation, is a great improvement: for, in proportion as the vacuum is rendered more perfect, the spirit will be drawn over at a lower temperature, and will be more fragrant, and better in every respect; and the still being surrounded with water, the heat can never rise beyond two hundred and twelve degrees; and consequently, that unpleasant smell which the spirit always has when the matter in the still is overheated or burnt to the bottom, is entirely avoided. It is further obvious, that upon this plan distillation may be carried on with a smaller expenditure of fuel than upon the old system.

I remain, thine sincerely,

William Allen.

To Henry Tritton.

LXI. *Account*

LXI. *Account of a Shower of Meteoric Stones which fell in the County of Limerick.* *Communicated by* William Higgins, *Esq.*

To Mr. Tilloch.

Sir, — I send you a copy of a letter which I have received from a gentleman of the highest respectability, who was an eye-witness to one of the most remarkable showers of meteoric stones on record. This shower fell in the county of Limerick.

The information with which I present you, was in answer to the following queries, which George Tuthill, Esq. of this city was good enough to transmit to his friend in Limerick, soon after the event occurred.

1. Have any persons seen the stones in the act of falling?

2. How soon after the large stones fell were they discovered? and were they hot?

3. Was the fall accompanied by thunder and lightning; and if so, was there but one clap and one flash, or how many?

4. What was the state of the weather?

5. What is the shape of the larger stones?

6. Have smaller stones fallen at the same time, and at what distance were they found from the larger ones?

7. Were there appearances of recent fractures on the surface of the large masses; and if so, whether those fractures corresponded in shape and number with the small fragments?

In consequence of the foregoing questions, I received the following letter:

"Limerick.

"Sir,—Friday morning, the 10th of September 1813, being very calm and serene and the sky clear, about nine o'clock a cloud appeared in the east, and very soon after I heard eleven distinct reports, appearing to proceed from thence, somewhat resembling the discharge of heavy artillery. Immediately after this followed a considerable noise not unlike the beating of a large drum, which was succeeded by an uproar resembling the continued discharge of musquetry in line. The sky above the place whence this noise appeared to issue became darkened and very much disturbed, making a hissing noise; and from thence appeared to issue with great violence different masses of matter, which directed their course with great velocity in a horizontal direction towards the *west*. One of these was observed to descend; it fell to the earth, and sunk into it more than a foot and a half, on the lands of Scagh in the neighbourhood of Pobuck's Well, in the county of Limerick. It was immediately dug up; and I have been informed by those who were present, and on whom I could rely, that it was then warm, and had a sulphurous smell. It weighed about seventeen pounds, and had no appearance of having been

 fractured

fractured in any part, for the whole of its surface was uniformly smooth and black as if affected by sulphur or gunpowder. Six or seven more of the same kind of masses, but smaller, and fractured, as if shattered from each other or from larger ones, descended at the same time, with great velocity, in different places between the lands of Scagh and the village of Adare. One more very large mass passed with great rapidity and considerable noise at a small distance from me; it came to the ground on the lands of Brasky, and penetrated a very hard and dry earth about two feet. This was not taken up for two days;—it appeared to be fractured in many places, and weighed about sixty-five pounds! Its shape was rather round, but irregular: it cannot be ascertained whether the small fragments which came down at the same time corresponded with the fractures of this large stone in shape or number; but the unfractured part of the surface has the same appearance as the one first mentioned. There fell also, at the same time, on the lands of Faha, another stone, which does not appear to have been part of, or separated from, any other mass: its *skin* is smooth and blackish, of the same appearance with the first-mentioned, and weighed above twenty-four pounds. Its shape is very irregular. This stone is in my possession, and for its volume is very heavy.

"There was no flash of lightning at the time of, or immediately before or after, the explosion; the day continued very calm and serene; was rather close and sultry, and without wind or rain. It is about three miles in a direct line from the lands of Brasky, where the very large stone descended, to the place where the small ones fell in Adare, and all the others fell intermediately; but they appeared to descend horizontally, and as if discharged from a bomb and scattered in the air.

"I am, sir,

"Your obedient servant,

"SAM. MAXWELL.

"*William Higgins, Esq.*
"Dublin Society-House."

There is no phænomenon in nature so strange or so difficult to be accounted for, as the existence of meteoric stones in the atmosphere, and the circumstances attending their motion and descent to the earth. The fiery meteors which deposit them are often seen at a considerable height above the clouds, moving in a horizontal direction with great velocity, but gradually approaching towards the earth. When they reach within a certain distance of it, or when they meet with clouds, the phænomena of thunder and lightning are produced, the ignition ceases, and the stones come down, most frequently shattered into masses of different sizes, with the effects of fusion, without exception, on

their

their surface, the fractured parts excepted, although internally they exhibit no such appearance.

In whatever part of the world those stones are found, they exhibit very nearly the same appearance as to colour, texture, fracture, &c. and on analysis give the same ingredients, sometimes varying very little in their proportions.

The stone which fell a few years ago in the county of Tipperary, and which weighed seven pounds and a half, was found by my analysis to consist of the same substances with many which had fallen on different parts of the globe, according to the analyses of Mr. Howard.

The following are the constituents of those stones, viz.

Silex in large quantities.
Magnesia.
Iron in its metallic state.
Nickel in small proportions.
Sulphur and oxide of iron.

As no other mineral substance hitherto discovered on our globe consists of the above ingredients, we must consider them as foreigners. Some philosophers suppose that they are projected from the volcanoes of the moon—that they are projected from the earth by means of volcanoes—that they are produced in the atmosphere by the gradual accumulation of minute and invisible atoms, &c. But as these speculations are inconsistent with sound philosophy, or even with plausible hypotheses, I shall drop the subject here.

It is supposed by Cladini that they never belonged to any planet, and that they were opaque wandering masses, before they reached the confines of our atmosphere. This, certainly, is the most rational mode of accounting for their presence in the situation in which we first behold them in the atmosphere.

However, to account for their becoming luminous or red hot, when they descend into the upper regions of our atmosphere, regions of eternal frost, has been a desideratum with me, and engaged much of my attention some time past.

These masses, like all other ponderable materials, contain specific heat round their atoms and particles; in moving through the atmosphere they collect electricity; and this continues increasing, as there is no other solid matter in those upper regions to prevent its accumulation*. When they acquire a sufficient quantity of electric matter, the entire or a portion of their specific heat is liberated, and much of it is thrown on their surface; this

* The upper part of the atmosphere which extends beyond the reach of clouds, contains a considerable quantity of free electricity, as the phænomenon of the *aurora borealis* sufficiently evinces.

gives the luminous appearance: as they contain much iron and sulphur, a portion of oxygen unites to their external parts. The degree of heat produced by these different circumstances will account for the superficial fused crust which invariably surrounds these substances. It is probable also that a quantity of electricity collects round those masses, so as to form a considerable and dense atmosphere, and that this electric atmosphere as they move along keeps the air in contact with them in a constant blaze.

These electric stones in descending towards the earth, when they meet a cloud comparatively negative, lose a portion of their electricity; which bursting forth with great vehemence exhibits the phænomena of thunder and lightning; at the same time that they are most commonly shattered into pieces. So soon as this takes place, their luminous appearance ceases, their specific heat resumes its former station, and they are precipitated to the earth, still retaining a considerable degree of heat. The stone that fell in the county of Tipperary could not be touched with the hand some time after its descent.

It is somewhat strange that those meteors should be found to move from E. to W., which is contrary to the motions of the earth; unless it had been occasioned by the electrical explosion, which might have scattered the stones in every direction by its violence. It is impossible that such explosions could be produced but by means of electricity: therefore, it appears rather singular that they should not be accompanied with lightning, which is generally the case; but probably the opacity or darkness of the clouds, during the fall in the county of Limerick, rendered it invisible. I am, sir,

Your very humble servant,

W. Higgins.

LXII. *Observations on the Letter of Mr.* Friedlander *on the actual State of Magnetism in Germany.* *By* M. C. Oppert, *M.D.**

The readers of the Gazette of Health will find in the first number of that work for 1817, a letter from Mr. Friedlander to the editor of that journal, On the present State of Magnetism in various Parts of Germany.

The author, who has just performed a rapid journey through that country, begins by giving a succinct account of the history of a sick person whom he saw at Hamburgh, who, having followed a course of magnetic treatment for several months without deriving therefrom any benefit, had been prevailed on by Mr. F.

* Translated from the German.

to

to abandon it. Going thence to Berlin, where magnetism is practised with more success and more zeal than any where else, he describes what he believes he there observed;—he displays, or rather he interprets, the opinion of the most eminent physicians of that metropolis;—he then gives his own particular opinions; and resting on general considerations, he endeavours to deny most of the effects of magnetism, by attributing to the force of imagination those effects the existence of which he is obliged to acknowledge.

The incorrect statement of facts related in that letter, and the false consequences drawn from them, impel me to answer the author. It is not my intention to enter on any controversy on the reality of the effects of magnetism,—on the methods employed in the practice of it,—nor on the applications which have hitherto been made of it for the cure of different diseases. These different subjects have already been treated in so many writings, that it appears unnecessary for me to discuss them anew. But I believe I owe it to this science and to truth, to rectify those relations, the incorrectness of which obscures the light of a doctrine which ought to be made perfectly clear; and to repel a satire the more misplaced, when attempting to throw ridicule upon subjects which from their nature ought to be treated in the most serious and even solemn manner.

Educated at Berlin, where I devoted five years to the study of medicine, I did not neglect any thing from which I could derive instruction in that science: and I believe I have it in my power to explain to philosophers and the learned, those facts, most of which are misrepresented in the letter of Mr. Friedlander.

The institution of Mr. Wolfart, Professor in the University of Berlin, is the principal object of the descriptions and satire of Mr. F. It has been established between ten and eleven years; and it has extended itself and obtained more success every year. The discovery of the property of the agent denominated magnetic fluid, of passing to unorganized bodies, impregnating them during a certain time, and of passing thence into the human body, suggested the apparatus known by the name of *baquets*. These are isolated vases filled with water, magnetized by a proceeding similar to that by which we magnetize human beings; and which thus become in some sort reservoirs of the magnetic fluid. Iron or steel conductors emerging from the water, direct the fluid to the patients placed round the baquet.

The necessity of multiplying the means of distributing magnetic aid, made it advisable to have recourse to those apparatus; for one magnetizer could not have strength or time sufficient to magnetize a great number of patients.—Experience has brought to perfection their use; and the luxury which prevails even in the simplest retreats of science, has given to these machines an elegance under which the sceptical think they trace mystery and chicanery. Mr. Wolfart's baquets were at first of the most simple construction; those of other physicians, whether in Berlin or any of the smaller towns of Germany, are so still: in fact, they are nothing but vessels filled with magnetized water and furnished with steel conductors. In the course of his practice Mr. Wolfart made additions to this apparatus; some of no importance, and others useful in promoting the circulation of the fluid. He inclosed the vessel of water in a mahogany case, which Mr. F. is pleased to compare to an altar, with the same aptitude with which he compares to a sanctuary a room in no respect different from any ordinary apartment. A steel rod is immersed perpendicularly in the middle of the vase, woollen ropes of about three lines diameter are attached to this conductor, and the patients surround with these cords such parts of their body as they conceive to be the seats of their disease. Mr. Wolfart thinks they increase the effect of the apparatus, by conducting the fluid which emanates from the reservoir. They are not of silk, as Mr. F. states them to be, but of worsted; for, according to Mr. Wolfart, silk will not serve the purpose of a magnetic conductor. As to the brass wires of which Mr. F. speaks, there are not any in the machine; and he is deceived in imagining that he recollects any communication established from the baquet to the sides of the apartment; for this would be entirely useless. In the upper part of the mahogany case there is a space above the baquet, which is filled with lambs' wool. The wool thus placed becomes charged with the magnetic fluid; and is applied in several local affections, to continue on the parts affected the constant influence of magnetism. Above the machine Mr. Wolfart has suspended a glass globe, quicksilvered like a looking-glass, which communicating with the central conductor by means of a woollen cord, partakes of the properties of the machine. He believes, in common with many magnetizers, that magnetism is propagated like light by irradiation; that, concentrating itself in a body within the glass globe, it is thence radiated so that its effects are diffused every way. For this purpose, and not to use as a lustre, he added a ball of glass to his apparatus.

Mr. Wolfart makes use of the baquets as they were at first used in the magnetic treatments. The patients seated round it take hold of the steel conductors, to draw thence the streams, which they direct according to their complaints. They experience nearly the same effects as from direct manipulations,—cessation from fatigue; the sensation of warmth; perspiration; every degree of sleepiness, and somnambulism;—indeed, all the effects consequent on direct manipulation are produced equally in the individuals

dividuals who have recourse to the baquet, more or less strongly, as they may be more or less sensible of magnetic action. I should repeat what has been said a thousand times, were I to attempt here to detail the course of those phænomena which, notwithstanding an infinity of hypotheses, are still incomprehensible.

To convince oneself of their reality, a transitory visit, a superficial view, is not sufficient:—an attentive examination, impartial researches, and observations closely followed up, are necessary to give this conviction. Had Mr. F., in his examination of this, exercised the same impartiality of which he has given proofs in other branches of science, he could not have stated the cases with the want of accuracy so evident in his letter: he would not have permitted himself to criticize so unjustly; he would not have endeavoured to deter the physicians of France from making trial of a means of cure, which, under their direction, would produce results the most advantageous to the progress of physiology and of the art of healing.

Mr. Wolfart's institution has existed, as I have already said, above ten years, and his practice has gradually increased. Most of his patients are individuals who had already exhausted all the resources of medicine;—yet this serves only to enhance the value of that agent of nature of which the powers are but beginning to be known, and to increase our respect for those physicians who are willing to study them and superintend their effects. Mr. Wolfart's success has been very greet: and as his experiments have been multiplied, the confidence of the public has increased; and the number of the sick who have resorted to his treatment has become so considerable in these last years, that he found himself under the necessity of sending some of these patients to other gentlemen of the faculty who have established treatments similar to his own. As to the result of the numerous experiments made by Mr. Wolfart, the method he follows, the theories by which he explains these effects,—it is to him we look for a detail of these, which he will not delay any longer than until the labours which he has undertaken shall afford him leisure:—to anticipate the explanations he intends to offer, would be both hazardous and indiscreet.

To one assertion of Mr. F. I must however reply in this place, because it might give ground for a serious attack on Mr. Wolfart's system. It is, that this gentleman prescribes and distributes medicines to his patients—so that the salutary effects of his treatment ought to be attributed to these common remedies, and not to magnetism. In the first place, these remedies are very simple, and have for the most part been employed only to acquire

quire the confidence of patients of that class of persons who think it impossible to relieve them without giving them some medicine to take. In the next place, Mr. Wolfart's institution not being intended for the trial of experiments, but for the cure of disease; this gentleman made use of all the means which his previous studies afforded for the attainment of that end, in the quickest way. Being perfectly aware of the effects of the several medicines, and of the benefit arising from their association to magnetism in the cure of certain diseases, he would not renounce their aid, to satisfy those persons who would have magnetism treated like the physical sciences, without regard to the cases of those who submit themselves to its action. Besides, the establishment of Mr. Wolfart was not a criterion at the time of Mr. F.'s visit: it was not under the direction of a medical practitioner; and though Mr. Wolfart permitted such of the faculty as had the curiosity to go into it, it was neither to instruct them in his system, nor to try to make proselytes to his doctrine: for truth will in the end never fail to establish herself. In short, it must be remarked,—and this entirely removes the objection to which I have been replying,—that in the number of persons who presented themselves, internal or external remedies were not administered to a fourth part of them. There is no doubt that from henceforth Mr. Wolfart will altogether give up their use; his institution being now raised to the rank of a medical academy destined to the purposes of instruction, magnetism will be exclusively adopted, in order to preserve the purity of the experiments: but for that reason he will send out to another treatment such of his patients as may require the union of medicine in the aid of magnetism. As to the credit enjoyed by magnetism at Berlin as a means of cure, it may be said that it increases every day, and that the most eminent physicians of that city hold this to be one of the most efficacious remedies yet known. They are far from considering it a specific in all cases; nor do they resort to it in such slight affections as yield easily to the common medicines; but numerous examples, observations collected from all quarters, have at length placed beyond all doubt that it is a valuable resource in different disorders, and, in many which are beyond the reach of medicine, that it is of inestimable utility. It frequently happens that Drs. Hufeland, Heim, Formey, and other celebrated physicians, send to Mr. Wolfart's institution patients who have been long under their hands without deriving benefit, and these persons leave the institution entirely cured by the sole application of magnetism. Nor are these cases rare: I have often witnessed them, and every inhabitant of Berlin may assure himself of their truth. That magnetism does not always cure;

cure; nay, that in certain cases it does not appear to have any effect, cannot be denied;—but unfortunately we are not more advanced with respect to most of the other remedies in our art: we do not know their manner of acting; we cannot determine with certainty what effects they will produce; we fail repeatedly in employing them in diseases of the nature of which we know no more than of the remedy; nor as yet are the physicians of Berlin, most of whom have recourse to magnetic treatment in certain cases, quite agreed as to the extent of its efficacy. Some apply it without distinction to every internal malady; others limit its application to determined cases. But in this respect magnetism shares the fate of all other remedies, which at one time rise, and at another fall, in the opinion of the faculty. Dr. Hufeland has himself too often and too clearly declared his own sentiments, to leave it to any one else to interpret them. He acknowledges the efficacy of magnetism; he ranks it among the most powerful remedies in his materia medica; and it is inconceivable that Mr. F. should appear to know nothing of this. Dr. Heim, Dr. Formey, and other eminent physicians, are of the same opinion. There are, indeed, a few who cannot yet bring themselves to admit the existence of a power of which they have no conception: but these persons are too wise and too modest to judge of and condemn what they as yet know very imperfectly.

The public usually sides with those physicians who are most esteemed. The majority are by this time acquainted with the effects of magnetism, and the admirable phænomena which sometimes accompany it. The great influx of sick persons to the magnetic treatment of Mr. Wolfart and of some other physicians, is sufficient proof of their confidence in it.

Persons of the highest consideration are not above inquiring into all that this new doctrine offers, that is remarkable and interesting; but indeed here all the mystery and juggling, which in other countries have been so inimical to the propagation of this discovery, have been carefully avoided. No man thinks of establishing a magnetic treatment to make his fortune. At Berlin, as in the greatest part of Germany, the disinterested love of truth and the most rigid principle reign in the empire of the sciences.

The government in Prussia has not seen with indifference the progress which the doctrine of magnetism has made towards perfection. Too enlightened not to be sensible of the advantages derivable from its protection of the science and of individuals, but too circumspect not to foresee the abuses which might counterbalance them,—it has taken such measures as are favourable to them, while they prevent the other. The public

practice

practice of magnetism has been forbidden to all persons who have not attained a knowledge of the elements of medicine, and who have not received a certificate, proving them duly qualified, from the committee appointed to examine the capability of the physicians. By this means there is now nothing to fear from a power which, in the hands of the ignorant, of enthusiasts, and quacks, might have been subversive even of social order. On the other hand, the government has raised magnetism to the rank of those sciences which are to form a part of the public instruction:—it has named Mr. Wolfart, Professor in ordinary of the Faculty, and Director of a Magnetic Seminary which it has added to the numerous useful public establishments existing at Berlin. Magnetism is thus placed within the observation of such of the faculty as can appretiate its advantages and its inconveniences, and separate that which may be illusory or exaggerated from that which is incontestably true.

The practice of magnetism is more or less known in the rest of Germany. I have travelled over a part of it, and have found that it is generally pursued with tolerable impartiality and zeal. At Vienna it had been prohibited by government; it was employed notwithstanding, and that even publicly. At the time of the Congress, several magnetic experiments were made in presence of the august strangers there assembled, which succeeded to their entire satisfaction. In fine, last summer the prohibition was entirely taken off by a government order, and the practice of magnetism was confided to medical gentlemen particularly approved. On this subject, Mr. F. seems ignorant of this decree, for he says nothing of it in his letter. He is doubtless not aware that the King of Sweden has just sent an eminent physician to Berlin to be instructed by Mr. Wolfart; and that the Emperor of Russia has sent thither Mr. Stoffregen, first physician to the Empress. Other foreign physicians have visited Berlin of their own accord, with the same views. This is sufficient proof that in the Northern countries magnetism is not regarded as a chimera.

In fact, it is time to leave off disputing the reality of a thing which has for the last thirty years occupied the attention of the public. Nothing is more easy in medicine, than to reason, as Mr. Pinel frequently says: but at the same time nothing is more useless: indeed, nothing tends so much to prevent real observation and accurate experiment. This has unfortunately been the lot of magnetism. Each man arrogates to himself the right of reasoning and deciding; they exhaust the arguments *à priori*, drawn from the elements of physiology: but no one examines into facts; no one gives himself the trouble of making accurate and

and continued researches. Ask these reasoners, who fill the medical and literary journals with their satires and declamations, whether they ever applied seriously to the study of the doctrine they attack;—in all probability, not one of them grounds his assertions on experiments tried by himself. Many of them fancy it would derogate from their dignity, even to make an inquiry on a subject against which they are prejudiced. Since the publication of the first Memoir of M. de Puységur, printed in 1784, a great many collections of facts have been published both in France and Germany. Some of these were probably of doubtful authority, but many were authenticated. The works of Messrs. Wienhold, Gmelin, Kluge, and still more recently that of Mr. Deleuze, contain a body of doctrine not one word of which is contrary to the actual state of the medical sciences. The phænomena there mentioned are given without exaggeration; the proofs discussed with impartiality: and yet the enemies of magnetism repeat the objections which are there refuted:—if they have taken the trouble of reading them, it must have been very superficially and with strong prejudice. In quoting from them, they cite a few sentences, which being detached from what precedes and from what follows, give occasion for ridicule. I do not see any candour in this mode of making quotations, but there is certainly in it great levity; and it is not by such means that they can ever arrive at positive results.

Magnetism had the misfortune to fall during the infancy of the discovery into the hands of individuals entirely ignorant of the elements of physiology and medicine. Some persons perverted it for their own interest, and thus abused a doctrine which they were not called on to profess, and the truth of which they even brought into disrepute by the pretensions of quackery. The regular physicians withdrew from the practice in proportion as these empyrics gained ground: they abandoned the practice of magnetism, and endeavoured to bring it into contempt. In consequence of this, for a long time the examination of the phænomena it presented was given up to men who had not sufficient instruction to weigh the circumstances in order to connect and compare them with other phænomena of nature. We are still engaged in establishing facts which have been seen a thousand times; in seeking the laws by which they must invariably be reproduced; in distinguishing what belongs to this new principle, from what might be effected by other causes. Those sciences which treat of the physical properties of animal bodies have advanced more rapidly. Electricity, galvanism, mineral magnetism have been studied; and although the phænomena which they present are neither less singular nor less inexplicable than those of magnetism, we have not neglected to examine, to establish,

establish, and to register them.—The contrary is the case with magnetism. They reason, they dispute, but do not examine; they judge without having observed; conclusion comes before experiment, instead of after; and how many prejudices, how many prepossessions, how many interests produce these conclusions! It is impossible to mistake them in reading the opinions, the decisions scattered through the periodical papers. If the medical men and the wits (for wits too will argue on medicine) would study the doctrine which they oppose, the truth of it would soon be evident to them; and we should then perhaps hear them giving to magnetism panegyrics as exaggerated as their present criticisms.

C. Oppert.

P. S.—My Observations were in the press when a letter was communicated to me of the Count de Lœwenhielm, minister plenipotentiary of the King of Sweden at the Court of Russia. This letter bears date Stockholm, 7th October 1816, and the person to whom it is addressed has given me permission to transcribe thence the following paragraph:

"I forgot to tell you that at Berlin Magnetism, known by the name of *Mesmerism*, receives much greater honour than with you or even with us. The King of Prussia has just appointed Mr. Wolfart, Professor of Mesmerism in the Academy; he has also founded an hospital of a hundred wards for the wounded, who are to be placed under exclusive magnetic treatment.

"I have just learnt from M. de Cederschoeld, a very eminent physician of Stockholm, whom I sent to Berlin at the expense of the Court, that Mr. de Stoffregen, physician to the Empress of Russia, and Mr. Malfatti, physician to the Court of Vienna, are also sent to confer with Mr. Wolfart, and to learn his practical system in the mode of using the *baquet*, a system founded on the surest principles of approved medical science."

LXIII. *Observations and Experiments on oxysulphuretted Muriatic Acid*; by the Marquis* Cosmo Ridolfi†.

While the important uses of oxygenated muriatic acid gas are daily multiplying in the arts, and many of its new combinations and properties are discovered with great advantage to chemists,

* However extraordinary some ideas contained in this memoir may appear, the author seems to have used so great circumspection in the experiments, and announces his hypothesis with so much modesty and diffidence, that we apprehend his labours will be acceptable to chemists.—Edit.

† From *Giornale di Fisica, Chimica, &c.* of Pavia, by Brugnatelli, Brunacci and Configliachi.

mists, its theory is still a subject of controversy; and its nature, to the disgrace of the first philosophers of the age, still remains enveloped in obscurity. Important facts exist in support of the ancient theory of Vauquelin and Berthollet, &c.; and many German chemists, at the head of whom is Berzelius, wish to preserve it against the delicate and ingenious experiments made by the celebrated Davy to establish his theory of chlorine, in which he is followed by the French and some Italian chemists. Since then Europe is divided in opinion, and the facts already known respecting this substance are not sufficient to settle the controversy, it would be too bold to pronounce sentence without adducing, as motives of the decision, new facts and conclusive experiments. It is therefore desirable that all chemists would communicate to the public the facts which present themselves in operating on oxygenated muriatic acid and its infinite combinations, whence some able chemist might combine, and, comparing them, decide the question. If I do not deceive myself, I hope that I have not been altogether unsuccessful in the following few simple observations on oxysulphuretted muriatic acid, although they are neither so complete nor extensive as they might have been, owing to some personal circumstances which impeded me*. I ought also to add, that I was led to these experiments by the reasonings of my friend D. Taddei, who assisted me in a great part of them; and hence they are announced under the name of both. We have already published our first attempt to demonstrate the presence of oxygen in oxysulphuric muriatic acid, and consequently support the theory of Berthollet. I have only to add, that the oxygenated muriatic gas which was passed over sulphur as pure and dry as possible, was passed over muriate of lime in a thousand different ways, to obtain the highest possible degree of dryness; and that, previous to beginning the experiment, it was endeavoured to fill the whole capacity of the apparatus with the same gas, thus expelling the atmospheric air which it previously contained. The sulphuric acid, in which the sulphur was changed by the action of nitric acid, was as highly concentrated as possible. Encouraged by the satisfactory result of our first attempt, we made others corresponding.

Over 315 parts of oxysulphuric muriatic acid put into a vessel full of muriatic acid gas, we threw 20 parts of pure and very dry phosphorus. By simple contact only, after some hours the

* In a note the author states these to be the injurious effects of oxymuriatic acid gas on his lungs, exciting cough, &c. which have compelled him to abandon the inquiry. It may be proper to observe, that this gas is not so dangerous to many others who are daily employed in preparing it for the arts, and that in several manufactories visited by the translator he never heard its being considered as extremely insupportable.

phosphorus

phosphorus was melted without any sensible increase of temperature, and liquor placed over it lost its red colour, becoming yellowish and somewhat turbid. As it appeared that by repose no subsequent change could take place, we applied a moderate heat; and fixing a bent tube in the vessel in which we operated, carried it to a bell-glass receiver over mercury. A strong ebullition immediately took place, and continued as long as there existed any uncombined phosphorus, although at its commencement the apparatus was removed from the action of the fire. The mass became of a transparent citrine colour, and an elastic fluid was separated during the experiment, which we recognised to be pure muriatic acid gas. As the experiment was made in a retort, distillation was easily effected without permitting the contact of the atmosphere: the muriatic acid passed over, and a little sulphurous acid; the sulphur was sublimed; a mass remained having a red colour, and the density of paste. Exposed to a naked fire without removing it from the same receiver, sulphur was again separated from it, and a white solid mass remained, soluble in water and with all the properties of phosphoric acid. This weighed 60 parts. Hence the phosphorus was found increased in weight by 30 parts of oxygen. Now in 315 parts of oxysulphuretted muriatic acid, there exist 24 of oxygen: thus we see that there was no other loss in this experiment but four parts of oxygen, which probably constituted the very small portion of sulphurous acid. The existence, therefore, appearing evident of a quantity of oxygen in this liquor, in such a state as to form new combinations with bodies greedy of it, we believed it our duty not to omit experimenting with carbon, thinking that by this means the theory of Berthollet might be established; since, if carbonic acid were formed, we should have a valuable fact to add to those already stated. Taking therefore 315 parts of oxysulphuretted muriatic acid, we put them into a tubulated retort communicating with a glass tube, within which was carbon obtained from the action of pure sulphuric acid on pure alcohol. This carbon was during an hour exposed to the most violent heat, and rapidly introduced into the tube (which passed through a small furnace, and after having two bends was united to a Woulf's apparatus). We commenced by rarefying the air within the vessels by means of a strong heat applied to the whole of their surface, and afterwards we heated the tube. But owing, perhaps, to the badness of the lute which we employed, we found this experiment extremely difficult to conduct to a fortunate result; as too great a heat melted the glass, while a temperature a little inferior left the experiment incomplete. A porcelain tube, had we been able to procure it, might perhaps have obviated this difficulty, and prevented the necessity of repeating

peating the same experiment several times before attaining the desired exactness. When the apparatus had attained the temperature of 35° of Wedgwood, we heated the acid in the retort, making it volatile, and keeping the furnace at a uniform temperature. A very great quantity of gas was evolved, which traversing the pure water in the connected vessels, the lime water in the globular glass, and afterwards a column of the same within a cylinder in order to wash it well, it was finally collected in the bell-glass. The experiment finished, we found the contents in examining every part of the apparatus and in analysing the products.

There was no residue in the retort which contained the oxysulphuretted muriatic acid, except a few drops of it, which were owing to a portion of vapour that had not passed into the apparatus, and which condensing by the cold had resumed the fluid state. These weighed five parts: Hence of 315 parts, only 310 passed in vapour over the red-hot carbon. In the space of the anterior tube with carbon there was a little sulphur regenerated and attached to its sides of a heavy red colour, and pasty-like wax, not acid by itself, and in that state which Berthollet called oxidated sulphur; in the posterior part of the tube with carbon there was also sulphur, but in a very small quantity and of a natural colour. This sulphur, besides the former, weighed 16 parts: Hence only 84 parts experienced the joint action of the heat and carbon, from which also should be deducted the proportional quantity of sulphur contained in the five parts of oxysulphuretted muriatic acid remaining in the retort. This sulphur, however, found in the posterior part of the tube with the carbon we considered as left by the sulphuretted hydrogen gas (evolved as we might say) by some peculiar decomposition. In the bends of the tubes which contained water, we found a large drop of a fluid which evinced many characters of hydrogenated sulphur*, called by Lampadius alcohol of sulphur. Its volatility

* The following is the experiment alluded to. He took 315 parts of sulphuretted muriatic acid with nitric acid prepared by synthesis, and with 160 parts of this acid converted into sulphuric acid all the sulphur contained in that substance, which was 100 parts: therefore 100 parts of sulphur to be converted into sulphuric acid require 133 parts oxygen; of which only 97 parts were furnished by the 160 of nitric acid. Now the 36 parts deficient are nearly found by the ancient theory, which considers 315 parts of sulphuretted muriatic acid as consisting of 100 sulphur, 181 muriatic acid, and 34 oxygen: Hence he infers that chlorine contains oxygen.

The author follows the opinion of those chemists who believe that alcohol of sulphur is composed of sulphur and hydrogen: but Clement and Desormes with others maintain that it consists of carbon and sulphur.—*Note of the Pavia Editors.*

appeared to be still greater than that which is commonly prepared; and perhaps this fluid differs something from it in other particulars, as we shall see. The limewater was excessively turbid, and proved to contain much calcareous carbonate and hydrosulphuret of lime. It passed to the state of an elastic fluid merely from hydrogen gas slightly sulphuretted. The formation of calcareous carbonate should be sufficient to confirm us in our mode of thinking respecting the presence of oxygen in oxysulphuretted muriatic acid; and in fact without it carbonic acid gas could not be formed, and in consequence we should not have obtained calcareous carbonate: but as this phænomenon was combined with others of much importance, we were thus led to make new researches, which produced the following results.

At first there appeared a quantity of calcareous carbonate, such as to contain so much carbonic acid which could not be supposed to proceed solely from the 34 parts of oxygen that seemed to form a part of the oxysulphuretted muriatic acid: besides, not having found the smallest quantity of the 181 parts of muriatic acid, except a slight indication in the anterior part of the tube, where the carbon was mixed with the sulphur, hence arose a doubt that the muriatic acid itself was decomposed, and that it had administered fresh oxygen. The carbon was diminished in weight, become of a surprising blackness, had an insupportable odour of sulphureous acid, and could not be inflamed if not previously exposed to a vivid heat: mean time by this process it was burnt to the surface of the sulphur, its bad smell had vanished, and it was thus become sufficiently combustible to consume entirely without leaving almost any residue, being probably converted by means of atmospheric oxygen into carbonic acid gas. In close vessels exposed to a heat much more intense than that which it experienced in the glass tube, it did not lose its strong smell, if not previously mixed with new and pure carbon, in which case some sulphur was sublimed. The sulphur which was deposited on the sides of the tube behind the carbon, and whose physical characters we have already described, being placed by us in a glass subliming vessel, left in the act of sublimation traces of carbon sufficiently sensible to be scraped, and from which we obtained a blackish powder extremely fine. It immediately occurred to us that this carbon mixt with sulphur proceeded from a reciprocal decomposition of a little carburetted over a greater portion of sulphuretted hydrogen gas; but as it presented an extraordinary brightness and combustibility, shining like iron filings and inflaming by the discharge of a small Leyden phial, we determined to combine it with superoxygenated muriate of potash to produce detonation. We were much surprised in this

this operation by the unexpected fulmination which happened, while we exercised on these two substances but a slight trituration to combine them intimately, a trituration effected with two fingers holding the mixture in the folds of a thin dry paper. The noise of the explosion was very great, and such as not to be reproduced by four times the quantity of the same salt mixed in any proportion with different combustible bodies. All the characters of this powder, its unequivocal odour of sulphureous acid, whereas previously it was entirely inodorous, made it be considered as a compound of sulphur and carbon combined together in unknown proportions, and perhaps both modified in their chemical properties. The large drops of hydrogenated sulphur which we found in the cavity of the apparatus, and which was as limpid as water, did not appear to contain an atom of carbon or of sulphur in the mixture. We divided it into three portions, which we treated as follows: the first was inflamed by the electric spark, burnt with a blue flame developing white vapours with a sulphureous smell, and leaving no residuum; the second portion, introduced into a glass globe of large capacity containing oxygenated muriatic acid gas very dry, became pale yellow, and terminated by leaving simple sulphur and destroying the colour of the gas. We then examined the atmosphere in the balloon glass, and found it to be pure muriatic acid gas. This fact, noticed by other chemists, has not been well observed or duly considered; and to explain this phænomenon with greater exactness, we employed, for the same experiment, the third portion of hydrogenated sulphur which remained. Instead of a simple spherical glass vessel, as in the preceding experiments, we used one elliptical and doubly tubulated. The inferior neck or tubulature of this vessel was closed by a steel stopcock, to which was attached a curved glass siphon, in which mercury was included, to show by its height in the one or other arm, the increase or diminution of the atmosphere in the glass. The other tubulature was armed with a steel joint, through which was passed a male screw of the same metal (all the steel parts were covered with a hard varnish to prevent the action of the oxygenated muriatic acid), at the extremity of which, introduced into the inside of the vessel, was a platina wire, to which was suspended a little platina cup. The inner rim was covered with skin, in order that the hydrogenated sulphur which was placed in the cup should not come in contact with the oxygenated muriatic acid, but at the pleasure of the operator, which was effected by means of a screw and the edges of the cup being brought in communication with it. The whole being thus disposed, by means of a strong pneumatic machine the atmospheric air was confined within the receiver, and afterwards we substituted very dry oxygenated muriatic acid gas. The apparatus was again emptied, and filled with the acid gas until we concluded that it could contain nothing but oxygenated muriatic acid gas; when we observed the temperature of the internal air by means of a thermometer which was immersed in it. Adapting the siphon to the stopcock, we observed the stationary point of the mercury in its two arms. Lowering the platina cup containing the hydrogenated sulphur by means of the screw, we noticed in an hour the following appearances, having the stopcock shut. The temperature of the inside of the vessel rather increased, and afterwards returned to its original state. The gas confined in the vessel had no colour, although at the commencement of the experiment it was greenish yellow. The hydrogenated sulphur from a fluid became a little pasty, from a perfect transparency to a somewhat milky appearance. The stopcock being opened, the mercury in the siphon evinced an increase of volume in the air of the receiver, the barometer indicating no diminution of pressure. After another hour we found the same things, only the increase of volume of the internal air was greater. At the end of a third hour we observed no change. We then resolved to try a higher temperature in the balloon glass, exposing it for a few moments to a bar of iron heated, but not shining. With this heat the paste in which the hydrogenated sulphur had been converted, became dry, and the sulphur remained in the cup in a white powder; some of this powder was sublimed on many parts of the sides of the vessel; this happened without the mass being melted, and it is evident that it was a sublimate from undecomposed hydrogenated sulphur, which abandoning its hydrogen had left this powder adhering to the sides. We left all to repose a little, and when it resumed its original temperature we observed a new increase of air. At this period we introduced a small quantity of oxygenated muriatic acid gas, the driest and purest possible. This was effected by means of a bladder, which threw into the apparatus oxygenated muriatic acid gas without permitting the condensed air to escape. After a few minutes the stopcock was opened, to see if there was any increase in the air, as it seemed there should be in proportion to the quantity of gas added; but instead of this we found a diminution, and the temperature of the gas as much raised: restored to its original degree, we added some new oxygenated muriatic acid gas, and after a certain repose found a diminution of air, an increase of temperature, and the white powder of sulphur had assumed its ordinary yellow colour. After a little repose we added more oxygenated muriatic gas, but we then observed an increase of the air and no change of colour.

We deemed it superfluous to continue longer these experiments,

ments, and resolved to examine the different residuums in the operations. We found that the weight of the sulphur corresponded to that of the hydrogenated sulphur employed, and that the mixt gas was composed of simple muriatic acid gas 92, and hydrogen gas 8. From these facts it seems just to conclude that the hydrogenated sulphur absorbed with avidity the oxygen, throwing off a little of its hydrogen; and such was its tendency to combine with the oxygen that it decomposed the oxygenated muriatic acid gas, taking from it the oxygen with which it was charged, and reducing it to simple muriatic acid gas. It appears therefore that common sulphur contains a dose of oxygen, as in our process sulphur of a white colour was obtained, which was coloured yellow in proportion as it absorbed oxygen from the oxygenated muriatic acid, after which it displayed all the characters and properties of common sulphur. It also contains hydrogen, according to the experiments of Berthollet; and after passing over the red-hot carbon, in such circumstances it furnished the inflammable liquor known by the name of hydrogenated sulphur: it appears that it only gave out a portion of its oxygen, which constituted perhaps the carbonic acid gas with carbon surcharged with hydrogen, of which charcoal is a copious source. Hence we could only form a theoretic hypothesis, since the principal object of our labours was not to obtain hydrogenated sulphur; and consequently we could not determine with certainty if, from the copious quantity of carbonic acid gas obtained by the action of the oxygen of the oxysulphuretted muriatic acid, some atom might not be involved in it, or might proceed from the oxygen in the pre-existent sulphur. If therefore any one should think it a principal object to repeat and extend the experiments of Lampadius, Robiquet, Vauquelin and Berthollet on this substance, it would be perhaps the most useful thing to observe accurately, by analysis, if sulphur naturally contains a dose of oxygen, or if it is apt to combine with oxygen after being modified by the action of carbon, and perhaps by that of muriatic acid, as we have seen by synthesis; and in case that this should be the fact, to determine if this reobtained sulphur had any property not common to natural sulphur; or if at least it were not at the same time observed under the aspect of a white powder. Whoever will reflect that, although hydrogenated, sulphur absorbs a portion of oxygen, it does not appear that it could deprive the oxygenated muriatic acid gas of its superabundant oxygen, reducing it to simple muriatic acid gas, at least not without using a strong dose of hydrogenated sulphur, always supposing 85 parts of muriatic acid gas to be surcharged with 15 of oxygen to constitute oxygenated muriatic acid gas. But if the facts which we have announced should throw any doubt

 in

on the theory of chlorine, we cannot, hence, unequivocally reassume the Lavoisierian doctrine in all its extensions, although it may be fair to institute the following deductions.

The hydrogenated sulphur changed the oxygenated muriatic acid into simple muriatic acid, giving out hydrogen, and absorbing a portion of its acidifying principle. Now this seems to be oxygen, from its property of producing sulphuric, phosphoric and carbonic acid, facts which we have already cited. The quantity of oxygen absorbed corresponds to the quantity of hydrogen emitted: now the hydrogenated sulphur absorbed from 100 parts of oxygenated muriatic acid gas eight parts of oxygen; there remain therefore combined with the 85 parts of muriatic acid gas only seven of surplus oxygen, and the properties of the acid gas are scarcely those of simple muriatic and not oxygenated muriatic: hence we conclude, that the muriatic acid gas may be surcharged with oxygen to a certain point, without evincing the properties of oxygenated muriatic acid gas; that the ultimate portions of oxygen under the circumstances in which they exist, determine its true formation, and thus subtracting its properties restore it to the state of simple muriatic acid gas. This reasoning is not entirely void of probability, as in like manner it is known that sulphureous acid has divers quantities of oxygen without altering its properties; so also nitrous acid, the ultimate portions of oxygen uniting and constituting it sulphuric or nitric acid: Hence may we not suspect that muriatic is a simple gradation of oxygenated muriatic acid? I know that the modern doctrine opposes this mode of reasoning, and considers these bodies as in a state of mixture and not in divers degrees of oxygenation; but this does not appear to me to be yet well ascertained; and if it were, it is easy to apply it to our case.

The water which lodged over the bubbles of hydrogenated sulphur in the cavities of the apparatus, and in which the gas was washed, was milky, and contained sulphuretted hydrogen and carbonic acid gases. The limewater in the balloon glass was converted into a hydrosulphuret of lime, and calcareous carbonate was formed, which, after being washed and dried, gave lime 114, carbonic acid gas 84, water 2 = 200. The carbonic acid gas consisted of 51·4 oxygen, 19 carbon, 10·6 water = 84. Here we find a quantity of oxygen equal to 17·4 more than what should really be found if the carbonic acid gas was all formed at the expense of the 34 parts of oxygen which accompanied the sulphur submitted to experiment, recollecting that the sulphuretted muriatic acid was at 10° of Reaumur (55° Fah.) under the barometric pressure of 27 inches eight lines (French), specific gravity 1·7, and composed of 100 parts of sulphur, 181 muriatic acid, 34 oxygen, = 315 parts. Neither the extra 17·4 parts

parts of oxygen which were found, can be attributed to the sulphur, in which I have intimated a doubt of the natural existence of oxygen as not being yet directly proved, and as not being capable of existing in a proportion exceeding 17 per cent., nor to the carbon, as it was not observed to be developed by Lampadius or Berthollet; nor from the atmospheric air, which was expelled previous to commencing the experiment; and even had it not been so, the whole capacity of the apparatus was not such as to contain so much oxygen, combined in the proportion in which it exists in the atmosphere. Recalling to mind the diminution of the 181 parts of muriatic acid, shall it not be permitted to doubt that the oxygen proceeded from some decomposition of this substance, of which the other elements being unobserved, their nature is unknown? Who would again offer the hypothesis that muriatic acid may be composed of hydrogen and oxygen, and that it differs from water only by the different proportions of its constituent parts? If it were permitted to think thus, we should doubt that in decomposing it might have yielded water, the formation of which we could not perceive, not having first weighed the water in the vessel, and some free oxygen that combining with the carbon had produced the extra carbonic acid gas, of which we have already spoken. We recommend to chemists the facts here stated, while we request their indulgence with respect to the above hypothesis; declaring that we are not fanatical framers of new systems, but only desirous of accumulating facts from which the most ingenious of the age may deduce new theories*.

LXIV. *On*

* *Observation by the Translator.*—Marquis Ridolfi is one of the very few Florentine nobles who devote their attention to the sciences; and hence, perhaps, we may discover more causes than reasons for some of the opinions contained in this memoir. It is evident that the author began his researches not so much like an inquirer after truth, as with the view of confirming some preconceived opinions which analytical experiments have proved, if not altogether erroneous, at least of a very equivocal character. To this sentiment must be ascribed such a series of facts and deductions founded on a single experiment with phosphorus, although the labours of Professor Branchi were surely sufficient to prove how little dependence should be placed on our actual knowledge of this body and its relations with others. Professor Brugnatelli is evidently of an opinion very different from that of the author; and in a comprehensive exposition of Berthollet's candid statement of his conversion to Sir H. Davy's theory of chlorine, clearly shows that nearly all the facts and circumstances which Ridolfi adduces, may be explained without admitting the unproved assertion of its containing oxygen. If indeed the Marquis Ridolfi's position be just,—that it cannot be proved by analysis to be a compound body,—no argument, *à priori*, should be admitted to the contrary from synthesis:—if it cannot be decomposed, all our knowledge and logical reasoning sanction the induction that it is simple.

LXIV. *On the Kaleidoscope.*

This amusement being now in the hands of almost every person, any description, more particular than what will present itself in the subjoined historical detail, will here be unnecessary.

Dr. Brewster, the patentee of this amusing instrument, is charged by many with being a plagiarist, and claiming that, as a new invention of his own, which is really old, and the discovery of another. We shall lay the grounds of this charge before our readers;—and we begin with some remarks which have appeared in the French Journals:

"Scarcely," says one of them, "had the Kaleidoscope been imported into Paris, when twenty competitors started forward, and each, his glass in his hand, contended for the attention of the public. To the *Kaleidoscope* one opposed the *Polyoscope;* another the *Metamorphosiscope;* and as the great majority of spectators called out for something French, we saw immediately this wish gratified by the *Transfigurateur*, the *French* lamp, &c."

"M. Robertson," a mathematical-instrument maker in Paris, of some eminence, "reclaims for France the priority of this invention. He brings in proof an instrument, of great dimension it is true, but which for many years has furnished in his cabinet the same various pictures which an adroit speculator has introduced into the Kaleidoscope. Thus the Professor Brewster of Edinburgh, to whom the English have attributed the honour of this discovery, is nothing more than an imitator. This is not the first time that a French discovery has taken the longest way of arriving at Paris. M. Chevalier too enters the lists; holding in one hand a work, *published more than fifty years ago*, in which the principle of this agreeable illusion is described, while in the other he presents us a lamp which, by adding much to the magic of the effects, merits truly the name which he gives it of the *French Multiplicator*."

However mortifying it may be to our ingenious neighbours, the French, to have their claims to the originality of this invention denied, the fact is, that, should the optical principle on

ple. But it requires little experience to know how fallacious and inadequate are all synthetical experiments; and as in their nature they must be founded on preconceived notions, they can never be of value, but only where they tend to confirm the accuracy of analysis, as in the decomposition and recomposition of water. But if the facts here adduced prove any thing, it is rather that chlorine and hydrogen produce muriatic acid, instead of the latter becoming chlorine by the addition of oxygen. This is not altogether improbable, nor incompatible with the reputed discovery of Lampadius, who supposes that muriatic acid is composed of hydrogen and oxygen united to an inflammable base.—*Rome,* Feb. 1818.

which

which the instrument is founded, and earlier publication, be held to constitute the invention, the discovery will be found to belong to England, notwithstanding the French work "published *more than fifty years ago*, in which the principle of this agreeable illusion is described;" for the principle was published in London more than eighty years ago, in a work entitled "*New Improvements of Planting and Gardening, both philosophical and practical, 6th Edition. By* RICHARD BRADLEY, *Professor of Botany at the University of Cambridge, and F.R.S. Printed for J. and J. Knapton, in St. Paul's Church-yard,* 1731." The following is printed from Bradley's first chapter.

"*Description and Use of a new Invention for the more speedily designing of Garden Plats, whereby we may produce more variety of Figures in an Hour's Time, than are to be found in all the Books on Gardening now extant.*

"Since the instrument I now design to treat of has afforded some pleasure to many of my acquaintance, I have been easily persuaded to make it public. It is of that *nature*, that the best designers or draughtsmen may improve and help their fancies by it, and may with more certainty hit the humour of those gentlemen they are to work for, without being at the trouble of making many varieties of figures or garden plats, which will lose time and call an unnecessary expense, which frequently discourages gentlemen from making up their gardens. In short, the charge of the instrument is so small, and its use so delightful and profitable, that I doubt not its favourable reception in the world. But to proceed:

"We must choose two pieces of looking-glass of equal bigness, of the figure of a long square, five inches in length and four in breadth: they must be covered on the back with paper or silk, to prevent rubbing off the silver, which would else be apt to crack off by frequent use. This covering for the back of the glasses must be so put on that nothing of it may appear about the edges of the bright side.

"The glasses being thus prepared, they must be laid face to face and hinged together, so that they may be made to open and shut at pleasure, like the leaves of a book. As for example, the first figure (Pl. III.) shows us the backs of the two glasses A and B joined together by hinges C C and D D, so that they may be opened and shut to any part of a circle. And now the glasses being thus fitted for our purpose, I shall proceed to explain the use of them.

"Draw a large circle upon paper, divide it into 3, 4, 5, 6, 7 or 8 equal parts, which being done, we may draw in every one of the divisions a figure at our pleasure, either for garden plats, or or fortifications. As for example; in the second figure, we see a circle divided into six parts, and upon the division marked A is drawn part of a design for a garden. Now to see that design entire, which is yet confused, we must place our glasses upon the paper, and open them to the sixth part of the circle, *i. e.*, one of them must stand upon line *b* to the centre, and the other must be opened exactly to the point *c*; so shall we discover an entire garden plat in a circular form (if we look into the glasses) divided into six parts, with as many walks leading to the centre, where we shall find a basis of an hexagonal figure.

"We may more plainly see how the glasses ought to be placed upon the design by viewing the third figure. The line A, where the glasses join, stands immediately over the centre of the circle: the glass B stands upon the line drawn from the centre to the point C, and the glass D stands upon the line leading from the centre to the point E. The glasses being thus placed, cannot fail to produce the compleat figure we look for: and so whatever equal part of a circle you mark out, let the line A stand always upon the centre, and open your glasses to the division you have made with your compasses. If instead of a circle you would have the figure of an hexagon, draw the straight line with a pen from the point *c* to the point *d* in the second figure; and by placing the glasses as before, you will have the figure desired.

"So likewise a pentagon may be perfectly represented by finding the fifth part of a circle, and placing the glasses upon the outlines of it, and the fourth part of a circle will likewise produce a square by means of the glasses, or, by the same rule, will give us any figure of equal sides. I easily suppose that a curious person by a little practice with these glasses may make many improvements with them, which perhaps I may not yet have discovered, or have for brevity's sake omitted to describe.

"It next follows that I explain how by these glasses we may, from the figure of a circle drawn upon paper, make an oval; and also by the same rule, represent a long square, from a perfect square. To do this, open the glasses and fix them to an exact square: place them over a circle, and move them to and fro till you see the representation of the oval figure you like best; and so having the glasses fixed, in like manner move them over a square piece of work, till you find the figure you desire of a long square. In these trials you will meet with many varieties of designs. As for instance, the fourth figure, although it seems to contain but a confused representation, may be varied into above 200 different representations by moving the glasses over it, which are opened and fixed to an exact square. In a word, from the most trifling designs, we may by this means produce some thousands of good draughts.

"But

"But that the fourth figure may yet be more intelligible and useful, I have drawn on every side of it a scale divided into equal parts, by which means we may ascertain the just proportion of any design we shall meet with in it.

"I have also marked every side of the fourth figure with a letter, as A B C D, the better to inform the reader of the use of the invention, and put him in the way to find out every design contained in that figure.

"*Exp.* 1. Turn the side A to any certain point, either to the north or to the window of your room, and when you have opened your glasses to an exact square, set one of them on the line of the side D, and the other on the line of the side C; you will then have a square figure four times as big as the engraved design in the plate; but if that representation should not be agreeable, move the glass (still open to a square) to the No. 5 of the side D, so will one of them be parallel to D, and the other stand upon the line of the side C: your first design will then be varied; and so by moving your glasses in like manner from point to point, the draughts will differ every variation of the glasses, till you have discovered at least fifty plans differing from one another.

"*Exp.* 2. Turn the side marked B of the fourth figure to the same point where A was before, and by moving your glasses as you did in former example, you will discover as great a variety of designs as had been observed in the foregoing experiment: then turn the side C to the place of B, and managing the glasses in the manner I have directed in first example, you may have a great variety of different plans which were not in the former trials; and the fourth, D, must be managed in the same manner with the others; so that from one plan alone, not exceeding the bigness of a man's hand, we may vary the figure at least 200 times; and so consequently from five figures of the like nature we might show about 1000 different sorts of garden plats; and if it should happen that the reader has any number of plans for parterres or wilderness works by him, he may by this method alter them at his pleasure, and produce such innumerable varieties that it is not possible the most able designer could ever have contrived.

"And seeing I have given such directions as I hope may inform the curious of the use of this new invented instrument, I think it may not be improper to advertise that the publisher of these papers is provided with glasses of several sizes ready fitted up for the experiment, at the following prices: the smaller sort at 3*s*. and the other at 5*s*."

In the foregoing description of Bradley's invention, the principle of reflection on which he constructs it, is precisely that which

which Dr. Brewster has employed in his Kaleidoscope; but the means by which the latter presents to the reflecting surfaces the objects that are to be reflected, are quite different. Even with Bradley the kind of objects and the means by which he presented these objects to the mirrors were what constituted his instrument a new invention; for the arrangement of the reflectors themselves was not of Bradley's discovering, as we shall prove immedialely.

We copy the following from *John Baptista Porta's Natural Magic*, the English Translation published in 1658.

"*To make a plain Glass that shall represent the Image manifold.*

"A glass is made that will make many representations, that is, that many things may be seen at once; for by opening and shutting it, you shall see twenty fingers for one, and more. You shall make it thus: Raise two brass looking-glasses [metallic mirrors], or of crystal, at right angles upon the same basis, and let them be in a proportion called sesquialtera, that is one and a half, or some other proportion, and let them be joined together longways, *that they may be shut and opend, like a book*; and the angles be divers, such as are made at Venice: For one face being objected you shall see many in them both, and this by so much the straighter, as you put them together, and the angles are less: but they will be diminished by opening them, and the angles being more obtuse, you shall see the fewer: so showing one figure, there will be more seen: and further, the right parts will show right, and the left to be left, which is contrary to looking-glasses; and this is done by mutual reflection and pulsation, whence ariseth the variety of images interchangeable."

From the foregoing it is manifest whence Bradley derived the principle which he applied to the construction of his instrument, for he borrows the very words of Porta, "*that they* (the mirrors) *may be shut and opened like a book*;" and hence it follows that if the *discovery of the principle* cannot be allowed to the French, so neither can it to the English: for Porta's work was first published (at Naples we believe) in 1538, in *four books*, and 35 years after (that is about the year 1573), in its enlarged form, comprising *twenty books*. Bradley was not called a plagiarist,—probably because his instrument, though identically the same as Porta's, was applied in a different way and to a different purpose. Should Dr. Brewster then be considered in that light, for having made use of the same principle in his instrument, which in construction is different from either Porta's or Bradley's? Porta, by looking at objects before him, along the angle formed at the joining of his glasses, saw them multiplied; Bradley, by by placing his joined glasses upon his drawings, at right angles to them, and looking at them, in the same manner, saw them multiplied; but the number of reflections could be calculated. Dr. Brewster, by putting the reflectors in a tube, and attaching thereto, and at right angles to them, two discs of glass with objects interposed, forms an optical instrument capable of producing an incalculable (if not an infinite) number of combinations, by merely making the discs, or the whole instrument, to revolve on its axis, while the eye looks through it. If the previous application of any known principle to the construction of instruments, is to be considered and held as embracing all future applications of the same principle, there can be no new inventions; for to obtain knowledge of a principle, not before known, is *a discovery*, and not an *invention:* no person can invent a principle; but he may apply a principle, when known, to a new purpose, and this new application with the new means employed, is what constitutes a new invention. T.

LXV. *Notices respecting New Books.*

A Treatise on the General Principles of Chemical Analyses. Translated from the French of L. J. THENARD, *Member of the Institute of France, Professor of Chemistry, &c. &c. By* ARNOLD MERRICK. 8vo. 346 pages.

IN the original this Treatise forms the concluding volume of Thenard's Chemistry, published in 1816. Some slight alterations and many additions, consisting chiefly of extracts from the other volumes, have been made by the translator, to render the work more complete as a treatise. The translator states that possessing, as we do, the excellent works of Dalton, Davy, Henry, Murray, and Thomson, a translation of the whole of Thenard's Elementary and Practical Treatise on Chemistry was quite unnecessary; "but as we have no separate and convenient work in English on chemical analyses, (the Essays of Bergman and Kirwan having been long out of print,) it has been judged that a translation of Thenard's treatise on that subject would be a valuable acquisition to the practical chemist. It is hoped that the present translation will be found sufficiently perspicuous, faithful and concise," and the translator modestly adds that "it pretends to no other kind of merit."

We consider the work before us as a valuable acquisition to the practical chemist. Its nature will be better understood from a summary of the contents than from an extract.

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Della

Della Longitudine, e Latitudine, delle Città di Pistoja e Prato, &c. Della Longitudine, e Latitudine, Geografica delle Città di Volterra, S. Miniato, e Fiesole, Memoria, &c.

"On the Latitude and Longitude, &c. of the Cities of Pistoja, Prato, Volterra, St. Miniato, and Fiesole. By Father INGHIRAMI, Public Professor of Astronomy in the Ximenian Institute, and of the higher Mathematics in the College of the *Scuole Pie* of Florence. 1817."

The former of these Memoirs is printed in the Transactions of the Royal Academy of Pistoja, and is the first account of the trigonometrical Survey of Tuscany now executing by the author; the latter is printed in a distinct tract. Those who believed every thing promulgated by Buonaparte, perhaps expected that such an undertaking originated with him, but the contrary is the fact; and so far from encouraging it, he absolutely prevented Baron Zach from proceeding in his experiments in 1808. It is true he made a road over a part of the Alps; but if he did, it was merely to facilitate the passage of the plunder of Italy to Paris. It was reserved for the paternal government of Ferdinand III. and a professor of religion, not of infidelity, to execute this arduous part of national geography: and the unassuming manner as well as judicious accuracy with which it is commenced, is the best answer which could be given to the French reproach of ignorance and incapacity in the natives of Tuscany. The instruments used by Professor Inghirami were constructed by Baron Reichembach of Munich, and consist of a theodolite or repeating circle, telescope, &c. which he has found singularly accurate and perfect. The greatest triangle which the author has hitherto been able to observe and measure in a country covered with mountains is about 40,000 French toises; as he repeats his observations always nine or ten times, with the difference very rarely of two seconds in each observation, he justly concludes that it is impossible an error greater than 18″ could occur; but in by far the greater number of instances he has found,—thanks either to the extreme accuracy of his instruments or to his own skill in observing,—an almost total identity. Thus, in the angle from Pietra Marina to Volterra he found only 27 hundreds of a toise in 10,000 toises, and in that from St. Miniato to Volterra only 26 hundreds of a toise; in other instances much less. But in a brief analysis it is impossible to convey any adequate idea of the author's labours in this interesting branch of trigonometry. The latitude of Florence at the Observatory over the Museum on the left bank of the Arno, is calculated at 43° 46′, 4·6″ north, and the longitude from the Island of Ferro 28° 55′ 2·4″. The author's first station was on the Cocollo, one of the

the ridge of mountains which bound the province of Carsentino, and separate Tuscany from the Ecclesiastical States on the north-east; it is situated S.E. by E. of Florence 57° 58′ 9″*, and from thence he carried his triangles to Monte Serario, north, to Pistoja, north-west, to St. Miniato, W. by S., and finally to Volterra, which is S.W. of Florence. The latitude of Fiesole, at the bell-tower of the cathedral, he found to be 43° 48′ 38·7″, and longitude 28° 57′ 45·9″, which is 2432·98 French toises distant from Florence: latitude of the city of Prato at the College Cicognini 43° 52′ 56·6″; longitude 28° 45′ 49·8; latitude of the city of Pistoja at the episcopal palace 43° 56′ 4·6″; longitude 28° 34 48·7″. Latitude of St. Miniato, south angle of the Tower 43° 41′ 3·8″; longitude 28° 31′ 21·9″, and distant from Florence 22·94 Tuscan miles of 68 to a degree. Latitude of Volterra at the Mastio Tower 43° 24′ 13·6″; longitude 28° 31′ 59·3″, and distant from Florence 31·59 Tuscan miles. The author's topographical description of his different stations, and the extensive views which they afforded of a beautiful country, will be found useful to future geographers and topographical travellers; but an abridgement of them would not be intelligible. Professor T. notices the discrepancies between the results of the English and French trigonometrical surveyors, and observes with more candour and no less judgement than some other geometers, that as the English surveyors have found it necessary to change their basis, "if we suppose an equal necessity for change and correction in the base of the French triangles, we might perhaps obtain an approximation in the results of the two operations, where all the diversity should not be attributed solely to the observations." The author, doubtless from his experience, well knows how much of this disparity may be attributed to a certain kind of dexterity in the production of plausible and uniform results, for which the French are not a little celebrated, as well as for address in overlooking certain difficulties which more candid and philosophical minds not only confess, but seek to trace to their cause. He seems to think that one of the causes or sources of error in the observation of angles may be ascribed to the lateral reflection of the atmosphere; but he dissents from the opinion of Dr. Brewster, that they are owing to some defect in the eye of the observer. On this head, as well as in the whole series of his observations, there are such evident traces of candour, judiciousness, and accuracy, that some person interested in such inquiries

* With this calculation the Professor favoured the Translator, it not being necessary to his plan to specify the different bearings of his stations from Florence.

will no doubt transfer them into English. For the reduction of his angles the Professor prefers the following formula:

"$C = O + \frac{r \sin.(O+y)}{D \sin 1''} - \frac{r \sin. y}{G \sin. 1''}$, where C is the deduced angle, O the one observed, r the distance between the place of observation and that of reduction; D and G the approximate distance between the two objects observed, the one on the right the other on the left, and between which is comprised the angle O; lastly, y is the angle of direction, that is the angle of the object on the left with the place of reduction." With this formula he reduces all his angles, and found between Corollo his first station and Pistoja 33916·23 French toises. From a memoir read to the Society of *Georgofili*, on the 11th December, in Florence, it appears that the Tuscan government has ordered the immediate completion of this trigonometrical survey of Tuscany, which is to be followed by the publication of a new and accurate map of the Grandukedom, and many other improvements in the agriculture, irrigation, management of the waters, &c. in that most interesting province of Italy. At the same time a mineral, geological, and botanical survey of that country is executing by different naturalists of known talents and knowledge.

An Essay on the Origin and Operation of the Dry Rot, with a View to its Prevention or Cure; to which are annexed Suggestions on the Cultivation of Forest Trees, and an Abstract of the several Forest Laws from the Reign of Canute to the present Time. By ROBERT M'WILLIAM, *Architect and Surveyor.* 4to. pp. 420.

The disease which it is the object of this work to aid in preventing or curing, has of late become familiar, in its baneful effects, to all who are conversant with building. It has not only become more general than it ever was in former times, but in this country its ravages have increased beyond all proportion to what has taken place in other parts of Europe. It has been reckoned that the annual expense occasioned by the destruction of timber, and the loss of labour in the necessary repairs, has been to government not less than from two to three, and to the whole nation from four to five millions sterling.

It is not surprising that under such circumstances the subject should have attracted very general attention, and that many remedies should have been proposed; but we agree with the ingenious author of the Essay before us, that even those who have gone furthest into the matter have merely contented themselves with having detected the proximate cause, without endeavouring

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to trace those remoter circumstances in which the disease originates.

In thehope of supplying this deficiency, Mr. M'William has attempted to trace the disease to its remotest source, and investigate all the causes that may co-operate in bringing it to maturity; whence he deduces the means of preventing its attack, arresting its progress, and remedying its effects.

The author in following out these objects has produced a volume of considerable bulk; which will be found to contain a great deal of highly interesting and important matter, conveyed in distinct and perspicuous language. In the extract from it, which we gave in our last number, on the *Fir Tree*, our readers have been already presented with a specimen of the work, to which we beg to refer them for ample confirmation of its claims on their attention.

Theoretisch, practische Wasser, baukunst von C. T. von Wiebeking. *Munchen* 1809–1817: — "Hydraulic Architecture theoretical and practical, by C. F. de WIEBEKING, Privy Counsellor to H. M. the King of Bavaria, 4 vols. 4to, with upwards of 150 large folio plans and maps. Munich 1809–1817.

The work on Hydraulic Architecture, by the Chevalier de Wiebeking, in four quarto volumes, is generally considered as one of the most complete and extensive treatises on this interesting science. The fourth and last volume, which was published in the course of last year, and has only lately reached this country, contains much highly interesting and valuable information respecting all the great works of Hydraulic Architecture, executed in different parts of Europe, developing their principles, mode of execution, advantages and defects; and suggesting useful ideas for their further improvement and conservation. Thus it serves to the student, as well as to the experienced engineer, as a never-failing assistant, and as a valuable book of reference in his pursuits and plans.

The fourth volume chiefly contains supplementary matter to the different divisions of the three former ones, and particularly treats in several chapters on the Inland Navigation of Great Britain, enumerating the different canals in various parts of this country; and of the great bridges that have lately been constructed across the river Thames at London. The Chevalier's observations on these subjects are very judicious, and exhibit much mind.

In speaking of the river Thames, M. de Wiebeking chiefly laments, that no proper means are used to promote the navigation and to preserve the necessary depth of the water in the river, and that thus in the greatest mercantile city of Europe, the finest river is in a state of actual neglect and degradation, the principal cause of which is the irregular width of its bed, which near London Bridge is considerably narrower than at Waterloo Bridge, and again contracts at Westminster Bridge; whereby the tide loses the power of removing the shoals and mud banks, that are daily increasing and diminishing the depth of the water, which at some not very distant period may prove fatal to the navigation of the river.

In order to obviate these inconveniencies, the Chevalier suggests, that the whole bed of the river, from Westminster Bridge down to the Tower, should be reduced to a certain uniform breadth, which he fixes at 550 feet; that London Bridge be removed entirely; and that, in the construction of new bridges, this principle be always adhered to. As to the removal of London Bridge, and the great advantages that would result from this operation to the navigation of the river, all the British engineers are, no doubt, of M. de Wiebeking's opinion; but as to his project, to reduce the width of the river to 550 feet, many may doubt whether, under the present circumstances, such an alteration would be advisable or even practicable, though the reasons which the Chevalier suggests for it are founded on sound principles and corroborated by experience. The immense saving, which a reduced width of the river would have caused in the construction of all the bridges over it, certainly would have been a matter of great consequence; and we may safely assert, that if before the year 1737, or the construction of Westminster Bridge, the public mind had been as enlightened as it is at present on the subject of the navigation and management of rivers, and if an experienced engineer had brought forward a plan similar to that of M. de Wiebeking's, the adoption and execution of it would have been highly beneficial to the navigation and conservancy of the river in general, to the port of London in particular, and to the improvement and embellishment of the city of London.

Instead of the London Docks, the Chevalier de Wiebeking is of opinion, that it would have been more advantageous for the trade and commerce of the city of London, if a solid quay had been constructed from Westminster Bridge down to London Bridge; and if, in the place of one great bason, several small basons had been excavated in different parts of the town, which by means of locks would have communicated with the river. The dimensions of these small basons he fixes to 120 feet breadth, and to 4 or 500 feet length. M. de Wiebeking further proposes

poses to join these small docks together by a canal parallel to the quay, which would produce the advantage of scouring alternatively, as the tide served, every one of these basons by the water contained in the others. At the side of this canal and the docks, warehouses of different descriptions could be constructed, and thus the mercantile depôts would have been nearly central in the metropolis; besides this advantage, the water in the canal would have been of great use in case of fire.

The Chevalier concludes these observations with the following passage (page 190): "According to this plan, the length of the Bridges of Westminster, Waterloo, Blackfriars, and Southwark, would have been considerably lessened, and the saving thus produced would have been more than sufficient for the building of a new bridge in the place of London Bridge; the great expense of the London Docks would have amply covered the costs of the execution of the plan which I have thus sketched in its outlines, if it had been proposed and attended to at the proper time. But at present the only object which is most deserving of the public attention, is the construction of a new bridge in the place of London Bridge, and that of a regular solid quay along the banks of the river, and whether, notwithstanding the excavation of the London Docks, the small shipbasons, which I proposed, would yet be of moment and use for the trade. I therefore leave it to the judgement and to the consideration of the British engineers, to decide whether this part of my plan still deserves to be attended to, and in what manner the principal ideas of it, viz. the construction of solid banks of the Thames, the excavation of small shipbasons, and the project of the new Bridge in place of London Bridge, might best be determined and executed. But before it is possible to enter into the particulars of these projects, it is indispensably necessary to procure more minute soundings of the river, than those that to the best of my knowledge have yet been made."

M. La Beaume has in the press "Observations on the Properties of the Air-Pump Vapour-Bath, pointing out their Efficacy in the Cure of Gout, Rheumatism, Palsy, &c. with cursory Remarks on factitious Airs, and on the improved State of medical Electricity in all its Branches, particularly in that of Galvanism, and their Efficacy in various Diseases."

Mr. Jonathan Otley, an ingenious mechanic, of Keswick in Cumberland, whose intimate acquaintance with the district of the Lakes, and with its curiosities and natural productions, has frequently occasioned him to be selected as *a guide* to visitors;

 to

to those in particular who have travelled therein in search of Mineral information; is about to publish a very improved and neat "Map of all the Lakes in Cumberland, Westmorland and Lancashire;" which, embracing the Coast from Lancaster to Workington, will show the heights of the principal Hills, and many other matters (useful or interesting to persons making the Tour of the Lakes) that have not hitherto been included in any Map. The scale is four miles to an inch, and the size 13 by $10\frac{1}{4}$ inches.

Pharmacopœia Medici Practici Universalis, sistens Medicamenta Præparata et Composita, cum eorum Usu et Dosibus; et Pharmacopœia Chirurgica, sistens Medicamenta Præparata et Composita, Usui externo et Morbis præcipue externis curandis dicata. Auctore F. SWEDIAUR, *M.D. Juxta Auctoris Textum recusa. Editionis Curam gessit, Additamentis locupletavit, et Notis elucidavit* J. B. VAN MONS, *M.D. in Regia Universitate Lovaniensi Professore Publ. &c.* 2 vols. duodecimo.

Medical science is much indebted to M. Van Mons for this new edition of Swediaur's excellent work, which had of late become rather scarce. The notes show a perfect acquaintance with all the other Pharmacopœias of repute, such as the London, Edinburgh, Berlin, Danish, &c. from which many valuable illustrations and corrections have been drawn; and the whole work is edited in a manner which does credit to the well-known ingenuity and diligence of the learned Professor.

Sir Humphry Davy has just published an interesting volume on the Safety Lamp for Coal Miners, with some Researches on Flame.

LXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

MAY 7.—A COMMUNICATION was read from Mr. Bevan, of the result of a series of meteorological observations made at Keswick in Cumberland.

14. No meeting.

21. Read a paper, by Col. W. Lambton, containing a variety of deductions from the calculation of an arc of the meridian taken in India. Read another paper, by Mr. Pond Astronomer Royal, on the best mode of making a Catalogue of the

Fixed

Fixed Stars, in which he points out several imperfections in the plan followed by the late Astronomer Royal, and proposes one of a more definite and comprehensive character.

28. Read a paper, by Mr. Pond, on the Parallax of the Fixed Stars, containing a further application of the principles developed in his former paper on Alpha Aquilæ.

A paper was read at the same meeting by Mr. M. Donovan, on the Oxides and Salts of Mercury.

LXVII. *Intelligence and Miscellaneous Articles.*

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for April 1818, it appears that during that month, the following was the work performed by the engines reported with each bushel of coals.

	Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
23 common engines averaged	22,982,057	various.
Woolf's at Wheal Vor ..	26,064,526	17·2 lib.
Ditto Wh. Abraham ..	32,723,166	16·8
Ditto ditto	23,626,456	5·63
Dalcouth engine	41,888,316	11·2
Wheal Abraham ditto ..	33,934,245	10 9
United Mines engine ..	33,564,503	13·6
Treskirby ditto	41,823,209	10·8
Wheal Chance ditto ..	33,932,578	11·3

NEW EXTRACTS FROM COAL.

Dr. Jassmeyer, Professor of Chemistry in Vienna, has made the discovery of a means to extract from coals two hitherto unknown acids, a resin, a resinous gum, and other elements, which he has employed with surprising success to the purposse of dyeing wool, silk, hair, and linen, and has produced from them red, black, yellow, and various shades of brown and gray. Count Von Chorinsky, President of the Aulic Chamber, and many other enlightened judges of these matters were present at these experiments, and testified their entire approbation of this useful discovery.

LOCUSTS IN INDIA.

About the 20th June 1812, a very large flight of locusts was observed hovering about Etawah, which at length settled in the fields east of the town, where they remained some time, and were seen copulating in vast numbers; they then took their departure, but continued to hover about the place for a month afterwards.

On the 18th of July, while riding in that direction, I discovered a tremendous swarm of very small dark-coloured insects in the vicinity of a large pool of stagnate water; they were collected in heaps, and covered the ground to a considerable distance. These, on minute inspection, proved to be locusts in miniature, but without wings. In this place they remained, hourly increasing in numbers, for some days, when the great body moved off, taking a direction towards the town of Etawah: they crept and hopped along at a slow rate, until they reached the town, where they divided into different bodies, still however keeping nearly the same direction, covering and destroying every thing green in their progress, and distributing themselves all over the neighbourhood. The devastation daily committed by them being almost incalculable, the farmers were under the necessity of collecting as many people as they could, in the vain hope that they might preserve the crop by sweeping the swarm backwards; but as often as they succeeded in repelling them in one quarter, they approached in another: fires were then lighted all round the fields with the same view:—this had the effect of keeping them off for a short time; but sufficient fuel could not be supplied, and the moment the fires became extinguished, the insects rushed in like a torrent. Multitudes were destroyed by the birds, and many more by branches of trees used by the farmers for that purpose, as well as by their being swept into large heaps, and consumed by fire; yet their numbers seemed nothing diminished. They so completely covered some mangoe trees, and the hedges surrounding the gardens, that the colour of the leaves could not be distinguished. They had no wings, and were about the size of small bees. They continued to creep along the ground, or hopped when their progress was interrupted.

July 27.—They were increasing in size, and had overspread that part of the country in every direction. From the want of rain, and the overwhelming inroad of these insects, the farmers were nearly ruined. Nothing impeded their progress; they climbed up the highest trees and scrambled over walls; and notwithstanding the exertions of several people with brooms, the verandah and outer walls of the hospital were completely covered with them. They no longer continued to move in one particular direction, but paraded backwards and forwards, wherever they could find food.

On the 28th of July the rains set in with considerable violence; the locusts took shelter on trees and bushes, devouring every leaf within their reach; none seemed to suffer from the rain.

On

On the 29th it did not rain, and the young swarm again were on the move, continuing their depredations; they were fast increasing in size, and equally lively as before the rain.

It again rained on the 30th, and again the locusts took shelter on the trees and fences; several large flights of locusts passed over the cantonments, and I observed that the wings of the young ones began to appear. The head still retained the dark red colour, but the black lines on the body had become much fainter.

Again on the 31st large flights continued to pass, driven by the wind to the southward; of course very few alighted. They caused little mischief within our view. The wings of the young tribe (the whole four being now formed) were about one-eighth of an inch in length. After this time I made no particular observations on their progress, being otherwise engaged, but they disappeared in a few days*.

A curious case has recently been communicated to Dr. Thomas Forster, of three children successively being born of the same mother, and each of them having five fingers and a thumb on each hand. They are the sons of a labouring man in Buckinghamshire, and the family are in all other respects healthy. The extra finger was situated on the outside of the little finger and on the same bone. It was in all the three cases amputated close to the joint during the first three days of infancy, and there is no apparent defect left on the hand. These sorts of monstrosities frequently are hereditary, and appear again and again in successive generations of the same family. But in the present case, no traces can be found of any thing of the kind having happened in the family before.

BAKERS' BREAD.

A recent investigation into the composition of London bakers' bread, on the part of some chemical gentlemen, has led to the detection of a quantity of alum contained in it, and of some potass; a circumstance which accounts for the constipating effects of this bread on the bowels. Our readers will find a curious account of the adulteration of bread, under the article *Baking*, in the *Supp. to Encyc. Britannica.*

NAUTICAL EPHEMERIS.

To Mr. Tilloch.

Sir,—I would call the attention of your numerous nautical readers

* These extraordinary facts are communicated to us by an intelligent correspondent, who with some of the officers of the 14th Native Infantry was an eye witness.—*Ed. of Calc. Mag.*

readers to two important errors of the equation of time, in the Nautical Ephemeris for 1818, and seven in the one for the year 1819. It becomes of consequence these errors should be known, as during the last year several vessels put into Portsmouth, out of their course, for the correction of their chronometers; which *appearance* of errors arose from the misprint of the Ephemeris.

			min.	sec.			min.	sec.	
1818.									
March	9	for	19	54	0	read	9	54	0
May	8	—	4	43	6	—	3	43	6
1819.									
April	12	—	1	58	0	—	0	58	0
——	13	—	1	41	9	—	0	41	9
——	14	—	1	26	1	—	0	26	1
——	15	—	1	10	6	—	0	10	6
June	5	—	1	2	2	—	2	2	2
——	28	—	1	40	0	—	2	40	0

Dec. 1, the letters "add," the top of the column, page 134, should be "subtract."

I am, sir, your obedient servant,

R. Webster.

DR. HENRY CLARKE.

It is with concern that we record the decease of another eminent mathematician and philosopher, Dr. Henry Clarke, on the 29th of April, at his house at Islington, in his 76th year. His death was occasioned by a fit of apoplexy, with which he was seized on the preceding day. He was fifteen years Professor of History, Geography, and Experimental Philosophy at the Royal Military College, from which he had just retired. He was the author of several mathematical and literary elementary works, and had an extensive knowledge of philosophy, mathematics, and the languages. By the Universty of Edinburgh he was honoured and rewarded with the degree of Doctor of Laws. He was a contributor to many mathematical and periodical journals, and corresponded with most of the eminent men of the last century. He has left behind him a blind aged widow to whom he was married fifty-two years, and a family of two sons and four daughters. His elegant and very choice collection of philosophical instruments, by which he delivered his lectures, and his select library of scientific books will shortly be given to the public by sale at auction.

LIST OF PATENTS FOR NEW INVENTIONS.

To Gilbert Lang and Robert Smith, both printers in Glasgow, for their mode of producing the Swiss new deep and pale

reds

reds by topical mordants and a pale blue discharge in said reds. —11th April 1818.—2 months to enrol specification.

To William Crawshay the younger, of Cyfaithfa Iron Works, in the county of Glamorgan, esq., and David Mushet, of Coleford in the county of Gloucester, iron master, for their improvement for the making and manufacturing of bar or other iron from certain refuse slags or cinders in the smelting of copper ores in the manufacturing of copper.—18th April.—2 months.

To Augustus Applegarth, of Nelson-square, Great Surrey-street, in the county of Surrey, printer, for certain improvements in the art of casting Stereotype or other plates for printing, and in the construction of plates for printing bank or bankers' notes or other printed impressions where difficulty of imitation is a desideratum.—23d April.—2 months.

To Edward Lillie Bridgman, of Goswell-street Road, in the parish of St. Luke, Middlesex, for certain improvements in making coffins, and in machines for conveying coffins for interment, and appendages to the same, in the church and burial-ground.—23d April.—2 months.

To George Tyer, of Homerton, Middlesex, for his chain pump. —2d May.—2 months.

To Joshua Rowe, of Torpoint, Cornwall, for certain improvements or processes applicable to the printing of cotton and other cloths, and to other purposes.—4th May.—6 months.

To Sir Thomas Cochrane, knight, commonly called Lord Cochrane, and Alexander Galloway, of Holborn in the county of Middlesex, engineer, for the working or making a manufacture, being a machine or machines for removing the inconvenience of smoke or gases generated in stoves, furnaces, or fire-places, by the ignition or combustion of coals or other inflammable substances, and in certain cases for directing the heat and applying such smoke or gases to various useful purposes, which will be of great public utility.—4th May.—6 months.

To Thomas Jones and Charles Plimley, both of Birmingham, for their improvement to blast engines and steam engines.—7th May.—2 months.

To William Bush the younger, of Bermondsey, Surrey, for his improvement in the method of frying and preparing of malt, wheat, and other grain.—5th May.—6 months.

To Wolf Benjamin, of Plymouth Dock, Devon, for his composition varying in colour, with a peculiar method of applying for the purpose of rendering canvass, linen, and cloth durable, pliable, free from breaking, and water-proof, and also for preserving every kind and description of wood from wind or weather, whether applied to ships, houses, or manufactories, and for all purposes where paint, varnish or tar, are used for the purpose of

preservation

preservation or beauty, and whether applied to cannon or iron of every description.—5th May.—2 months.

To Thomas Todd, of Swansea, Glamorgan, for certain improvements in rolling of iron, and making wire, nails, brads, and screws.—7th May.—6 months.

To William Church, of Turner-street, Commercial-road, for certain improvements in or upon the machinery for making nails and spikes of various forms and dimensions, and also wire and screws of iron, copper, brass, or any other suitable metal.—7th May.—6 months.

To Henry Constantine Jennings, of Carburton-street, Fitzroy-square, in the parish of St. Marylebone, Middlesex, for his improvement in the mariner's compass.—7th May.—6 months.

To Robert Eccles, of Edinburgh, for his certain improvements in the masts, sails, and rigging of ships or sailing vessels.—9th May.—2 months.

To Thomas Hills, of Bromley, Middlesex, and Uriah Haddock, of the City-terrace, City-road, Middlesex, for improvement in the manufacture of sulphuric acid.—18th May.—6 months.

To Thomas Brown Milnes, of Lenton, Nottinghamshire, for certain improvements on machinery for the finishing of cotton, angola, and lambs'wool stockings, and other frame-work goods; also for the application of known powers to the working of the said machinery.—19th May.—6 months.

To Maurice St. Leger, of St. Giles, Camberwell, Surrey, for his improved method of making lime. — 19th May. — 6 months.

To Thomas Motley, of the Strand, Middlesex, for his improvement on ladders.—19th May.—2 months.

ASTRONOMICAL PHÆNOMENA, JUNE 1818.

D. H. M.		D. H. M.	
2. 11.20	☾ A ♉	18. .	☾ in perigee
4. .	☾ in apogee	18. .	♃ υ′ ♐ * 6′ N.
6.12.14.	☾ υ ♊	18.20.42	☾ φ ♐
7. .	☿ η ♉ * 3′ S.	19. 4. 4	☾ τ ♐
7. 4. 0.	☾ ψ² ♋	21. .	♀ 7 ♋ * 2′ S.
9. .	♀ 7 ♋ * 7′ S.	21.13.34	☾ ε ♑
9.12. 3.	☾ η ♌	21.14.25	☉ enters ♋
12.17.54.	☾ γ ♍	23. .	♂ 11 ♌ * 5′ S.
13. 8. 0.	☾ θ ♍	24.10.32	☾ 29 ♓
14.15.43.	☾ λ ♍	26.17.22	☾ o ♓
16 9.22.	☾ δ ♏	29.17.12	☾ A ♉

ASTRO-

ASTRONOMICAL OBSERVATIONS.

Longitude 9^{m} $26^{s}\cdot5$ W. Latitude 51° 43′ 1″·8.

Eclipse of the sun, May 4th. Beginning 5^{h} 48^{m} 15^{s} mean time.
End .. 7 33 26

Thin clouds passing during the whole of the observation render it in some degree imperfect: it may however be depended upon to five seconds.

Meteorological Journal kept at Walthamstow, Essex, from April 15 *to May* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twelve and Two P.M.]

Date.	Therm.	Barom.	Wind.
April			
15	47 58	29·65	E.—Sunshine; clear, and *cumuli;* very fine day; bright moon and star-light, and windy.
16	52 55	29·35	E.—Sun and wind; very fine day; rain and windy.
17	51 53	29·25	E.—Rain; fine day; moon-light, and windy.
18	44 51	29·30	E.—Wind and *cirrostratus;* very fine day; bright moon-light.
19	45 51	29·69	NE.—Clear and clouds, (*cumuli*); very fine day; bright moon-light.
20	37 54	29·85	NE.—White frost early; sunshine; very fine day; bright moon-light. Full moon.
21	39 56	29·65	NE—SE.—Clear cold wind, and sunshine; fine day; windy, and *cirrostratus.*
22			SE—S—E.—Gray morn; gray day; very dark night.
23	46 43	29·45	NE.—Rain; very rainy day; night rainy and windy.
24	48 50	29·39	NE.—Damp and calm; rain; cloudy and wind rising; night, great rain and wind.
25	44 57	20·09	SE.—Very damp; gleams of sun; fine day; star-light.
26	53 51	29·45	E by S.—Sunshine and calm; sun and showers; *cirrus* and clear; great lightning at night; *cirrostratus* low; clear night.
27	52 63	29·35	SE.—Rainy; fine day; *cirrostratus,* and clear night. Moon last quarter.
28	51 58	29·65	SW—NW.—Fine morn; very fine day; star-light.

May

Date.	Therm.	Barom.	Wind.
May			
29	42 62	30·05	SE.—Sunshine; fine day; star-light.
30	52 50	29·75	SE—NE—SE.— Cloudy; rainy till about 5 P.M.; very dark night.
May			
1	56 64	29·70	SW.—Clear and *cumuli*; fine day; star-light.
2	56 65	29·90	SE.—Slight rain; small showers, and gleams of sun; star-light.
3	53 61	29·51	E.—Rain in the night; *cirrus* and clear; fine day; after 5 P.M. great showers, and thunder; dark night.
4	50 69	29·50	NW.—Sun; fine windy day; bright star-light.
5	49 66	29·50	NE—SE.—Eclipse of the sun; sunshine, and foggy; fine day; frequent showers, and thunder after 7 P.M. rainy. New moon.
6	50 64	29·40	NE—SW.—Damp and rain; wind and *cumuli*; showers; clear and *cirrus*.
7	51 61	29·35	SW.—Windy and hazy; fine day, but some showers; star-light, and windy.
8	50 61	29·50	SW.—Sun, and *cirrus;* fine day; great rain after 5 P.M.; very rainy night and windy.
9	48 61	29·50	S—NW.—Great floods; sun, and *cumuli*; fine day, but some showers; moon and star-light.
10	57 63	29·90	SW—S.—Sun, and *cumuli*; very fine day; moon-light.
11	50 67	29·85	SW.—Sun and *cumuli*; very fine day; cloudy.
12	51 67	29·50	NW—W.—Rain; clear, and *cirrostratus*; fine day; moon-light.
13	49 67	29·55	SE.—Rain early; clear, clouds, and sun; very fine day; clear, and moon bright. Moon first quarter.
14	49 58	29·40	SE—S.— Sunshine early; some rain; fine day; moon, stars, and *cirrocumuli*.
15	52 68	29·50	SE—NW—SE.— *Cirrostratus*, and clear; very fine day, but not much sunshine; moon light, and some *cumuli*.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
April 15	10	51·	29·55	Very fine
16	11	50·5	29·53	Ditto
17	12	51·	29·51	Cloudy
18	13	47·	29·56	Fine
19	14	44·5	29·93	Cloudy
20	full	52·5	29·94	Very fine — the air was drier these two days than it has been for the three last years — wind E.N.E.
21	16	52·	29·89	Ditto
22	17	44·	29·83	Rain
23	18	43·	29·71	Ditto all day
24	19	42·	29·53	Ditto, both morning and evening
25	20	46·	29·35	Cloudy—rain A.M.
26	21	51·5	29·67	Cloudy—heavy rain in the evening with tremendous thunder-storm
27	22	61·5	29·63	Ditto—rain A.M.
28	23	59·	30·01	Very fine
29	24	60·5	30·09	Ditto
30	25	47·	29·85	Rain
May 1	26	57·5	29·87	Very fine
2	27	61·	29·98	Ditto
3	28	53·	29·63	Rain
4	29	60·	29·66	Fine
5	new	56·5	29·61	Ditto
6	1	58·	29·48	Cloudy—rain in the evening
7	2	53·5	29·49	Rain
8	3	63·	29·71	Fine—rain in the evening
9	4	59·	29·69	Ditto ditto
10	5	57·	29·94	Ditto
11	6	63·5	29·76	Ditto—rain P.M.
12	7	61·	29·77	Ditto
13	8	53·	29·51	Rain
14	9	60·	29·49	Rain

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For May, 1818.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	56	66	55	29·52	47	Fair
28	52	62	50	·80	49	Fair
29	50	64	47	·82	61	Fair
30	50	54	54	·56	0	Rain
May 1	53	62	55	·75	48	Fair
2	55	64	55	·72	39	Fair
3	54	65	57	·42	30	Showery
4	56	65	55	·50	38	Fair
5	55	64	52	·38	42	Fair
6	52	63	53	·26	20	Showery
7	53	64	52	·32	27	Showery
8	55	64	50	·50	26	Cloudy—heavy rain at night
9	54	63	49	·62	39	Showery
10	53	62	54	·72	51	Fair
11	54	64	55	·60	50	Fair
12	55	63	54	·58	45	Fair
13	54	60	48	·32	40	Showery
14	50	58	50	·34	36	Showery
15	55	62	54	·45	42	Cloudy
16	55	63	55	·62	48	Fair
17	55	55	54	·65	0	Small rain
18	56	68	50	·82	50	Fair
19	50	56	48	·90	42	Cloudy
20	48	64	47	30·00	57	Fair
21	47	56	47	·11	52	Fair
22	48	58	46	·16	54	Fair
23	46	58	46	·21	50	Fair
24	50	63	60	·26	45	Fair
25	50	65	52	·22	46	Fair
26	55	67	54	·23	52	Fair

N.B. The Barometer's height is taken at one o'clock.

LXVIII. *On the Pressure of the Earth against Revetements and Retaining Walls.* *By Mr.* THOMAS TREDGOLD.

It has been observed, in making experiments on the strength of revetements, that the pressure of the earth increases in a certain degree, the stability of the wall*. To account for this increase of stability, the writer of an article on the subject, in the *Encyclopædia Metropolitana*†, has adopted the method of resolving the force, resulting from the pressure of the earth, that appears to have been first practised by Rondelet ‡: but this method is evidently inferior to the one used by Coulomb and Prony, as it does not indicate the angle of fracture; consequently does not give the maximum pressure of the earth; and when the cohesion is omitted, it is much inferior even in point of simplicity.

The cohesion of the earth, which is so strongly objected to by the writer above mentioned, is not a necessary appendage to Coulomb's Theory, and would, perhaps, be better left out; also, similar objections might be urged against introducing the cohesion of the mortar in calculating the strength of the wall.

But to return to the stability imparted to the wall by the pressure of the earth.—It appears to result partly from friction; as it must be evident no motion could take place without friction against the back of the wall. I will, therefore, attempt to include its effect in an investigation of the pressure of the earth. Let ABCD (Plate IV. fig. 12) represent a section of the wall; and BDE a section of the prism of earth that slides forward in the case of fracture, DE representing the plane of fracture, which we will suppose to be a plane surface.

Put $W=$ the weight of the prism of earth acting against the wall.

$R=$ the resistance of the wall, or the horizontal force that retains the sliding prism on the plane of fracture.

$a=$ the angle which the plane of fracture makes with the horizon.

$f=$ the friction when the pressure is unity.

$h=$ the height of the wall $=$ BD.

And, $S=$ the weight of a cube of earth whose side is unity.

It is shown, by writers on mechanics, that when a body is sustained upon an inclined plane, and is in equilibrio, the whole force in the direction ED is $= \frac{W(\sin. a - f\cos. a)}{\text{radius}}$. (A)

* Col. Pasley's "Course of Military Instruction," vol. iii. chap. xxv.

† Art. *Mechanics*, section xxv. part i.

‡ *L'Art de Bâtir*, tome iii. p. 128.

This force is opposed by the resistance of the wall, which may be supposed to act in a horizontal direction; and when reduced to the direction DE it becomes $= \frac{R.(\cos a + f \sin. a)}{\text{radius}}$. (B)

Also, the pressure against the back of the wall is equal to R; consequently the friction is $= Rf$; and when this force is reduced to the direction DE, it is $= \frac{R.(f \sin. a - f^2 \cos. a)}{\text{radius}}$. (C)

And, when the forces (A), (B), and (C) are in equilibrio, we have

$$R.(\cos. a + 2f \sin. a - f^2 \cos. a) = W.(\sin. a - f \cos. a).$$

But, making the radius $=1$, this equation may be put under a form better suited to the present purpose; that is

$$R = \frac{W.(\tan. a - f)}{1 + 2f \tan. a - f^2}. \quad (D)$$

Or, because $W = \frac{1}{2} h^2 S \times \frac{1}{\tan. a}$;

$$R = \frac{\frac{1}{2}h^2 S(\tan. a - f)}{\tan. a + 2f \tan.^2 a - f^2 \tan. a}. \quad (E)$$

The second part of this expression becomes a *maximum* when

$$\tan. a = f + \sqrt{\left(\frac{1+f^2}{2}\right)}.$$

And if the angle which the plane of repose makes with the horizon be denoted by c; then $f = \frac{\sin. c}{\cos. c}$; consequently

$$\tan. a = \frac{\sin. c + \sqrt{\frac{1}{2}}}{\cos. c}. \quad (F)$$

If the friction against the back of the wall had been neglected, the expression for the tan. a would have become equivalent to the simple and elegant one obtained by M. de Prony.

The value of the tan. a (F) being introduced in the equation (E), it becomes

$$R = \frac{\frac{1}{2} h^2 S}{\sin. c \sqrt{2} + 1 + \frac{\sin.^3 c \sqrt{2} + \sin.^2 c}{\cos.^2 c} + \frac{\sqrt{2}}{2 \cos. c}}. \quad (G)$$

But the numeral value of the denominator will be constant for the same kind of earth; and let this value be $= t$;

$$\text{then } R = \frac{h^2 S}{2t}. \quad (H)$$

And as the wall may either slide on its base, or turn on the point C as a centre of motion; it may be shown, that in the latter case, the leverage $Dp = \frac{1}{3}h$.

The preceding inquiry extends only to retaining walls, counterscarp revetements, terrepleins without parapets, &c. But a simple

simple case of partial scarp revetements presents itself, which it would be as well to consider.

Suppose the revetement (fig. 13) to have a berm AB equal to the breadth of the top of the wall, and that the plane of fracture produced would intersect the earthen scarp at G; also let h' be the height of this point above the top of the wall $=$ FB; h being the height of the wall as before. It is obvious that the equation (D) still expresses the conditions of equilibrium; that is,

$$R = \frac{W.(\tan.a - f)}{1 + 2f\tan.a - f^2}.$$

But if b be the angle which the front of the scarp BG makes with the horizon; then by supposition $\frac{h + h'}{\tan.a} = \frac{h'}{\tan.b}$; therefore $W = \frac{1}{2}hS\frac{h + h'}{\tan.a}$. And as this change in the value of W will not affect that of the angle of fracture in any considerable degree, it may be neglected; in which case

$$R = hS\frac{h + h'}{2t}. \quad \text{(I)}$$

Hence the leverage becomes $Dp = h\frac{2h + 3h'}{6(h + h')}$, and the value of h' is $= \frac{h\tan.b}{\tan.a - \tan.b}$.

But in the actual construction of revêtements the angle b is generally 45°, and in that case $h' = \frac{h}{\tan.a - 1}$; consequently

$$R = h^2S\frac{\tan.a}{2t(\tan.a - 1)}. \quad \text{(K)}$$

$$\text{And, } Dp = h\frac{2\tan.a + 1}{6\tan.a}.$$

The equation (K) might be applied to demirevetements, and to partial revêtements in general, where it would be accurate enough for practical purposes: and it must be remembered that the old methods fail completely when applied to these revetements.

According to the principles I have endeavoured to point out, it appears, that when the earthen scarp exceeds the height h', it might be raised to any height without increasing the pressure against the wall; as is indicated by the experiments of Col. Pasley*. But if we suppose (with Belidor, Rondelet, &c. &c.) that the angle of fracture coincides with the natural slope of the earth; and also that the inclination of the earthen scarp BG (fig. 13) is the same as the natural slope (which is generally taken at 45°); it is obvious that the pressure ought to increase with the height of the earthen scarp, without limit.

* "Course of Military Instruction," vol. iii. chap. xxv.

This, however, is directly contrary to the results of the experiments above quoted, and to the experience of every practical man, who has been guided by the intuitive suggestions of his own feelings in preference to the rules of mechanical writers.

20, Bentinck-street, May 25, 1818.

LXIX. *On the Seeds of Plants. By Mrs.* Agnes Ibbetson.

To Mr. Tilloch.

Sir, — My object, it is well known, is to simplify my subject, render the real knowledge of the form and nature of plants easy; to furnish the means of comparing together the analysis of their resemblance and their differences; and by these means assign to each part *a name*, that may recall not only an appellation, but the idea of their *properties* and *qualities*, and what situation *each part should hold* in their general *character* in the *vegetable world*, when compared with the *animal creation*.

To show the formation of a plant by taking plants up *progressively* from the ground, and then *dissecting them in their increase*, must appear (I should suppose) of all means the *most exact* and *conclusive;* since one specimen becomes inevitably the correction of the preceding ones.

It is certainly the most *exact method* of understanding the formation of a plant, though it was never before known that a vegetable could be so taken: but common sense shows, that if the last part can be examined *progressively* and outwardly, the first should be sought in the same manner, in its *regular process in the interior;* and if every part of a plant assimilates with the animal creation in so exact a manner *interiorly*, is it likely it should differ in so essential a point as to form its *progeny* or *seed between the skins?* Is there a single instance in nature of such a formation, either animal, reptile, or insect tribe? the latter of which so much resembles plants. In my last letter I again brought forward that curious circumstance of which each dissection confirms the *truth*, and which forms a most beautiful and simple *delineation* of the construction of *vegetable nature;* I mean the fact "that the corculum of the seed is protruded in the root." It has been said that I assert "that the seed is formed there." I appeal to my various letters in this work and in Nicholson's Journal, for the refutation of this charge; proving there positively, that I never advanced such a proposition. No two things can be more different than the corculum of the seed and the seed itself; the first being inserted within the second in the

way

way of a loose ball, perfectly unfastened, but by a string, and always independent of the interior of the seed,—nay, in its first week's entrance easily to be shaken out; as for instance, the heart of the walnut, nut, bean, &c. and every other kind of vegetable. The corculum when first composed in the radicle is simply a diminutive ball of powder, which may be seen to insert itself into its allotted place in the seed, as soon as it gains the top of the plant: but first it crosses the *root* adjoining the radicle, and runs up the *alburnum vessels*, which convey it by degrees up to the flower; it then consists of the *ball*, and a diminutive *sprig within*, which was inserted while leaving the radicle and passing into the root. That there can be no doubt of the existence of these *diminutive balls*, nor the shoot within them, *is certain*; since they were first discovered by Leuwenhoek, and afterwards that extreme clever philosopher Mr. Henry Baker;—nor did the latter know of the previous discovery. I also was in the same predicament; from not possessing the works of either of these gentlemen, I was ignorant that they preceded me. But sure such testimonies in favour of the fact, and from such a quarters, do not require my assurance of its truth,—and is it likely that three dissectors should have been convinced of its exactness by continual observation, if it was not just? I must also notice, that these pictures of the heart of the seeds are taken much earlier than any one has before *drawn these specimens*—when the corculum had just entered the empty bag of the seed, I know no one besides who has dissected them so early. Why then is it doubted, after such evidence? Has any person proved by dissection that it is a mistake? Has any one dissected progressively, to inquire into the truth of it?—Have they tried and followed the various specimens for one year, taking the seed from the first formation to its completion? No. How then can they know these facts to be false, when it is only by this means their truth can be properly ascertained? Another gentleman gave his testimony of having seen the balls pass up the alburnum. Sir J. E. Smith saw those balls in a specimen I sent him, and took them for nutriment. He was perfectly right in the idea; but not the *situation he assigned to the nourishment*, which is never found in the alburnum; but the balls being followed up to *impregnation*, could not be any *other matter* than a part of the *seeds*, or rather the seeds themselves.

To prove of what consequence it is to take plants *progressively*, and how *impossible it is* to comprehend them if *not so taken*, I shall show the progress of the *heart* of the *seeds*, from the moment they are formed in the radicle till they *enter* the *seeds* in the seed-vessel: and giving specimens sufficient *just* to *mark* the progress of the *whole* series, and prove that these balls must be the heart of seeds. Fig. 1, is the radicle: the termination of the thread-root *a a*, is the powder which enters from the earth, and forms itself into a ball; and *b b* is the part of the radicle which joins to the larger root, in which the diminutive shoot is inserted within the ball. Fig. 2, is where the hearts of the seeds cross the large root *c c*, and make their way to the alburnum vessels *d d*, running up each of these in the stem to attain the bud generally discovered at top. Fig. 3, is where the hearts of the seeds run up the *short flower-stalks*, and enter the flower-buds at the bottom of the flower, which is the seed-vessel:—the flowers are then very young buds. Fig. 4, is where the bags of the seeds are placed in regular order in the seed-vessel, and open their tops for the reception of the hearts of the seeds, and afterwards of the atmospheric nutriment, which soon half fills the bags of the seeds at the top where the heart is; *f f f*, also fig. 4, shows the atmospheric hair. Fig. 5, is where the nutriment of the root is received, the vessels being made for the purpose of throwing up the powder, which is at this time only to be traced from the root upwards, while all the mechanism of the hairs has disappeared, or changed their forms. Fig. 6, is where the seeds are impregnated: the cord *e e* having once before passed through the seed-vessel, has run up the pistil to fetch the powder of the stamen, which it there mixes with the sweet juices of the pistil; and they together form that vivifying matter which enables the shoot in the heart of the seed to spread and form its primordial sprig: it also enables the whole seed-vessel to surround itself with the female shoot, which soon form the new stripes which run up the wood of the young plant when in the earth (as I shall in future time show).

I must still give another specimen, (fig. 7,) which serves most admirably to prove how true it is that the powder flows up the stem of all plants, to fill the seeds (see fig. 5); since, if the stem is cut at that time, it will be found with six or seven apertures open the whole way up the stem (see *a a a*) for that purpose; that s, regular vessels that convey the powder up the pith, which being cut horizontally, show the open mouths of those vessels and the seed powder that runs up them, fig. 7, *a a a*:—this lasts but a week or two at most, and then the vessels form themselves into other matter, equally serviceable. In short, the whole of this process proves that the heart of the seed is formed by the matter which comes from the earth; that before it leaves its radicle it acquires a diminutive shoot; that when that shoot is acquired, the balls run across the root, up the various alburnum vessels; that they convey them up these vessels to the various buds, at the bottom of which the empty seed-bags are ready to receive them; that they run up the flower-stalks into these seed-bags, and

and fasten themselves at the top of the seed; and then alternately take in first the liquid matter of the atmosphere, then the powder of the root, and that thus the seed is filled; when the flower then becomes perfect, the pistil shows its juice in its sparkling drop, ready to receive the powder of the stamen which soon falls on it, and the seeds with their corculum become fructified:—Followed up thus, how can the heart be any thing but what I have named it? If one specimen failed, it would prove the whole false. I therefore once more appeal to common sense, to show that this method of dissecting vegetables is exactly like that allowed to be so perfect in the egg, where it is taken from the moment of dropping from the hen, the observation continuing till the chicken is hatched, by alternate eggs taken as they advance in their progressive improvement,—thus showing the perfect formation of the bird in a regular series. I shall perhaps be blamed for this repetition of description, and thus giving in a following course, some of the prints I have before ventured to give singly: but I have met with such contradiction, and been repaid by such unbelief; that though it is a series of dissections of nearly seventeen years standing, constantly corrected by yearly observation, and made perfect by repeated reviews of the several facts; yet it is treated as if it was the hasty production of a few months, uncorrected by experience and unsupported by the observation of others*. I cannot help, therefore, from my real love of science, making a last effort to establish its truth, though I do it at the edge of the grave; and in a situation when I can no longer have any other interest in it but what that regard bestows. I cannot still but wish the truth may appear, while the eyes (discoverers of the whole) may be able to correct any mistakes which may be noticed in thus reviewing the subject.

It is said it must be false, because the solar microscope deceives. The solar microscope may show a diversity of lights, but these discoveries have been vindicated by the lowest powers of optics. It is in the common half-guinea microscope that the greatest part of this has been viewed and drawn, with now and then the use of the compound. When first I dissected plants, I began by very high powers; but soon I corrected this error, and commenced

* I have already noticed that in the hearts of the seeds alone, as coming from the roots, and forming the branches within, I can bring four of our first dissectors, who vindicate me in various parts of this series of discoveries. I have already shown that Leuwenhoek and Mr. Baker saw the shoot in the heart of the seed before impregnation; that another gentleman drew the balls or hearts of the seeds when mounting in the alburnum vessel; and that they observed with great astonishment, that the seeds were as perfect in a very young bud as in a larger one, which seem to look like a previous formation.

 by

by very low ones, and raised them by degrees; by which means I became a perfect judge of the truth of the drawings presented: and as I always changed my glass according as my object required, (since without this many parts must have remained perfectly unknown,) but by viewing the object thus very little enlarged, and then increasing its size by slow degrees, I often drew the specimen as all the various powers presented, and thus compared them together, considering each part in its variety of points and varyings;—and it is astonishing of what use this method was to me, in learning to comprehend the hairs in particular; since it enabled me often to see the entrance of the liquid into the retort, and to pursue it in its after-course in the veins of the plant; and this means alone proved to me how many times the hairs were altered, for the purpose of their preparing, analysing, and converting the various liquids of the atmosphere, as is beautifully seen in the cucumber hairs;—for can the fact bear another construction? When the seeds receive in that plant the juices of the atmosphere, they do not (as in the wheat and gourd) retain the same formed hairs throughout the whole enlargement of the flower; but correct the juices, and prepare them for insertion into the plant, through those hairs, by changing their form three different times (see fig. 8, 9 and 10): and this change is made visible by an alteration of colour in the juice also. Thus the first matter they receive from the atmosphere, and which flows into the seeds through the hairs, is apparent water, by means of a simple retort, as at fig. 8; but as soon as the seeds are prepared for receiving, the corculum, fig. 9, is dispensed to them; and though the liquid enters the hairs like water, it changes twice, first to milky white and then to green; and has one of the most complicated hairs to bestow it on the plant;—they then alter to the atmospheric nutriment, and the juice becomes green, and then a deep yellow within the fig. 10. Why should the form and shape of the hairs be thus altered, if they did not reanalyse the juices and newly concoct them for the seeds? Why should the colours alter as the juices are pressed from valve to valve, where compression or expletion often takes place in producing the proper effect, and rendering it fit for the seed? If they require not other preparations, why is the formation of each seed so different in this respect, one demanding the same hair throughout the whole formation of the seed (as in the gourd and wheat), while the cucumber requires it to be so often changed?—The matter speaks for itself. It is assuredly not possible to give another interpretation to this phænomenon, which has indeed been followed by me from specimen to specimen, till the whole appears so very simple and plain, that I am astonished a dissenting voice can arise;—nor could it be so, but from the want of seeing in the incredulous

incredulous person. When the last juice is taken into the seed from the atmosphere, then the hairs continue in the cucumber, which is an unusual case; nor do these hairs die away till the fruit grows of some age. The last figure is a curious kind of bottle with a top, which visibly produces an effect on the liquid; and when the yellow juice is received into the seeds they are about half full, though sometimes apparently distended, but soon followed by the powder of the root: in most plants, the hairs then die away before the next process.

In my last letter I showed in the view of the corculum, how, after mounting the seed, it entered the seed-vessel and seed: I showed it in two very different genera of plants, the wheat and gourd. I have since tried near forty different sorts—every plant I could gain, and they all filled their seeds nearly in the same manner. But in trees (as I have before shown) there is this previous alteration, owing to the arresting the seed so long in the seed-vessel:—when the corculum runs from the alburnum vessels they surround the buds, and for a time become torpid, while the buds change their place, arranging themselves by degrees on the new shoot formed for the purpose. The hearts then follow them, and gather into one collection at the bottom of the new shoot; and preparing a vessel for the purpose, run within it, in the curious manner I have before shown, dropping into each bud so many hearts as the seeds require. But in all other plants, especially annuals and herbaceous plants, and those I am now showing, they proceed without stopping, to the seed-vessel and seeds. One of the most curious subjects I have discovered in plants is the entrance of the juices through the hairs of plants. I have repeatedly shown that that which was taken for perspiration in plants, is the reception of the atmospheric juices into the plants: indeed it is a disgrace to science that this mistake should subsist, while the most minute microscope will show the folly of calling that perspiration which is on a long pedestal, and which changes its forms with each plant, and presents a set of chemical glasses that when magnified, can only be construed into *retorts*, *receivers*, *pneumatic instruments*, &c. &c. Some of them resemble ours; some are so curious that they fill the mind with astonishment, and show that if we properly attended to them without prejudice or the spirit of contradiction, we should gain a knowledge in areometry, no other *subject could so well supply*; for the manner in which these instruments manage the gases, and contrive to form a vacuum whenever it is necessary, between the astonishing valves of their various formed retorts, is most marvellous. But I have seen them repeatedly, when much magnified, boil up as on fire, while a part has exploded without the smallest danger to the rest,—but that is not always the case. How can such

such instruments as I have now shown at 8, 9 and 10, be called perspiration? and yet they are all of the same kind, and I never yet saw one without a stand. If the purport was to throw out a juice from the vegetable, it is easily done: but great mechanism is required to draw a liquid within the plant, especially as the juices are also to be prepared by the affinity of the different matters, by means of chemical attraction and attraction of composition: nor do I the least doubt that, minute as these hairs are, electricity plays her part among them, and assists in these various labours of *natural art.*

It is provoking to consider that this formation rather increases our difficulties in accounting for the means and laws by which plants are governed; as it is much more easy (difficult as we have hitherto found it) to suppose the flow of a liquid up the stem of a tree, than to imagine a power by which a powder is thus conveyed. We have capillary attraction, the force and pressure of air, &c. &c. but I know of no law in areology or in hydrostatics that can carry a powder, especially an almost dry powder (as it appears): and yet no person can dissect long, and not admit the validity of this process. That several different powders are conveyed up the trunk of an amazing high tree, by means of vessels protruded for the time, and that apertures are to be seen up which the powders pass, if horizontal cuttings are made in the trunk,—is a truth I have ascertained beyond all contradiction; but that the time must be watched, as it lasts but a week or two in the year. There must then be a *power*, a *law* in *nature* we are not yet informed of, or acquainted with, which will account for these wonders. But by following nature in her *progressive movements* we shall easily reach the development of the whole, provided we condescend to be led *by nature*, not to *lead her*: if, instead of following by exact dissections, we choose always to go back to the authors that have existed *previous to us*, we shall never *advance*, but always add to our mistakes and increase our difficulties. Sure it is the strangest thing, when I wish to know how a tree is made, and that I have perseverance to go on in the study, that I should prefer going back to Malpighy, &c. &c. rather than take that tree down, cut it to pieces, watch in others like it, their daily growth, and follow their increase;—would not constant examination in four or five years teach me more than Malpighy? I have the greatest opinion of our predecessors in botany, particularly in dissective botany: but I would rather examine them *after studying nature* than *before*, and compare nature and my own opinions *together*, afterwards.

I am, sir,

Your obliged servant,

AGNES IBBETSON.

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Description of the Plate No. 7, [see Pl. IV.]

Fig. 1. Presents the end of the radicle just where it is enlarged from the side root. *a a* are the first forming of the balls, which are certainly the hearts of the seeds; and *b b* the larger balls where they take in the little shoot. Leuwenhoek and Mr. H. Baker saw this shoot as well as I.

2. The alburnum vessels branching up from the root *c c*, till they reach the buds passing through the alburnum vessel.

3. The bud of the cucumber cut longitudinally, showing its interior, *d d* its female flowers, with *e e* its seed-vessel and seeds, when the hearts of the seeds begin to fix themselves on the seeds *f f*, and run up the flower-stem *g g*.

4. The seed-vessel cut horizontally, showing the peculiar hair belonging to the first fixing of the seeds, when they are taking in their hearts to the seeds, as at *h h*.

5. The cutting longitudinally when the seeds receive the juices of the atmosphere from the peculiar hair *i i*.

6. When the same hair is continued and the matter is received from the roots by the peculiar figure *k k*.

7. The horizontal cutting of the stem of the cucumber, showing the apertures which admit the powder all the way up the root within the pith.

8, 9, 10. The three hairs: the first begins in the very early bud.

11. The sort of excrescence that admits the powder, and throws it into the seed at *l l*.

LXX. *On a new Mode of Artificial Congelation.* *By* John Leslie, *F.R.S. E. Professor of Mathematics in the University of Edinburgh*.*

We have now to relate a discovery which will enable human skill to command the refrigerating powers of nature; and, by the help of an adequate machinery, to create cold and produce ice, on a large scale, at all seasons, and in the hottest climates of the globe. But, to explain this interesting subject with greater clearness and accuracy, it is requisite to trace the successive advances which conducted to the result. Where a conclusion appears simple, the careless observer is apt to suppose it easily at-

* From Supplement to the fourth and fifth editions of the Encyclopædia Britannica, vol. iii. part i.

tained;

tained; yet, though sound philosophy tends always to simplification, the rare quality of simplicity is scarcely ever the flash of intuition, but the slow fruit of close and patient investigation.

In pursuing the researches with his hygrometer, Professor Leslie was early induced to inquire into the condition of the higher atmosphere, and its relations to humidity. He thus detected a fact of great importance in meteorology, and pointing at various ulterior views.

Investigation of its Principles.

As rarefaction enlarges the capacity of air for heat, so it likewise augments the disposition to hold moisture; at the same time, that the removal of the ordinary pressure facilitates the expansion of the liquid matter, and its conversion into a gaseous form. Accordingly, if the hygrometer be suspended within a large receiver, from which a certain portion of air is quickly abstracted, it will sink with rapidity. In summer the additional dryness thus produced amounts to about 50 hygrometric degrees, each time the air has its rarefaction doubled; so that, supposing the operation of exhausting to be performed with expedition, and the residuum reduced to a sixty-fourth part, the hygrometer would mark a descent of 300°. But this effect is only momentary; for the thin air very soon becomes charged with moisture, and, consequently, ceases to act on the wet ball of the hygrometer. The cold, however, excited on the surface of that ball, by such intense evaporation, will have previously frozen the coating.

The increased power of aqueous solution which air acquires as it grows thinner, being ascertained and carefully investigated, the object was to combine the action of absorbent with the transient dryness produced within a receiver by rarefaction. The sentient ball of the hygrometer being covered with dry salt of tartar, the instrument first indicated increasing dryness, and afterwards, as the rarefaction proceeded, it changed its course, and marked humidity. The same variation of effect nearly was observed, when the hygrometer had been wetted as usual with pure water, and a broad saucer containing the mild vegetable alkali was placed on the plate of the air-pump. It was thus proved, that the action of this imperfect absorbent is soon overpowered by the tendency to vaporization in attenuated air, and that, beyond a certain limit, it surrenders its latent moisture.

Mr. Leslie resolved, therefore, to try the effect of sulphuric acid, whose peculiar energy as an absorbent he had, under other circumstances, already ascertained. But various incidents prevented him, for a considerable time, from resuming his philosophical inquiries. At last he began those projected experiments, and

and was almost immediately rewarded by the disclosure of a property, the application of which blazed on his fancy. In the month of June 1810, having introduced a surface of sulphuric acid under the receiver of an air-pump, he perceived with pleasure that this substance only superadded its peculiar attraction for moisture, to the ordinary effects resulting from the progress of exhaustion; and, what was still more important, that it continued to support, with undiminished energy, the dryness thus created. The attenuated air was not suffered, as before, to grow charged with humidity; but each portion of that medium, as fast as it became saturated by touching the wet ball of the hygrometer, transported its vapour to the acid, and was thence sent back denuded of the load, and fitted again to renew its attack with fresh vigour. By this perpetual circulation, therefore, between the exhaling and the absorbing surface, the diffuse residuum of air is maintained constantly at the same state of dryness. The sentient ball of the hygrometer, which had been covered with several folds of wetted tissue paper, was observed, at an early stage of the operation, suddenly to lose its blue tint and assume a dull white, while the coloured liquor sprung upwards in the stem, where it continued, for the space of a minute, stationary, and again slowly subsided. The act of congelation had, therefore, at this moment taken place, and the paper remained frozen several minutes, till its congealed moisture was entirely dispersed. Pursuing this decisive intimation, the hygrometer was removed, and a watch-glass filled with water substituted in its place. By a few strokes of the pump, the whole was converted into a solid cake of ice, which, being left in the rare medium, continued to evaporate, and, after the interval of perhaps an hour, totally disappeared. A small cup for holding the water was next adopted, and the whole apparatus gradually enlarged.

Efficient Power nearly the same at all Temperatures.

The powers, both of vaporization and of absorption, being greatly augmented in the higher temperatures, the same limit of cold nearly is in all cases attained, by a certain measure of exhaustion. When the air has been rarefied 250 times, the utmost that, under such circumstances, can perhaps be effected, the surface of evaporation is cooled down 120 degrees of Fahrenheit in winter, and would probably sink near 200 in summer. Nay, a far less tenuity of the medium, when combined with the action of sulphuric acid, is capable of producing and supporting a very intense cold. If the air be rarefied only 50 times, a depression of temperature will be produced, amounting to 80 or even 100 degrees of Fahrenheit's scale.

Mode

Mode of proceeding.

We are thus enabled, in the hottest weather, to freeze a mass of water, and to keep it frozen, till it gradually wastes away, by a continued but invisible process of evaporation. The only thing required is, that the surface of the acid should approach tolerably near to that of the water, and should have a greater extent; for otherwise the moisture would exhale more copiously than it could be transferred and absorbed, and, consequently, the dryness of the rarefied medium would become reduced, and its evaporating energy essentially impaired. The acid should be poured to the depth perhaps of half an inch in a broad flat dish, which is covered by a receiver of a form nearly hemispherical; the water exposed to congelation may be contained in a shallow cup, about half the width of the dish, and having its rim supported by a narrow porcelain ring upheld above the surface of the acid by three slender feet. It is of consequence that the water should be insulated as much as possible, or should present only a humid surface to the contact of the surrounding medium; for the dry sides of the cup might receive, from communication with the external air, such accessions of heat, as greatly to diminish, if not to counteract the refrigerating effects of evaporation. This inconvenience, however, is in a great measure obviated, by investing the cup with an outer case at the interval of about half an inch. If both the cup and its case consist of glass, the process of congelation is viewed most completely; yet when they are formed of a bright metal, the effect appears on the whole more striking. But the preferable mode, and that which prevents any waste of the powers of refrigeration, is to expose the water in a pan of porous earthen-ware. If common water be used, it will evolve air bubbles very copiously as the exhaustion proceeds; in a few minutes, and long before the limit of rarefaction has been attained, the icy spiculæ will shoot beautifully through the liquid mass, and entwine it with a reticulated contexture. As the process of congelation goes forward, a new discharge of air from the substance of the water takes place, and marks the regular advances of consolidation. But after the water has all become solid ice, which, unless it exceed the depth of an inch, may generally be effected in less than half an hour, the circle of evaporation and subsequent absorption is still maintained. A minute film of ice, abstracting from the internal mass a redoubled share of heat, passes, by invisible transitions, successively into the state of water and of steam, which, dissolving in the thin ambient air, is conveyed to the acid, where it again assumes the liquid form, and, in the act of combination, likewise surrenders its heat.

Moderate

Moderate Rarefaction sufficient to maintain the Ice.

In performing this experiment, the object is generally to seek at first to push the rarefaction as far as the circumstances will admit. But the disposition of the water to fill the receiver with vapour, being only in part subdued by the action of the sulphuric acid, a limit is soon opposed to the progress of exhaustion, and the included air can seldom be rarefied above a hundred times, or till its elasticity can support no more than a column of mercury about three-tenths of an inch in height. A smaller rarefaction, perhaps from ten times to twenty times, will be found sufficient to support congelation after it has once taken place. The ice then becomes rounded by degrees at the edges, and wastes away insensibly, its surface being incessantly corroded by the play of the ambient air, and the minute exhalations conveyed by an invisible process to the sulphuric acid, which, from its absorbing the vapour, is all the time maintained above the temperature of the apartment. The ice, kept in this way, suffers a very slow consumption; for a lump of it, about a pound in weight and two inches thick, is sometimes not entirely gone in the space of eight or ten days. During the whole progress of its wasting, the ice still commonly retains an uniform transparent consistence; but, in a more advanced stage, it occasionally betrays a sort of honey-combed appearance, owing to the minute cavities formed by globules of air, set loose in the act of freezing, yet entangled in the mass, and which are afterwards enlarged by the erosion of the solvent medium.

But almost every practical object is attained, through far inferior powers of refrigeration. Water is the most easily frozen, by leaving it, perhaps for the space of an hour, to the slow action of air that has been rarefied only in a very moderate degree. This process meets with less impediment, and the ice formed by it appears likewise more compact, when the water has been already purged of the greater part of its combined air, either by distillation or by long continued boiling. The water which has undergone such operation, should be introduced as quickly as possible into a decanter, and filled up close to the stopper, else it will attract air most greedily, and return nearly to its former state in the course of a few hours.

Elegant Mode of Freezing.

The most elegant and instructive mode of effecting artificial congelation, is to perform the process under the transferrer of an air-pump. A thick but clear glass cup being selected, of about two or three inches in diameter, has its lips ground flat, and covered occasionally, though not absolutely shut, with a broad

broad circular lid of plate-glass, which is suspended horizontally from a rod passing through a collar of leather. This cup is nearly filled with fresh distilled water, and supported by a slender metallic ring, with glass feet, about an inch above the surface of a body of sulphuric acid, perhaps three quarters of an inch in thickness, and occupying the bottom of a deep glass bason that has a diameter of nearly seven inches. In this state, the receiver being adapted, and the lid pressed down to cover the mouth of the cup, the transferrer is screwed to the air-pump, and the rarefaction, under those circumstances, pushed so far as to leave only about the hundred and fiftieth part of a residuum; and the cock being turned to secure that exhaustion, the compound apparatus is then detached from the pump, and removed to some convenient apartment. As long as the cup is covered, the water will remain quite unaltered; but, on drawing up the rod half an inch or more, to admit the play of the rare medium, a bundle of spicular ice will, after the lapse perhaps of five minutes, dart suddenly through the whole of the liquid mass; and the consolidation will afterwards descend regularly, thickening the horizontal stratum by insensible gradations, and forming in its progress a beautiful transparent cake. On letting down the cover again, the process of evaporation being now checked or almost entirely stopped, the ice returns slowly into its former liquid condition. In this way, the same portion of water may, even at distant intervals of time, be repeatedly congealed and thawed successively twenty or thirty times. During the first operations of freezing, some air is liberated; but this extrication diminishes at each subsequent act, and the ice, free from the smallest specks, resembles a piece of the purest crystal.

Progress of Congelation.

This artificial freezing of water in a cup of glass or metal, affords the best opportunity of examining the progress of crystallization. The appearance presented, however, is extremely various. When the frigorific action is most intense, the congelation sweeps at once over the whole surface of the water, obscuring it like a cloud. But, in general, the process advances more slowly; bundles of spiculæ, from different points, sometimes from the centre, though commonly from the sides of the cup, stretching out and spreading by degrees with a sort of feathered texture. By this combined operation, the surface of the water soon becomes an uniform sheet of ice. Yet the effect is at times singularly varied; the spicular shoots, advancing in different directions, come to inclose, near the middle of the cup, a rectilineal space, which, by unequal though continued encroachment, is reduced to a triangle; and the mass below, being partly frozen, and therefore

therefore expanded, the water is gradually squeezed up through the orifice, and forms by congelation a regular pyramid, rising by successive steps; or, if the projecting force be greater, and the hole more contracted, it will dart off like a pillar. The radiating or feathered lines which at first mark the frozen surface, are only the edges of very thin plates of ice, implanted at determinate angles, but each parcel composed of parallel planes. This internal formation appears very conspicuous in the congealed mass which has been removed from a metallic cup, before it is entirely consolidated.—Sea water will freeze with almost equal ease; but it forms an incompact ice like congealed syrup, or what is commonly called *water-ice*.

When cups of glass or metal are used, the cold excited at the open surface of the liquid extends its influence gradually downwards. But if the water be exposed in a porous vessel, the process of evaporation, then taking effect on all sides, proceeds with a nearly regular consolidation towards the centre of the mass, thickening rather faster at the bottom from its proximity to the action of the absorbent, and leaving sometimes a reticulated space near the middle of the upper surface, through which the air, disengaged by the progress of congelation, makes its escape.

Singular Modification of the Process.

When very feeble powers of refrigeration are employed, a most singular and beautiful appearance is, in course of time, slowly produced. If a pan of porous earthen-ware, from four to six inches wide, be filled to the utmost with common water till it rise above the lips, and planted above a dish of ten or twelve inches diameter, containing a body of sulphuric acid, and then a broad round receiver placed over it; on reducing the included air to some limit between the twentieth and the fifth part of its usual density, according to the coldness of the apartment, the liquid mass will, in the space of an hour or two, become entwined with icy shoots, which gradually enlarge and acquire more solidity, but always leave the fabric loose and unfrozen below. The icy crust which covers the rim, now receiving continual accessions from beneath, rises perpendicularly by insensible degrees. From each point on the rough surface of the vessel, filaments of ice, like bundles of spun glass, are protruded, fed by the humidity conveyed through its substance, and forming in their aggregation a fine silvery surface, analogous to that of fibrous gypsum or satin-spar. At the same time, another similar growth, though of less extent, takes place on the under side of the pan, so that continuous icy threads might appear vertically to transpierce the ware. The whole of the bottom becomes likewise covered over with elegant icy foliations. Twenty or thirty hours may be required to produce those singular effects; but the upper body of ice continues to rise for the space of several days, till it forms a circular wall of near three inches in height, leaving an interior grotto lined with fantastic groups of icicles. In the meanwhile the exfoliations have disappeared from the under side, and the outer incrustation is reduced, by the absorbing process, to a narrow ring. The icy wall now suffers a regular waste from external erosion, and its fibrous structure becomes rounded and less apparent. Of its altitude, however, it loses but little for some time; and even a deposition of congealed films along its coping or upper edge, seems to take place, at a certain stage of the process. This curious effect is owing to a circumstance, which, as it serves to explain some of the grand productions of nature, particularly the *Icebergs* of the Arctic Circle, merits particular attention. The circular margin of the ice, being nearer the action of the sulphuric acid than its inner cavity, must suffer, by direct evaporation, a greater loss of heat; and, consequently, each portion of thin air that rises from the low cavity, being chilled in passing over the colder ledge, must deposit a minute corresponding share of its moisture, which instantly attaches itself and incrusts the ring. Whatever inequalities existed at first in the surface of the ice, will hence continually increase.

Artificial Congelation best performed on a large Scale.

Artificial congelation is always most commodiously performed on a large scale. Since the extreme of rarefaction is not wanted, the air-pump employed in the process admits of being considerably simplified, and rendered vastly more expeditious in its operation. Two or three minutes at most will be sufficient for procuring the degree of exhaustion required, and the combined powers of evaporation and absorption will afterwards gradually produce their capital effect. In general, plates of about a foot in diameter should be preferred, which can be connected at pleasure with the main body of the pump. The dish holding the sulphuric acid is nearly as wide as the flat receiver: and a set of evaporating pans belongs to it, of different sizes, from seven to three inches in diameter, which are severally to be used according to circumstances. The largest pan is employed in the cold season, and the smaller ones may be successively taken as the season becomes sultry. On the whole, it is better not to overstrain the operation, and rather to divide the water under different receivers, if unusual powers of refrigeration should be required. As soon as the air is partly extracted from one receiver, the communication is immediately stopped with the barrel of the pump, and the process of exhaustion is repeated on another. In this way, any number of receivers, it is evident, may

may be connected with the same machine. If we suppose but six of these to be used, the labour of a quarter of an hour will set as many evaporating pans in full action, and may, therefore, in less than an hour afterwards produce nearly six pounds of solid ice. The waste which the water sustains during this conversion is extremely small, seldom indeed amounting to the fiftieth part of the whole. Nor, till after multiplied repetitions, is the action of the sulphuric acid considerably enfeebled by its aqueous absorption. At first that diminution is hardly perceptible, not being the hundredth part when the acid has acquired as much as the tenth of its weight of water. But such influence gains rapidly, and rises with accelerated progression. When the quantity of moisture absorbed amounts to the fourth part by weight of the acid, the power of supporting cold is diminished by a twentieth; and, after the weights of both these come to be equal, the refrigerating energy is reduced to less than the half. Sulphuric acid is hence capable of effecting the congelation of more than twenty times its weight of water, before it has imbibed near an equal bulk of that liquid, or has lost about the eighth part of its refrigerating power. The acid should then be removed, and concentrated anew by slow distillation.

Congelation might be connected with the Steam-Engine.

When the exhaling and absorbing surfaces are rightly disposed and apportioned, the moderate rarefaction, from twenty to forty times, which is adequate to the freezing of water, may be readily procured by the condensation of steam. In all manufactures where the steam-engine is employed, ice may, therefore, at all times be formed in any quantity, and with very little additional expense. It is only required to bring a narrow pipe from the condensing vessel, and to direct it along a range of receivers, under each of which the water and the acid are severally placed. These receivers, with which the pipe communicates by distinct apertures, may, for the sake of œconomy, be constructed of cast-iron, and adapted with hinges to the rim of a broad shallow dish of the same metal, but lined with lead to hold the acid.

Congelation of Mercury.

The combined powers of rarefaction and absorption are capable of generating much greater effects than the mere freezing of water. Such frigorific energy, however, is at all times sufficient for effecting the congelation of mercury. Accordingly, if mercury, contained in a hollow pear-shaped piece of ice, be suspended by cross threads near a broad surface of sulphuric acid under a receiver; on urging the rarefaction, it will become frozen, and may remain in that solid state for the space of several hours.

 But

But this very striking experiment is easily performed without any foreign aid. Having introduced mercury into the large bulb of a thermometer, and attached the tube to the rod of a transferrer, let this be placed over the wide dish containing sulphuric acid, in the midst of which is planted a very small tumbler nearly filled with water. After the included air has been rarefied about fifty times, let the bulb be dipped repeatedly into the very cold but unfrozen water, and again drawn up about an inch; in this way, it will become incrusted with successive coats of ice, to the thickness perhaps of the twentieth part of an inch. The water being now removed, the pendent icicle cut away from the bulb, and its surface smoothed by the touch of a warm finger, the transferrer is again replaced, the bulb let down within half an inch of the acid, and the exhaustion pushed to the utmost. When the siphon-gauge has come to stand under the tenth of an inch, the icy crust starts into divided fissures, and the mercury, having gradually descended in the tube till it reach its point of congelation, or 39 degrees below zero, sinks by a sudden contraction almost into the cavity of the bulb; and the apparatus being then removed and the ball broken, the metal appears a solid shining mass, that will bear the stroke of a hammer.

Still greater Cold created.

But a still greater degree of cold may be created, by applying the same process likewise to cool the atmosphere which encircles the apparatus itself. A glass matrass was blown nearly of a hemispherical shape, its bottom quite flat, and about three inches in diameter, and its neck about half an inch wide and cut square over. The whole was covered with a coat of patent lint, which takes up water very copiously, a portion of sulphuric acid was next introduced, forming a layer of perhaps a quarter of an inch thick, and a spirit of wine thermometer, having its bulb also cased with wetted lint, was then inserted within the matrass, a brass ring attached to the tube securing it in the right position. Things being thus arranged, the matrass or flat bottle, with its thermometer, was placed on a slender stool with glass feet, about an inch above the sulphuric acid in the broad bason, and the large receiver luted over it. The air was then partly extracted, till the gauge came below one inch. In a few minutes the lint was frozen entirely, and looked white. After an interval of a quarter of an hour, to allow time for the evaporation of that icy coat to cool down the interior apparatus, the pump was again urged, and the exhaustion pushed to about three-tenths of an inch. In a short while, the inclosed thermometer sunk not fewer than 180 degrees, and remained stationary, till the ice had wasted away.

It

It is obvious, therefore, that the refrigerating powers could be pushed still further by a judicious combination of the apparatus. It would be easy to show, that the maximum effect is obtained, when the dimensions of the successive cases rise in a geometrical progression. The action, however, is not doubled for each additional case, but increased rather more than one-half.

Simpler Mode of Congelation, by dry pounded Whinstone, or parched Oatmeal.

These plans are difficult in the execution; and though they enlarge our conceptions of the extent of the descending scale of heat, yet they furnish merely speculative results. A very important practical improvement has been lately made in the process of artificial congelation. Sulphuric acid is certainly a cheap and most powerful agent of absorption; but the danger in using such a corrosive liquid, especially by unskilful persons, formed always a serious obstacle to its general adoption. Mr. Leslie had early noticed the remarkable absorbent quality of our mouldering whinstone or porphyritic trap; and in April 1817 he substituted that material, grossly pounded and dried before a kitchen fire, instead of sulphuric acid, and actually froze a body of water with great facility. This earth will attract the fiftieth part of its weight of moisture before its absorbent power is reduced to the one-half, and is hence capable of freezing the sixth part of its weight of water. It may be repeatedly dried, and will, after each operation, act with the same energy as at first.

But an absorbent still more convenient and powerful has since occurred to Mr. Leslie;—it is merely *parched oatmeal*. With a body of oatmeal of a foot in diameter, and rather more than an inch deep, he froze a pound and a quarter of water, contained in a hemispherical porous cup. The meal is easily dried and restored again to its action. In a hot climate, the exposure to the sun alone might prove sufficient. By the help of this simple material, therefore, ice will be easily and safely produced in any climate, and even at sea.

Other substances could, no doubt, be employed as absorbents. But, except the muriate of lime, or what is called the *oil of salt* desiccated, none hold out any prospect of success. Dried common salt will barely effect congelation; and stucco, or the sulphate of lime, deprived of its water of crystallization, which might seem to promise a powerful absorption, has scarcely any efficacy whatever.

LXXI. On

LXXI. *On the Height of the Aurora Borealis from the Earth.* *By* James Hoy, *Esq. F.L.S.*

To Mr. Tilloch.

Sir, — Upon reading in your useful Magazine for February last, the Rev. Dr. Maskelyne's "Plan for observing Fire-ball Meteors," republished with Dr. Clarke's Additional Directions, one cannot help regretting that these directions have been so seldom put in practice. The reason, however, is obvious;—these meteors appear so suddenly, move so rapidly, and disappear again so quickly, that they are gone before one can pay such attention to them as to determine any thing almost, except the direction of their motion. The *aurora borealis* is another meteor, of which observations may be more easily made: but as its rays or streams of light change their form and place, for the most part, very quickly too, it is seldom that observers, at a distance from one another, can make a simultaneous observation upon one and the same point of the same ray, when the phænomenon gets the name of *merry dancers.* Sometimes, however, it puts on the appearance of a luminous arch, nearly permanent in the same place, or of such slow motion, that a difference of five or even of ten minutes in the times of observing the altitude of the top of the arch, or what stars it covers and makes a near approach unto, &c. made by persons at a distance from each other, would not much affect the result, in determining the distance of the arch from the earth.

Two luminous arches of this kind have been observed here during the course of a twelvemonth;—the first on March 5, 1817. The exact time of its first appearance was not observed, but at 8^h P.M. it was very brilliant, and of nearly an equal breadth throughout, which breadth filled the space between the stars *Castor* and *Pollux,* the edges of it scarcely extending beyond them. Tracing it westward, its northern edge touched *Aldebaran,* and thence continued in a direct line to the horizon about 20° to the south of west. Its eastern end was as much to the north of east. It narrowed away to points at both ends.

At $8^h\ 32^m$ its south edge passed about one degree above the stars *Regulus* and *Procyon;* and to the westward its breadth filled the space between *Bellatrix* and the foremost star (δ) of *Orion's Belt.* The zenith distance therefore of the highest part of the arch was 45° 30′ to the southward. Its light had then become much fainter, and in five minutes afterwards it disappeared altogether; a considerable time before the moon rose.

This same arch was mentioned in the Edinburgh newspapers to have been observed at Glasgow: *there* at $8^h\ 30^m$, it is said to have

have shone directly over the *Pointers,* and also over the *Plëiades;* the zenith distance of the top of the arch behoved therefore to be 5° or 6° (suppose 5° 30′) to the northward:—let this be added to 45° 30′, the zenith distance here, the sum is 51°. If from this sum we subtract 1° 47′, the difference of latitude between Glasgow 55° 51′, and Gordon Castle 57° 38′, the remainder 49° 13′ is the angle subtended by the distance of Gordon Castle from Glasgow;—this distance by Arrowsmith's Map is about 128 miles.

From these data, calculating by plane trigonometry, the highest part of the arch appears to have been distant from Gordon Castle 168½ miles; and from Glasgow 118½ miles, its least distance from the earth; consequently not quite 118 miles.

Another very luminous arch of this kind appeared here on the 5th of February last, at 8h 20m P.M. When it was first observed, its breadth was about one degree; both ends of it reached nearly to the horizon, a little to the N. of E. and to the S. of W. Its upper edge passed about 5° to the south of the star *Capella,* touching the most southerly of the *Two Kids*. To the westward the arch was parallel to the two brightest stars in *Aries,* and at the distance of its own breadth, or one degree below them. The zenith distance of the top of the arch was consequently about 14° towards the south. At half past eight o'clock, the middle part of the arch and *Capella* were obscured by clouds; the western part had become faint, and a little further separate from the stars in *Aries;* the eastern half was still bright, and had moved considerably northward. At 8h 50m it was all gone.

If any of the numerous readers of The Philosophical Magazine shall have happened to see either of these two phænomena, and to have made similar observations of them, it would be desirable if they would send them to you to be published, that they might either confirm or correct the foregoing result; and afford means for determining whether these arches of the *aurora borealis* are generally at the same distance from the earth; or whether there are not considerable differences in their heights at different times. I am, &c.

Gordon Castle, May 12, 1818. JAMES HOY.

LXXII. *On the Necessity of Animal Decay.*

To Mr. Tilloch.

SIR, — AMONG the reasons usually given for the necessary decay and dissolution of animals and other organized bodies, is there any one that will bear the test of accurate examination?

If the animal system were so contrived that every part might be repaired, why should old age and rigidity supervene? The idea that the sensibility to impressions becomes blunted and jaded by frequent repetitions, is entirely drawn from false analogies. 'Tis true, that the ear may become jaded with hearing music, and the eye with seeing colours; but this is in consequence of the *alteration* which takes place in the state or *set* of the muscular fibres and nerves belonging to those organs, upon being exercised. It is similar to the limbs becoming weary after long-continued walking or riding. Give the muscles time to rest, and if they have received no permanent injury, no loss but what may be repaired,—if their fibres can recover their former *set* and elongation, the same vividness and freshness of sensation will recur as before. The pleasures arising from exercise fade as fatigue comes on; but upon interposing a proper interval, and becoming thoroughly refreshed, the same delight in it is felt as before: nor is it ever found that the exquisiteness and intensity of pleasurable sensations arising from hunting, shooting, music, wine, &c., at all diminish upon repetition, so long as the body retains its full vigour; that is, it is not found that mere repetition has any effect in deadening the sensations.

The illustration from musical strings giving way as they are played upon, proves nothing. The *set* of the particles of matter in the string alters upon continued tension and vibration; and the old state cannot be brought back. If it be said that in like manner the *set* of the muscular fibres, &c. alters and cannot be restored, I agree to the position—that is, I allow that thorough repair does not take place in the animal system. But I ask the cause: I ask whether it would shock our reasoning faculties to learn that in some other world there are organized beings, not subject to decay—in whom nutrition and absorption, &c. restore and replace every thing that is lost or disturbed? If it would not,—if we readily acquiesce in the idea, it is incumbent on us, if we would be truly scientific, to endeavour to point out those peculiarities in *our* system, that preclude the possibility of eternal verdure in the individual.

One of these peculiarities we shall find, I think, in the nature and offices of our vascular system—in the circumstance that *the carriers of matter for repair do not carry matter to repair themselves.* Without pretending to understand those still disputed points concerning the manner of deposition of the coagulable lymph, &c. &c. I think I may venture to affirm that our anatomists do not consider the blood in any artery as being the direct and immediate renovator and repairer of the coats of that artery; every artery and every lymphatic, &c. *down to the most minute,* being considered as having their subsidiary artery attached

tached to them, whose office it is to carry them repairs. Now, unless we suppose the series of branching arteries absolutely infinite, it must stop somewhere; there must be a set of fine carriers, that have no carriers to support and repair them.

The friction, &c. in these fine carrying vessels may be so small as not materially to injure them in fifty years; or in one hundred years; or in one thousand years; but without repair they cannot last for ever. When the time comes that they are no longer serviceable, the next set above them cease to be fed and repaired. When the second set can hold out no longer, the next or third set cease; and so on. That is, *a gradual decay, beginning from the finest and minutest vessels, is the necessary consequence of the repairers in our vascular system not repairing themselves.*

It is not necessary for me here to enlarge upon the correspondence of the phænomena of old age with the natural results of this principle. I leave that to abler hands; contented with pointing out, however inadequately and obscurely, a real cause, hitherto unnoticed I believe, of the fatal necessity of animal decay. I am, sir,

With great respect yours,

May 19, 1818. Z. A.

LXXIII. *On Chemical Philosophy. By Mr.* Matthew Allan, *Lecturer.*

[Continued from p. 352.]

Essay V.

From the foregoing observations the question naturally again presents itself—"What are the properties of this power by which it is deranged in its distribution, or by which it suffers or is capable of this derangement in its distribution, and by which again this deranged balance is restored?" It will be recollected that, in defending the propriety of the generic term, Attractive Agencies, which was proposed, and under which were classed Electricity, Caloric, and Light, I there considered them as mere phænomena arising from the active operations of the power in different states of existence, more or less *combined* with other substances; and yet characterized them as powers still highly attenuated, notwithstanding the idea then thrown out, that the diversity of their appearance arose from holding other substances more or less in solution; and that consequently (as has been already noticed) it was more or less pure, more or less attenuated or condensed, and more or less rapid in its

its movements, had power to pass through substances with more or less facility, and was more or less active or energetic in its actions.

If then we considered attractive agencies, or these unconfinable substances as they are called in some systems, or these diversified appearances produced in this way during the actions of this power, as still highly attenuated in their nature,—how much more so must the pure and uncombined form of this power itself be!—In fact, I conceive we can form no adequate conception of the extreme subtilty of this power in its separate and unmixed state; in its pure state it permeates every substance. That it possesses this extreme tenuity, subtilty* or rarity, will be readily granted, without pressing any further arguments in its defence, or at all bringing forward the host of facts that prove it, or without in this place entering into a comparative estimate of each—of galvanism, electricity, caloric, or light, and of those distinctive characters which have hitherto placed them at such a remote distance from each other, and formed them into separate and distinct branches of science. If then its extreme tenuity be granted, we have one property determined; and another will be still more readily allowed without any arguments to support it, —that it possesses varied degrees of attraction for different substances. That these degrees should differ in different substances, will be afterwards stated and considered; and we shall also show that it is the grand solvent. In short, I shall endeavour to prove how this highly attenuated fluid, by virtue of some of its more evident qualities, must produce certain effects: That its extreme tenuity, its solvent powers, and varied attractive relation to substances, must, in proportion to its quantity and the time taken to act, and to the concentrated nature of these actions, and the solubility of the substances on which it acts, tend more or less to change their state of existence, or carry them in one way and draw them in another: or, in other words, from its solvent or attractive powers, it has the disposition to enter into and adhere to substances, and from its tenuity, and its permeating and irresistible force, to restore every derangement in its due distribution; and hence its movements to the point by which and to which its solvent or attractive powers are demanded and directed: and from this point where it is accumulated the radiation of the excess takes place; two opposite movements are pro-

* It may be conceived that there are too many epithets used: but the fact is, that I now consider it the safer side to err upon; since in laying these ideas before a Society, now some time ago, in a more condensed and abstract form, those who spoke upon the subject appeared to me not to have understood one sentence throughout; as for instance, by the word "rarity" they understood that I meant the scarcity of attraction.

duced;

duced; or one substance is dissolved in it and another precipitated from it, or drawn in this way and carried in another. Hence also it is evident from these properties, that by motion, friction, and alterations of arrangement, either a stream will be induced, or some disturbance in its due diffusion: every stream induced, motion or change must accumulate it on one point and abstract so much from another; all disturbance, all motion, all change, &c. may be so defined (if any definition should regard merely effects), from that which gives motion to the first germ of vegetation, to that which shakes Etna or convulses the atmosphere round the globe. That such mechanical actions should produce these effects, I conceive is capable of demonstration. The wind blows in a certain direction, and the billows partake of the same course. Machinery collects and produces a current of air in the direction of their movements.

From these properties of this power, then, we perceive that it will both by chemical and mechanical means be put in motion; and if this current is demanded in one way rather than another, or if these motions are made in any given and determinate direction,—and all motion must tend to some point;—we find also a current proceeding in a certain direction: and hence on these principles we have the action of the electric machine. Where we contrive to intercept this fluid so put in motion, we there find it accumulated or in excess; and if in this state we can remove the cause which intercepts, or assist its motion by some conducting body, we find it move from this point; and thus we perceive how by mechanical contrivances, its concentration upon and radiation from substances, the point which is in excess is carried by this excess, and the other which is deficient is drawn towards it; and hence we have two apparent currents or movements in two opposite directions, bodies and the particles of bodies moved in one way and drawn in another—two different poles, negative and positive, imparting in one direction and drawing in another: if the rubbing body attracts less than the body rubbed, this body is positive, and the other negative; and the contrary when the circumstances are reversed. Hence "*negative and positive are mere relative states of existence,*"—that positive particles or points which are in excess are carried and draw negative points or particles towards them, or particles which have less than their natural share; and hence, as has already often been repeated, particles unite and separate in the same order under the same circumstances: this order (I shall under Chemical Affinity show) is the mean between the dissimilarity of chemical and the similarity of the physical qualities: the greater the dissimilarity of the chemical, it is evident, greater must be the action—the greater must be the negative and positive state of the particles; and

and consequently the moving and uniting power must be greater. The similarity in the physical qualities, in the gravity and solubility of the particles, will keep them in contact in a proper position for the action arising from the chemical qualities.

It is on these principles that I propose to explain in full detail chemical affinities, &c.: and if this explanation can be disproved, I request it may be shown me. Thus this power carries in one direction and draws in another; and this depends on the qualities of the particles; so that positive and negative, attraction and repulsion, "*are merely relative states of existence*," relatively to each other in excess or defective, and of course the direction of the motion as well as chemical combinations depend on these relative states; and this not merely as it regards particles but stupendous masses of matter, the centripetal and centrifugal forces depending on the same principles.

Thus I infer, from the solvent properties, from the tenuity, and from the effects this power produces, that all movements and changes are the result of this first sublime command or fiat of the Creator:

LET ONE POWER BE DIFFUSED THROUGHOUT THE UNIVERSE, AND LET EVERY KIND AND STATE AND FORM OF MATERIAL EXISTENCE HAVE ITS OWN APPROPRIATE AND RELATIVE SHARE.

This is the UNIVERSAL LAW OF NATURE, which is equally applicable to solar systems as it is to the particles of matter. Whenever this distribution or "appropriate and relative share" is disturbed or accumulated on one point and abstracted from another, this power is at the same instant exerted,—this power of nature, this wonderful agent of creation, instantly obeys this GRAND LAW; the one point as readily parts with its excess as the other receives its deficiency; and this perpetual disturbance produced by its own operations, and this constant exertion to maintain a proper distribution, keeps up all the movements and changes of the universe.

Is it not obvious this must be the case? that this action and reaction, that this disturbance and effort to restore the equilibrium must take place? That if it gives to and preserves matter in its various states and stages and forms of existence, then every change in these forms and states and stages of existence must disturb its due and relative diffusion and distribution,—must be imparted in one way and received in another,—abstracted from this point and demanded by that, by which its circulation and movements and actions in the universe continue their ceaseless and unmeasurable round?

I say, Is it not obvious that this must be the case, if in this subtle, solvent and attractive power, substances are more or less soluble,—have capacity or power to retain or contain it in greater

or

or less quantity; and to conduct or receive and impart it with more or less facility, and are surrounded by other substances of a similar or different nature;—that then these differences in the circumstances and in the kinds of matter must by every alteration in these circumstances, or in the position of the substances, or in the quantity of this power, be deranged in a greater or less degree in its "due and relative diffusion," and of course call this GRAND LAW of nature into action, to restore "this due and relative share?" and hence it is imparted and received from one to another, in greater or less quantity; with more or less intensity, and in a certain and determinate direction.

Its quantity and intensity will depend on the nature and extent of the change which variations in the quantity of this power, in the arrangement of the substances and in the circumstances, produce on different species of matter; for it is given out or *demanded* (and of course received) in proportion to the extent of the change produced.

The imperfect and erroneous views on the doctrine of heat appear to me to have arisen from overlooking the fact,—that this power when demanded flows in from all points to supply this demand, (This explains the operation of freezing mixtures, explosion, &c.) with as much facility and rapidity as it flows out when in excess, or when separated from substances it held in solution, and *whatever* receives or demands it on some point in sufficient quantities produces heat or flame. The intensity of this quantity again depends on its being given out or received on some point of concentration, or in a more or less diffused manner, or in a greater or less degree of purity or separate and unmixed state, which degree of purity or separate and unmixed form is in proportion as substances are more or less soluble in it, and the time it has to effect their dissolution; and of course in proportion to the quantity it holds in solution, it is rendered more or less attenuated or condensed: and hence we find it passing through substances with more or less facility, or with a greater or less degree of rapidity, and in proportion to this resistance of its passage through them, has more or less time to produce effects upon them: and besides, this detention giving it more or less time to produce these effects, in some instances they tend to direct and fix its action, from the disposition these substances held in solution have to combine with others. At other times, from the same cause, but with different substances, it becomes more or less energetic and effectual in its actions, in proportion as it is more or less free or unoccupied to act; so that though the energies and properties of this power appear so various, they are in reality always the same.

Its apparent energies, however, are widely different, owing to these

these circumstances we have enumerated, on its being more or less occupied or free to act; as water already saturated with one salt in solution, is less able to dissolve a salt of another kind: in fact, these observations are equally true of this power in all cases. For instance, a man has power to walk, whether he move or not; but if he carries weight, he can neither walk at the same rate, nor for the same time, nor to the same distance: the power in the abstract is the same, but part of it is occupied in support of this burthen. We express all this in common conversation, when we say, "of what use are strength or talents unless they are exerted? and that they are best shown by undivided exertion." This is true in physiology.—Let the nervous energy be occupied too much in one way, and the functions of some other, if not of every other, part are either diminished or impaired.

Thus its apparent energies and intensity are different, not merely from its being imparted and received in greater or less quantity, and with a greater or less degree of rapidity, and of purity or separated form; but also as it is in a more or less concentrated or diffused manner. All these and many other circumstances attending its action are so infinitely varied in the degree and complexity of their combination, and depend on such an infinite number of causes and circumstances, and all these in themselves so delicate and interwoven with each other,—that it is impossible, and if it were possible would be less profitable, I think, for us to trace them through all their diversity of form and appearance, than the task we have assigned to ourselves, though we intend to support these general principles by descending to the minute detail of facts, which facts in diversity of form and appearance are infinite; and hence one of the in some measure appropriate and descriptive names applied to it, in the mythology of the ancients, was Proteus. Their mythology too, let me here observe, originated in either unnecessarily multiplying the causes of things, or in that of improperly applying names, descriptive of partial effects and peculiarities.

I say, all these and many other circumstances, though they have not hitherto been noticed, not only agree with the facts and phænomena of nature, but point out in the most beautiful manner the causes which vary the effects and appearances of one GRAND AGENT in creation; which point out why it assumes these different forms and produces these different effects;—why chemical attraction and the attraction of gravitation are different; that the one is the particular, and the other the general effects of the same power;—that the one is the local and concentrated, and the other diffused and aggregate effects of its operation;—why in its concentrated actions the heat is sometimes without light; why they generally appear together; why it is electricity

city in one instance and galvanism in another; and what constitutes the apparent difference between them;—why, for instance, galvanism exists in a lower state of intensity than electricity in producing shocks; why it is less rapid in its movements, &c.;—why again its power of producing chemical effects is so much greater than that of electricity*; why again the colorific rays of the prism produce chemical effects, while the calorific scarcely produce any; why they are more refrangible than the heat-making rays? — colour and light depending (as will, I trust, be evident when we come to the particular consideration of light) on the partial solution of substances in this power, and that *light is the phænomenon attending the ignition and solution, or separation and condensation, of matter.* So that heat and light may be contrasted with each other, in the same way as we have hinted above galvanism and electricity may be contrasted. Hence, as in galvanism, the colorific or light-making rays are those which, in consequence of holding something in solution, are in proportion to this, and the unequal thickness or density of substances, as drops of water, cut glass, knots of glass, a glass prism, &c. divided in their passage through them, meeting with a greater or less degree of resistance, according to these variations of thickness and their own variations of tenuity; and besides this, their chemical relations to each other, or the mutual affinities they exert on each other. It is thus these dfferences of refrangibility, reflection, polarization, &c. are produced, and not by differences in the size of the particles.

It will from the same considerations also appear very evident, why I conceive that the heating and melting effects which the disengagement of caloric from one body and its transference to another, or that of mixing bodies of different degrees of temperature together, have not correctly ascertained or measured the relative quantities of caloric, or of the quantity they contain or retain in any state. That, consequently, the tables of capacity are in some instances erroneous; that the heating and melting effects depend not merely on its quantity, but also on the time it has to produce the effect, whether it is retained, or rapidly escapes in its irresistible, attenuated, and unconfinable form.

Thus having stated or anticipated in general terms the more particular explanation of the various effects and phænomena which are produced by one power, it will be necessary that

* See the article *Galvanism*, in the Edinburgh Encyclopædia, where some of the above questions are stated, as questions which require to be answered before we can fully understand the nature of either electricity or galvanism. These questions will, I think, be satisfactorily answered in the next Essay.

I be

I be still more particular; for it is not enough to state in general terms *that changes and phænomena differ not in kind but in degree; not in the power producing them, but in the nature and circumstances of its actions.* It is not enough (until such ideas become familarized) merely to say that, in consequence of the solvent properties which this power has for substances, varying in each species of matter, this variety and these circumstances must modify its actions, and of course occasion effects and phænomena as infinitely varied as these circumstances and substances are themselves. We must be still more minute; for, not duly considering these modifying causes, there have been introduced a number of powers and agencies in creation, to which very opposite and dissimilar properties have been assigned, in order either to account for the differences in the phænomena, or as descriptive of some peculiar effects and appearances themselves, which, though they may have been useful in exciting controversy, stimulating inquiry, and accumulating an immense mass of facts and experiments, have not yet taught us the general principles of science or knowledge, or simplified our views of nature,—the grand aim of philosophy, an aim at once noble, elevating and sublime. So far indeed from this having been the case,—so far from the mode hitherto adopted having led us to "this consummation so devoutly to be wished"—it seems to me to labour mightily for the purpose of preventing such a glorious end. Yet I am convinced, all the phænomena and effects, and even particular anomalies and difficulties which exist in science, according to any other view with which I am acquainted, are not only at once explained on the principles I have stated; but that without them objections and difficulties present themselves every where around me which I cannot remove; so much so, that I know not what more is wanting to add to my conviction, that this theory is copied from the grand book of nature. That I wish this the case I am certain: on all subjects the possession of truth has ever been my object;—let us assist each other in such a pursuit; let us be regardless of the credit of the discovery, while we are anxious for the possession of truth. We must see but one object of pursuit, if we wish light to occupy the regions of intellect, and direct their efforts. We must study nature, and not the fluctuating fashion of opinion, if we expect to arrive at truth. We must not be swayed by the voice of praise or of blame. We must not be deterred by the passions and prejudices of men. We must sacrifice what labour and habit have endeared and entwined round the heart. We must be able to feel, to think, and to stand alone; then, and not till then, shall the reign of truth commence, and banish error and vice from the world.

LXXIV. *On*

LXXIV. *Experiments made in France upon the Use of distilled Sea-Water for domestic Purposes, and its Effects upon the Constitution when taken as a Beverage**.

In consequence of the great want of good fresh water in many of the maritime parts of France, the Government, some time since, ordered some experiments to be made upon an extensive scale, in order to ascertain how far sea-water, when distilled, could be used with success. Little or no use had hitherto been made of water so prepared, except in long voyages, and chiefly then only as a matter of necessity. There are above two hundred leagues of sea-coast in France, where, to the breadth of many miles, the inhabitants are compelled to make use of bad and impure water, which, in many cases, is injurious to the health of themselves and their animals. In similar cases it was the custom of the ancients to construct cisterns; but these are not only expensive in themselves, but their utility depends upon the quantity of rain that falls, while upon the shores of the most barren places nature has supplied a variety of vegetable matter, which, when dried, would not only serve as fuel for the purposes of distillation, but from the ashes of which might be obtained a saline substance, sufficient to repay the expense of collecting, drying, and burning. Thus the fuel for the distillation of the sea-water would in reality cost nothing, while its preparation would employ many individuals, particularly women and children. Before, however, erecting any apparatus for this purpose, it was necessary to ascertain both the utility and salubrity of the water thus prepared.

It is well known that Bougainville, Phipps, Homelin, &c. had employed this water with much success; but they, like most of the chemists of the last age, did not endeavour to imitate the process of nature in all its simplicity, but mixed various substances with the sea-water, in order to take away or lessen the effect of the empyreuma arising from the distillation, and which was so unpleasant to the smell and taste. And it is this which in general renders sailors so averse to it, and excites a prejudice very unfavourable to the salubrity of distilled sea-water. One of the great objects to be ascertained was, whether this disagreeable smell and taste was peculiar to sea-water, or arose from the act of distillation.

In the month of July last year, the king ordered some experiments to be made upon a large scale at the three ports of Brest, Rochefort, and Toulon. The instructions given were as follows:—"That a sufficient quantity of sea-water should be

* From the *Annales de Chimie et de Physique* for Jan. 1818.

distilled to prepare, for the space of a month, bread and other food," for a certain number of criminals who were employed on the works of those ports, and also to supply them with drink, keeping from them during that period every other liquid. Ten or twelve of these persons at each port voluntarily came forward and offered themselves for the experiment.

The persons employed by Government first distilled a sufficient quantity of sea-water, without the admixture of any other substance; this produce dissolved soap, dressed vegetables, produced the same appearances with the aërometer as that distilled from spring water. There was no difference between the one and the other. But the distilled sea-water had always that empyreumatic smell and taste of which we have before spoken; and it was so strong, that the Commission at Toulon called it *Odeur de marine*, and that of Rochefort, *Odeur de marecage*. But this is not peculiar to sea-water, for the result of a distillation of fresh water has always the same taste and smell.

Neither of these liquids immediately loses this by being filtered through charcoal; but by being exposed for some time to the air, the distilled sea-water loses this unpleasant quality, and then it does not differ from fresh water derived from the purest source, and both have equally stood every chemical test to which they have been exposed. The chemical properties of this water having thus been determined, it remains to give an account of the effects upon the individuals who underwent the experiment. These are the principal results:

Brest.—During the first days those who drank the water complained of a weight upon the stomach. This indisposition, which was the only one they experienced, soon decreased upon taking exercise, and totally went off by an additional ounce of biscuit added to their common ration. One of them on the twenty-ninth day had a few symptoms, but which he himself attributed to an indigestion, from some bacon he had eaten. Eight individuals drank twenty-five pints a-day, rather more than three pints each. [N. B. The French pint contains very near fifty-seven cubic inches English measure, and is the regulation size for the claret or Bordeaux bottle; but in general the bottles are rather smaller. The French pint, therefore, is equal to rather more than 19-20ths of an English quart, wine measure.]

Toulon.—The results obtained at the Arsenal of this town were not less decisive or satisfactory. The six persons who made the experiment acquired a greater degree of freshness in their appearance, and were much fatter. Their daily consumption of distilled water was nine pounds (*poids de marc*) for drink, and eleven

eleven pounds for cooking. This is nearly the same relative quantity as those at Brest.

ROCHEFORT.—The experiments here have not been made with the same regularity; because the fifteen persons fixed upon had all agreed among themselves to say that they were very ill. The two principal ones complained of violent colics and diarrhœas; but the plot was discovered, and upon being put upon the sick list (*à la diète*) they were laughed at by their companions. No one of them was really indisposed; on the contrary, many thought they experienced some good effect in regard to some infirmities under which they had long laboured.

The above are not, however, the only experiments which have been made upon this beverage. Several persons wishing to ascertain its effects by individual experience, have voluntarily confined themselves to its use; and the members of the Commission of Inquiry are almost in the daily practice of taking it. The captain of the Duclat has taken it every day at his meals for twenty days, and has experienced not the smallest inconvenience from its use. MM. Vasse and Chatelain, apothecaries to the marine at Brest, have occasionally kept the water in their mouths for four hours by constantly renewing it, and have not found either the sharp taste, or other caustic qualities, which have been said to be peculiar to it: and here it may be proper to state, that the mouths of all the individuals who had taken the water for a length of time were examined, without the detection of any thing in them either of a swollen or inflammatory appearance.

Such are the reports of commissioners employed to investigate the effects of distilled sea-water; who, although separated at a great distance from each other, and having no communication, all agree in the inference, that it may be employed without any injury to the health, both as a beverage and in cookery, for the space of at least a month; and the fair presumption is, that it may be so employed for a much longer time; and that in consequence it must be considered as a very happy resource in long voyages, especially in voyages of discovery.

LXXV. *On the Dry-Rot.* *By Mr.* GAVIN INGLIS.

To Mr. Tilloch.

April 29, 1818.

SIR,—EVERY lover of his country must learn with peculiar anxiety and regret the alarming ravages committed by the dry-rot in that hitherto invulnerable bulwark of our happy nation "the wooden walls of Old England." The state of the Eden sloop of war, the Topaze frigate, &c. as mentioned in the Philosophical Magazine for last month, cannot be viewed with indifference; and the sum of 5000*l.* paid by Government to Dr. Lukin for his ineffectual labours in endeavouring to stop the progress of this most destructive malady, is an incontrovertible proof of the anxious attention of those in power to this indispensable department of our national defence. The statement alluded to in a matter of such importance to the country, I should suppose alone sufficient to rouse the efforts of every scientific individual in the kingdom to stay the effects of a baneful canker, that may one day leave Government deficient in means of chastizing an insolent foe, and which must damp the spirit of enterprise, and ruin the prosperity and political influence of our envied isle.

The plan I have to propose for consideration for the cure of the dry-rot in those ships already infected, the prevention of its effects in those that are unsuspected, and the preparation of the timber for those building, or to be built, is not the effusion of idle speculation, but the result of much deliberate thought and observation, and very simple in its application.

In the first place allow me to premise, that all timber destined for ship-building, particularly for the Navy, where strength and durability are so essentially necessary, ought never to be cut till after the fall of the leaf. At this stage, Nature has done her work for the season; the efforts of vegetation are gone to rest, and all the juices of the plant fully ripened and completely lignated. Then, and only then, should all navy timber be cut down. But so soon as the roots begin to absorb the new sap, cease felling till next fall. By the time the buds begin to swell, the new and unlignated sap has reached the most remote twig, and the trunk and branches of the tree are surcharged with fresh juice, and every pore replete with vegetable life. Timber cut in this state, or in leaf, or in flower, must be found perfectly full of new blood, and in the very strength of vegetation; consequently completely loaded with the germs of future corruption. This is the fundamental basis of the dry-rot. The vegetative principle does not expire with the stroke of the axe, nor the rag of the saw, but will, uncorrected, still exist, and in time produce in every pore innumerable fungi, and render even the hardest portion of the plank food for worms. In examining masses of oak dug from the alluvial strata of the country, where it has lain for ages unknown, many of them are found fresh and sound as the day in which they had been torn from their respective roots. But wherever this has been found the case, the timber is uniformly black as ebony and obdurately hard. I was led from curiosity to examine several of these old trunks; and searching minutely for the cause of this preservation in the effect, and to satisfy myself whether

whether the oak had attained this colour merely from age, or owed its preservation to the cause that produced the black colour, I chemically examined different specimens, and found in that investigation a far greater proportion of iron than could possibly be supposed to exist in the natural state of the tree. To this iron I attribute the incorruptibility and high state of preservation in which this antediluvian timber is found. This extraneous iron must have been supplied from the ore of the soil, or chalybeate waters. In this state of solution the iron would penetrate the substance of the wood, unite with the astringent principle, and give it not only the black colour, but communicate such a density of texture as almost to resist the sharpest instrument.

I have no wish to trespass with multitudinous remarks, nor to fill pages with speculative theory. Allow me to express it as my opinion, that the most effectual preventive of the dry-rot would be to cut all navy timber at the season before stated, and treat it as after mentioned. But should this period of cutting be conceived incompatible with that important branch of national industry the leather trade, the same means will season the new timber, and render it proof against the dry-rot, that will cure it in the old, viz. by an application of iron in a state of solution. This can be obtained at comparatively little or no expense from a solution of green copperas, and will be found a most effectual remedy, either as cure or preventive of that most destructive canker.

New timber, previous to its being marshallated, should be cut up into all the various forms which the ship-carpenter may require, and allowed a time to dry. Then soak every chip to be used in the navy in a well prepared solution, till it has acquired the colour of new ink. This would completely counteract every vegetative principle, and communicate that incorruptible durability and firmness of texture, so essentially requisite in navy timber, by the chemical combinations already described; with this addition, that the sulphur of the solution, penetrating the substance of the plank, would defend the vessel against the ravages of insects.

These are the grounds, outlines, and principles, of a plan, that, if properly conducted, will, in my opinion, for ever banish all rot and canker from the British navy.

G. INGLIS.

LXXVI. *On the Electric Properties of Metals, and the absolute Positive and Negative Electric Powers of various Substances.* By Mr. J. Tatum.

To Mr. Tilloch.

Sir, — Should you think the account of the following experiments calculated to remove the erroneous but widely disseminated opinion of the *non*-electric properties of *metals;* and also, of the *absolute* positive and negative electric powers residing in peculiar substances, they are very much at your service.

To prove that *metals* possess *all* the electric properties with which vitreous and resinous bodies are endowed, nothing but friction is necessary, when their positive and negative properties will be elicited, and to as great an extent as is produced by the above bodies.

Exp. 1. Let a plate of zinc (of about two inches diameter) provided with an insulating handle rising from its centre, be quickly drawn over a piece of silk, and on presenting it to an electrometer, it will be found to produce strong signs of *positive* electricity.

Exp. 2. But if a plate of silver of the same size be subject to friction on silk, (or, what is better, on fur, for then I have had it produce a spark which could be both heard and seen,) it will produce signs of *negative* electricity.

From these experiments, we see that both positive and negative electricity are produced from *metallic* bodies, and by the same means with which they are produced from vitreous and resinous substances.

That positive and negative electricity is not peculiar to any particular body, but depends on the greater or less attraction which the rubber possesses for the electric fluid, will, I think, be evident from the following experiments.

Exp. 3. If in the place of *silk*, on which the plate of zinc was rubbed, we substitute *fur*, the plate of zinc will now give signs of *negative* electricity.

Exp. 4. And if we draw white silk over black silk, we shall find the white betray signs of *positive* electricity, and the black negative electricity.

Exp. 5. But if the same *white silk* be drawn over fur, it will now give signs of *negative* electricity.

Exp. 6. If yellow silk be drawn over the white silk, the yellow will be *negatively* electrified, and the white positively electrified.

Exp. 7. But if the *yellow* be drawn over the black, the yellow will now become *positively* electrified.

From

From the above experiments, we see that the same body may be made to produce either positive or negative electricity at pleasure; and that *metallic* bodies, like vitreous and resinous ones, will by similar treatment, like them, produce similar states of electricity.

From which I think it is but fair to conclude, that the classing of bodies into electrics and *non*-electrics, as well as into positive and negative electrics, is founded in error, and at variance with experiments.

P.S.—The silk which I used was wrapped several times round a thin board about one foot three inches long, and five inches wide, with a handle similar to a battledore.

I am, sir, yours, &c.

Dorset-treet, June 14, 1818. J. TATUM.

LXXVII. *Process practised in the Establishment of Syoise sur Seine to extract Vinegar or Acetic Acid from Wood. By M.* P. L. DUPUYTREN*.

AT one end of a large building calculated for the purpose, are four furnaces, adapted to receive large retorts, the lower parts of which are made in cast iron, and the rest in strong iron plates: at a small distance from the bottom of these retorts is the opening of a copper pipe of three inches diameter, which rises through the metal of the retort, and widens like a tunnel at the upper end; a copper cylinder eight or nine inches wide, and eighteen or twenty long, is fixed to this tunnel, passing out of the building, is bent downwards, and is plunged into a large tub full of water, which is constantly renewed: from this it discharges itself into a condenser, to which are adapted on one side a small cock to carry off liquids, and on the other a cylinder of about the same bore as the one mentioned above, and which rises vertically, then turns down and enters into the building, where it is again bent, and opens at the hearth of the furnace.

This apparatus being arranged, the retorts are filled with pieces of wood which have been cut a year, and which must be chosen straight and long, and about as large as the wrist; these pieces are arranged in the retort with order. When the retort is full, it is covered with its lid, which is fastened on by means of screws; it is then luted carefully with clay, and by means of a crane two men raise it up and place it in its furnace: over this is put a covering of masonry of considerable weight; the cylinder is fitted to the retort, and the fire is lighted.

* From the *Journal de Pharmacie.*

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The water which is in the wood soon dissipates, and the carbonization begins. Then there is disengaged a good deal of carbonic acid; acetic acid combined with water; carbonated hydrogen; and an oily matter similar to tar, with perhaps a small portion of carbonic acid gas.

From part of the retort in which the decomposition is made, all these matters are forced to pass through the entire mass of wood to get to the opening of the pipe before mentioned, which is purposely placed at the inferior extremity; by this pipe they enter into the copper cylinder which conveys them to the condenser; there almost all that is water, the acetic acid and oily matter become condensed, and run off through the little cock; while all the carbonic acid, the carbonated hydrogen gas, carbonic acid gas, which also carry with them a small quantity of the other products, rise up by the other cylinder, and go into the fire, where they serve as fuel.

When this operation has continued five hours, these inflammable vapours are directed, by means of a cock, under the fire of another retort, just lighted; the heat of the first furnace, and that which escapes from the wood during its decomposition, being sufficient to complete the carbonization of all which is contained in it, without the aid of the combustion of the gas. It is not necessary even to wait till the evaporation of these vapours has ceased, before the tube is removed from the ash-pit of the first furnace, because the charcoal would be too weak.

As soon as the neighbouring retort begins to give out its gaseous products, and can do without further assistance, the pipe is removed from the ash-pit, and the remaining gas which comes out is ignited, in order to prevent the disagreeable smell arising from it. The flame thus produced is as large as the pipe, rises many feet above the pipe, and lasts about half an hour. The moment the retort is removed, it is replaced by another, and the same process is observed as with the former.

Caution however is required in this process; for the moment the retort is taken out of the furnace, the copper cylinder is filled with inflammable gas: if it is joined and luted immediately to that which succeeds it, the gas will mix with the air contained in it, and the very smallest spark that could possibly penetrate the fissures of the retort would occasion an explosion; for which reason the apparatus is never to be luted till the very instant of the appearance of the empyreumatic vapour.

The retorts are from about seventy-two to a hundred cubic feet in capacity. They contain one and a half to two loads of wood, which, when it is well chosen and of a good quality, will yield twenty-eight per cent. of charcoal, and yield two hundred

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and forty to three hundred litres of pyroligneous acid, containing one-twelfth of tar.

The charcoal retains the form of the wood; it is intermixed only with a small quantity of dust which proceeds from the bark; it possesses all the qualities of good charcoal, but its combustion is more rapid and more brilliant, and less is required for raising liquors to boiling heat. If exposed in contact with the air, it gains ten per cent. in weight.

The most satisfactory results are obtained from hard woods; but white woods are rejected: it takes five or six hours to char the wood, and seven hours to cool the charcoal.

When the retort is taken out, the pyroligneous acid is a reddish, semi-transparent liquid, and of a strong empyreumatic acid taste and smell; every hundred parts will saturate as much subcarbonate of alkali as seven and a half to nine and a half of sulphuric acid will concentrate.

The liquid flows from the condenser in a continual stream, of the size of a goose quill, and is conducted into a large wooden cask placed in a cellar, where, in cooling, it deposits the greater part of its tar; hence it passes into another cask, where it remains in store.

It is preferable in this state to vinegar for all kinds of dyeing and printing on linen; it possesses an oil which is an excellent mordant for linen and cotton, and prints with a more brilliant, durable, and fine colour. It likewise serves to give a rose-colour to woods, feathers, and straw, &c.

In order to separate the acid from the empyreumatic oil, which colours and changes its nature, it is run into a large iron boiler, where as much subcarbonate of lime is added as it can decompose when cold. When arrived at this point, a certain quantity of the tar which floats on the surface is taken off with a skimmer, and by means of a pump the liquor is raised up in a cauldron, where it is then boiled. The saturation is then continued with quick-lime, and decomposes the acetate of alkali which remains in dissolution; and the sulphate of lime which precipitates, carries with it a fresh quantity of tar.

The deposit being accomplished, the liquor is passed into another cauldron, and there it is concentrated by a slight boiling till a thin skin arises; then it is put into wooden tubs, where in cooling it becomes solid.

The produce thus obtained is extremely impure and black, in consequence of its retaining some portion of oily matter. This foreign substance resists repeated crystallization, and cannot be taken away except by melting. These impure crystals are therefore put into a cast-iron cauldron, where they undergo the

aqueous

aqueous fusion; all the water that they contain is evaporated; and when they are dry, the fire is increased till all the matter is in a burning fusion. It is then run into appropriate squares, in which it becomes solid.

In this state it is black like coal, but it easily dissolves in warm water; and this solution filtered and stirred with care gives crystals of acetate of alkali, which retain scarcely any of the empyreuma. It is then melted in a certain large quantity of water, and is decomposed by means of sulphuric acid of commerce; it then gives out sulphate of alkali crystallized, and acetic acid, which only remains to be distilled to be obtained in perfect purity. The process may be carried on in large stone pitchers, called tourilles. While the burning fusion is going on, only a small quantity of acetate of alkali is decomposed, which depends probably on the presence of the oily matter.

This acetic acid, thus rectified, shows eleven degrees on the hydrometer of Baumé. It is to be preferred to distilled vinegar, as the latter is never so concentrated, and always retains some portion of vegetable matter which injures the beauty of the fabrics; besides which, for all preparations to be done with the acetic acid of wood, it does not require to be purified.

In order to concentrate this acid to a proper state for crystallization, it is sufficient to combine it with acetate of lime, and to decompose this salt slightly calcined, by concentrated sulphuric acid. At the instant of contact the reaction is extremely rapid, and the acetic acid is disengaged, giving up to the sulphate of lime all the superfluous water.

The waters from the first and second crystallization of the acetate of alkali are no longer susceptible of giving fresh crystals by evaporation. As that, no doubt, depends on the too great quantity of oily matter which is in it, it is probable that by calcining them alone, or with a mixture of charcoal powder, or perhaps by making them boil with charcoal, they might again acquire the properties of crystallization.

Being evaporated to dryness, they are mixed with tar, and then serve as fuel to heat the cauldrons. But as the cinders from them are not quite deprived of the acetate of alkali, they are passed through a reverberating furnace, washed in lye, and by the second crystallization the finest subcarbonate of alkali is obtained.

LXXVIII. *On a new and expeditious Method of naming at sight the Roots of complete Cubes under Ten Figures.* By John Evans *Jun. A.M.*

Sir, — In a former volume of your valuable Miscellany, I recollect having met with some communications, in which is given a method of naming at sight the root of any complete cube number under seven figures. The principle was briefly this:—The root will evidently not exceed two figures;—of which the first, or digit of tens, will be the root of the greatest cube contained in the 4th, 5th, and 6th figures of the given number, counting from the right hand:—and the latter, or *terminating* figure of the root, will be found by considering that if the cube terminate in 1, 4, 5, 6, or 9, the root will terminate in the same figure; if in 2, 3, 7, or 8, the termination of the root will be 8, 7, 3, or 2, the respective deficiencies from 10.

Now, sir, it has occurred to me, that by uniting another principle with the method just alluded to, we may name at sight, if required, the root of any cube number not exceeding nine figures. This is effected by the following rules:

1. It is obvious that the required root will never exceed three figures:—the *first* of which will be the root of the greatest cube contained in the 7th, 8th, and 9th figures counting from the right.

2. The *terminating* figure of the root may be found as before from the termination of the given cube.

3. To obtain the *middle* figure of the root, divide the given cube by 11, and according as the remainder is,

0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10,
take 0, 1, 7, 9, 5, 3, 8, 6, 2, 4, or 10;

which being subtracted from the *sum* of the *first* and *last* figures (found as above) borrowing 11, if necessary, will give the *middle* figure of the root.

Take as an example the complete cube 504358336.

1. The greatest cube in 504, the 7th, 8th, and 9th figures, is 343, or $(7)^3$: therefore the *first* figure of the root is 7.

2. Since the cube ends in 6, the *termination* of the root is 6.

3. Dividing the given cube by 11, the remainder is 9, to which 4 corresponds in the series given above. Then the two former figures 7 and 6 added together give 13, from which 4 being deducted, leaves 9 for the *middle* figure of the root. Hence the root required is 796.

Again, let the proposed cube be 6028568.

1. The root of the greatest cube in 6 is 1.

2. The termination will be $10 - 8 = 2$.

3. The

3. The remainder, after dividing by 11, is 7, whose corresponding number 6 being subtracted from $2 + 1 + 11$, or 14, leaves 8 for the middle figure of the root, which is therefore 182.

The principles on which are founded the rules for obtaining the *first* and *last* figures of the root, are perhaps sufficiently evident. The rule, however, for the *middle* figure, on which alone I rest my claim for originality in the present instance, depends upon the following obvious property. If a given number divided by 11 leave a certain remainder r, and the cube of r be divided by 11, the new remainder r' left after this second division will be the same as what would remain, if the cube of the original number were divided by 11; and further, if r be 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10, it will be found upon trial that r' will be 0, 1, 8, 5, 9, 4, 7, 2, 6, 3, or 10: or inversely, if r' be 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10, r will be 0, 1, 7, 9, 5, 3, 8, 6, 2, 4, or 10. Consequently since r' is obtained by dividing the proposed cube by 11, r may be found by the series; *i. e.* the remainder that should result from the division of the *root* by 11. But it is well known that if the remainder arising from casting the elevens out of the *even* digits of any given number, be subtracted from the remainder arising from casting the elevens out of the *odd* digits, the resulting remainder is what would remain, if the given number were divided by 11. Hence if r be subtracted from the *sum* of the first and last figures already found, borrowing 11, if necessary, it is obvious that the difference will be the middle figure of the root.

If, sir, the method I have just described, which is, to the best of my knowledge, entirely *new* as respects its *extension* to cubes above six figures, and which certainly far surpasses the common method in conciseness and facility of application, be deemed by you worthy of a place in your Magazine, your insertion of it will oblige yours, &c.

Islington, June 16, 1818. JOHN EVANS Jun.

P. S.—Before I conclude this communication, allow me just to hint, that in the rule I have suggested, the method of *casting out elevens* may be sometimes conveniently substituted for the division by 11.

It may likewise not be improper to remark that the series 0, 1, 7, 9, 5, 3, 8, 6, 2, 4, 10, may be easily and firmly fixed in the memory, by observing that if we omit the 0, the sum of any two terms equally distant from the extremes is always 11; thus $1 + 10 = 7 + 4$ &c. $= 11$: also that the first five consist of all the *odd* numbers, the last five of all the *even* numbers under 11: and lastly, that the *differences* of the series are in the following regular order, 6, 2, −4, −2, 5, −2, −4, 2, 6.

LXXIX. *His-*

LXXIX. *History of Dr.* Brewster's *Kaleidoscope, with Remarks on its supposed Resemblance to other Combinations of plain Mirrors.*

In our last Number we laid before our readers a few observations on this instrument, the result of which was, that the kaleidoscope is in truth a new invention, unknown to those from whom some have affected to consider Dr. Brewster as having borrowed it. As the instrument excites universal attention, a brief sketch of the history of the invention, for which we are indebted to a friend at Edinburgh, cannot but prove acceptable to our readers.

In the year 1814, when Dr. Brewster was engaged in experiments on the polarization of light by successive reflections between plates of glass, which were published in the Philosophical Transactions for 1815, and honoured by the Royal Society of London with the Copley Medal, the reflectors were in some cases inclined to each other, and he had occasion to remark the circular arrangement of the images of a candle round a centre, or the multiplication of the sectors formed by the extremities of the glass plates. In repeating, at a subsequent period, the experiments of M. Biot on the action of fluids upon light, Dr. B. placed the fluids in a trough formed by two plates of glass cemented together at an angle. The eye being necessarily placed at one end, some of the cement which had been pressed through between the plates appeared to be arranged into a regular figure. The symmetry of this figure being very remarkable, Dr. B. set himself to investigate the cause of the phænomenon, and in doing this he discovered the leading principles of the kaleidoscope. He found that, in order to produce perfectly beautiful and symmetrical forms, three conditions were necessary.

1. That the reflectors should be placed at an angle, which was an *even* or an *odd* aliquot part of a circle, when the object was regular; or the *even* aliquot part of a circle when the object was irregular.

2. That out of an infinite number of positions for the object both within and without the reflectors, there was *only one* position where perfect symmetry could be obtained, namely, by placing the object in contact with the ends of the reflectors.

3. That out of an infinite number of positions of the eye, there was *only one* where the symmetry was perfect, namely, as near as possible to the angular point, so that the circular field could be distinctly seen; and that this point was the *only one* out of an infinite number at which the uniformity of the light of the circular field was a maximum.

Upon

Upon these principles Dr. B. constructed an instrument, in which he fixed *permanently* across the ends of reflectors, pieces of coloured glass, and other irregular objects, and he showed the instrument in this state to some members of the Royal Society of Edinburgh, who were much struck with the beauty of its effects. In this case, however, the forms were nearly permanent, and a slight variation was produced by varying the position of the instrument, with respect to the light. The great step, however, towards the completion of the instrument remained yet to be made, and it was not till some time afterwards that the idea occurred to Dr. B. *of giving motion to objects, such as pieces of coloured glass, &c. which were either fixed or placed loosely in a cell at the end of the instrument.* When this idea was carried into execution, the kaleidoscope, in its *simple form,* was completed.

In this state, however, the kaleidoscope could not be considered as a general philosophical instrument of universal application; for it was incapable of producing beautiful forms unless the object was nearly in perfect contact with the end of the reflectors.

The next, and by far the most important step of the invention was therefore to remove this limitation by employing a draw tube and lens, by means of which beautiful forms could be created from objects of all sizes, and at all distances from the observer. In this way the power of the kaleidoscope was indefinitely extended, and every object in nature could be introduced into the picture, in the same manner as if these objects had been reduced in size, and actually placed at the end of the reflectors.

When the instrument was brought to this state of perfection, Dr. Brewster was urged by his friends to secure the exclusive property of it by a patent, and he accordingly took out a patent for "a New Optical Instrument for creating and exhibiting beautiful forms." In the specification of his patent he describes the kaleidoscope in two different forms. The first consists of two reflecting planes, put together according to the principles already described, and placed in a tube, with an eye-hole in the particular position which gives symmetry and a maximum uniformity of light, and with objects, such as coloured glass, *placed in the position of symmetry, and put in motion either by a rotatory movement, or by their own gravity, or by both combined.* The second form of the instrument, described in the specification, is, when the tube containing the reflectors is placed in a second tube, at the end of which is a convex lens which introduces into the picture objects of all magnitudes, and at every distance, as has been already described.

After the patent was signed, and the instruments in a state of forwardness,

forwardness, the gentleman who was employed to manufacture them under the patent, carried a kaleidoscope to show to the principal London opticians, for the purpose of taking orders from them. These gentlemen naturally made one for their own use, and for the amusement of their friends; and the character of the instrument being thus made public, the tinmen and glaziers began to manufacture the detached parts of it, in order to evade the patent; while others manufactured and sold the instrument complete, without being aware that the exclusive property of it had been secured by a patent.

In this way the invasion of the patent right became general among that class of individuals against whom the law is seldom enforced but in its terrors. Some workmen of a higher class were encouraged to piracy by this universal opposition to the patent; but none of the respectable London opticians would yield to the clamours of their customers, to encroach upon the rights of an inventor, to whom they were at least indebted for a new and a lucrative article of trade.

In order to justify these piratical proceedings, it became necessary to search out some combinations of plain mirrors, which might be supposed to have some resemblance to Dr. Brewster's instrument; and it would have been strange indeed, if some theorem or experiment had not been discovered, which could have been used to impose upon the great crowd, who are entirely ignorant of the principles and construction of optical instruments. There never was a popular invention, which the labours of envious individuals did not attempt to trace to some remote period; and in the present case, so many persons had hazarded their fortunes and their characters, that it became necessary to lay hold of something which could be construed into an anticipation of the kaleidoscope.

The first supposed anticipation of the kaleidoscope was found in Prop. xiii. and xiv. of Professor Wood's Optics, where that learned author gives a mathematical investigation of the number and arrangement of the images formed by two reflectors, either inclined or parallel to each other. This theorem assigns no position either to the eye or to the object, and does not even include the principle of inversion, which is absolutely necessary to the production of symmetrical forms. The theorem is true, whatever be the position of the object or of the eye. In order to put this matter to rest, Dr. Brewster wrote a letter to Professor Wood, requesting him to say if he had any idea of the effects of the kaleidoscope when he wrote those propositions. To this letter Dr. B. received the following handsome and satisfactory answer:

"St. John's,

"St. John's, May 19, 1818.

"Sir,—The propositions I have given relating to the number of images formed by plane reflectors inclined to each other, contain merely the mathematical calculation of their number and arrangement. *The effects produced by the kaleidoscope were never in my contemplation.* My attention has for some years been turned to other subjects, and I regret that I have not time to read your Optical Treatise, which I am sure would give me great pleasure. I am, sir,

"Your obedient humble servant,

"J. Wood."

The next supposed anticipation of the kaleidoscope was an instrument proposed by Mr. Bradley in 1717. This instrument consists of two large pieces of silvered looking-glass, *five inches wide* and *four inches high*, jointed together with hinges, and opening like a book. These plates being set upon a geometrical drawing, and the eye being placed in front of the mirrors, the lines of the drawing were seen multiplied by repeated reflections. This instrument was described long before by Kircher, and did not receive a single improvement from the hands of Bradley. It has been often made by the opticians, and was principally used for multiplying the human face, when placed between the mirrors; but no person ever thought of applying it to any purpose of utility, or of using it as an instrument of rational amusement, by the creation of beautiful forms. From the very construction of the instrument, indeed, it is quite incapable of producing any of the singular effects exhibited by the kaleidoscope. It gives, indeed, a series of reflected images arranged round a centre; but so does a pair of looking-glasses placed angularly in an apartment, and so do the pieces of mirror glass with which jewellers multiply the wares exhibited at their windows. It might therefore be as gravely maintained that any of these combinations of mirrors was a kaleidoscope, as that Bradley's pair of plates was an anticipation of that instrument. As the similarity between the two has been maintained by ignorant and interested individuals, we shall be at some pains to explain to the reader the differences between these two instruments; and we shall do this, first, upon the supposition that the two instruments are applied to geometric lines upon paper.

1. In Bradley's instrument, the length is less than the breadth of the plates.	1. In the kaleidoscope, the length of the plates must be four, or five, or six times their breadth.
2. Bradley's instrument cannot be used with a tube.	2. The kaleidoscope cannot be used without a tube.
	3. In

3. In Bradley's instrument, from the erroneous position of the eye, there is a great inequality of light in the sectors, and the last sectors are scarcely visible.	3. In the kaleidoscope, the eye is placed so that the uniformity of light is a maximum, and the last sectors are distinctly visible.
4. In Bradley's instrument, the figure consists of elliptical, and consequently unequal sectors.	4. In the kaleidoscope, all the sectors are equal, and compose a perfect circle, and the picture is perfectly symmetrical.
5. In Bradley's instrument, the unequal sectors *do not unite*, but are all separated from one another by a space equal to the thickness of the mirror glass.	5. In the kaleidoscope, the equal sectors all unite into a complete and perfectly symmetrical figure.
6. In Bradley's instrument, the images reflected from the first surface interfere with those reflected from the second, and produce a confusion and overlapping of images entirely inconsistent with symmetry.	6. In the kaleidoscope, the secondary reflections are entirely removed, and therefore no confusion takes place.
7. In Bradley's instrument, the defects in the junction of the plates are all rendered visible by the erroneous position of the eye.	7. In the kaleidoscope, the eye is placed so that these defects of junction are invisible.

The reader will observe, that in this comparison the two instruments are supposed to be applied to *geometric lines upon paper*, and that this was *the only purpose* to which Bradley ever thought of applying his mirrors; yet the kaleidoscope is in every respect a superior instrument, even for that inferior purpose, and gives true symmetrical forms, which the other instrument is incapable of doing.

In the comparison which has now been made, we have degraded the kaleidoscope, by contrasting its effects with those which Bradley's instrument is capable of producing, for these effects are not worth the looking at. When we attempt to employ Bradley's instrument to produce the effects which have been so much admired in the kaleidoscope, namely, to produce beautiful forms from transparent or opaque coloured objects contained in a cell, and at the end of the reflectors, it fails so entirely, that no person has succeeded in the attempt. It is indeed quite impossible to produce by it the beautiful and symmetrical forms which the kaleidoscope displays. Had this been possible, Dr. Brewster's patent might have been invaded with

 impunity

impunity by every person who chose to manufacture Bradley's instrument; but this was never tried*, and for the best of all reasons, because nobody would have purchased it.

We trust that no person, who wishes to judge of this subject with candour, will form an opinion without having *actually seen and used* the instrument proposed by Bradley. Let any person take Bradley's plates, and, having set them at an angle of 30° or $22\frac{1}{2}$°, place them upon a cell containing fragments of coloured glass, he will infallibly find that he cannot produce a picture of any symmetry or beauty. The disunion of the sectors, the darkness of the last reflections, and the enormous deviation from symmetry, towards the centre of the figure, will convince him, if he required conviction, that the instrument is entirely useless as a kaleidoscope. To those, however, who are are not capable, either for want of knowledge, or want of time, to make such a comparison, we may present the opinion of three of the most eminent natural philosophers of the present day, viz. the celebrated Mr. Watt, Professor Playfair, and Professor Pictet.

"It has been said here," says Mr. Watt, "that you took the idea of the kaleidoscope from an old book on gardening. My friend the Rev. Mr. Corrie has procured me a sight of the book. It is Bradley's Improvements of Planting and Gardening. London 1731, part 2d, chap. 1st. It consists of two pieces of looking-glass of equal bigness, of the figure of a long square, five inches long and four inches high, hinged together, upon one of the narrow sides, so as to open and shut like the leaves of a book, which, being set upon their edges upon a drawing, will show it multiplied by repeated reflections. This instrument I have seen in my father's possession seventy years ago, and frequently since, but what has become of it I know not. In my opinion, the application of the principle is very different from that of your kaleidoscope."

The following is Professor Playfair's opinion:

"Edinburgh, May 11, 1818.

"I have examined the kaleidoscope invented by Dr. Brewster, and compared it with the description of an instrument which it has been said to resemble, constructed by Bradley in 1717. I have also compared its effect with an experiment to which it may be thought to have some analogy, described by Mr. Wood in his Optics, Prop. 13 and 14.

* In illustration of this argument, we may state the following fact: Mr. Carpenter of Birmingham, being anxious to evade Dr. Brewster's patent, at a time when the manufacture of the patent kaleidoscope was in the hands of another person, attempted to construct instruments in imitation of Bradley's. After exercising his ingenuity for some time, he abandoned the attempt as impracticable, and set off for Scotland for the purpose of offering his services in manufacturing the patent instrument.

"From

"From both these contrivances, and from every optical instrument with which I am acquainted, the kaleidoscope appears to differ essentially both in its effect and in the principles of its construction.

"As to the effect, the thing produced by the kaleidoscope is a series of figures presented with the most perfect symmetry, so as always to compose a whole, in which nothing is wanting and nothing redundant. It matters not what the object be to which the instrument is directed: if it only be in its proper place, the effect just described is sure to take place, and with an endless variety. In this respect, the kaleidoscope appears to be quite singular among optical instruments. Neither the instrument of Bradley, nor the experiment or theorem in Wood's book, have any resemblance to this; they go no further than the multiplication of the figure.

"Next, as to the principle of construction, Dr. Brewster's instrument requires *a particular position of the eye of the observer, and of the object looked at,* in order to its effect. If either of these is wanting, the symmetry vanishes, and the figures are irregular and disunited. In the other two cases, no particular position, either for the eye or the object, is required.

"For these reasons, Dr. Brewster's invention seems to me quite unlike the other two. Indeed, as far as I know, it is quite singular among optical instruments; and it will be matter of sincere regret, if any imaginary or vague analogy between it and other optical instruments, should be the means of depriving the Doctor of any part of the reward to which his skill, ingenuity, and perseverance, entitle him so well.

"JOHN PLAYFAIR,
"Professor of Natural Philosophy in the University of Edinburgh.

"P. S.—Granting that there were a resemblance between the kaleidoscope and Bradley's instrument, in any of the particulars mentioned above, the introduction of coloured and moveable objects, at the end of the reflectors, is quite peculiar to Dr. Brewster's instrument. Besides this, a circumstance highly deserving of attention, is the use of two lenses and a draw tube, so that the action of the kaleidoscope is extended to objects of all sizes, and at all distances from the observer, and united, by that means, to the advantages of the telescope. J. P."

Professor Pictet's opinion is stated in the following letter:

"SIR,—Among your friends, I have not been one of the least painfully affected by the shameful invasion of your rights as an inventor, which I have been a witness of lately in London. Not only none of the allegations of the invaders of your patent, grounded on a pretended similarity between your kaleidoscope

 and

and Bradley's instrument, or such as Wood's or Harris's theories might have suggested, appear to me to have any real foundation; but I can affirm, that neither in any of the French, German, or Italian authors, who, to my knowledge, have treated of optics, nor in Professor Charles's justly celebrated and most complete collection of optical instruments at Paris, have I read or seen any thing resembling your ingenious apparatus, which, from its numberless applications, and the pleasure it affords, and will continue to afford, to millions of beholders of its matchless effects, may be ranked among the most happy inventions science ever presented to the lovers of rational enjoyment.

"M. A. Pictet,
"Professor of Nat. Phil. in the Academy of Geneva.

"*To Dr. Brewster.*"

The propositions in Harris's Opticks relate, like Professor Wood's, merely to the multiplication and circular arrangement of the apertures or sectors formed by the inclined mirrors, and to the progress of a ray of light reflected between two inclined or parallel mirrors; and no allusion whatever is made, in the propositions themselves, to any instrument. In the proposition respecting the multiplication of the sectors, the eye of the observer is never once mentioned, and the proposition is true if the eye has an infinite number of positions; whereas, in the kaleidoscope, the eye can only have one position. In the other proposition, (Prop. XVII.) respecting the progress of the rays, the eye and the object are actually stated to be placed *between the reflectors;* and even if the eye had been placed without the reflectors, as in the kaleidoscope, the position assigned it, at a great distance from the angular point, is a demonstration that Harris was *entirely ignorant of the positions of symmetry either for the object or the eye,* and could not have combined two reflectors so as to form a kaleidoscope for producing beautiful or symmetrical forms. The *only practical part* of Harris's propositions is the 5th and 6th scholia to Prop. XVII. In the 5th scholium he proposes a sort of catoptric box or cistula, known long before his time, composed of four mirrors, arranged in a most unscientific manner, and containing opaque objects *between the speculums.* "Whatever they are," says he, when speaking of the objects, "the upright figures between the speculums should be slender, and not too many in number, otherwise they will too much *obstruct the reflected rays from coming to the eye.*" This shows, in a most decisive manner, that Harris knew nothing of the kaleidoscope, and that he has not even improved the common catoptric cistula, which had been known long before. The principle of inversion, and the positions of symmetry, were entirely unknown to him. In the 6th scholium, he speaks of rooms lined with looking-glasses,

glasses, and of luminous amphitheatres, which, as the Editor of the Literary Journal observes, have been described and figured by all the old writers on optics*.

The persons who have pretended to compare Dr. Brewster's kaleidoscope with the combinations of plain mirrors described by preceding authors, have not only been utterly unacquainted with the principles of optics, but have not been at the trouble either of understanding the principles on which the patent kaleidoscope is constructed, or of examining the construction of the instrument itself. Because it contains two plain mirrors, they infer that it must be the same as every other instrument that contains two plain mirrors; and hence the same persons would, by a similar process of reasoning, have concluded that a telescope is a microscope, or that a pair of spectacles with a double lens is the same as a telescope or a microscope, because all these instruments contain two lenses. An astronomical telescope differs from a compound microscope only in having the lenses placed at different distances. The progress of the rays is exactly the same in both these instruments, and the effect in both is produced by the enlargement of the angle subtended by the object. Yet surely there is no person so senseless as to deny that he who first combined two lenses in such a manner as to discover the mountains of the moon, the satellites of Jupiter and Saturn, and all the wonders of the system of the universe, was the author of an original invention. He who produces effects which were never produced before, even by means which have been long known, is unquestionably an original inventor; and upon this principle alone can the telescope be considered as an invention different from the microscope. In the case of the kaleidoscope, the originality of the invention is far more striking. Every person admits that effects are produced by Dr. Brewster's instrument, of which no conception could have been previously formed.

All those who saw it, acknowledged that they had never seen any thing resembling it before; and those very persons who had been possessors of Bradley's instrument, who had read Harris's Opticks, and who had used other combinations of plain mirrors, never supposed for a moment, that the pleasure which they derived from the kaleidoscope had any relation to the effects described by these authors.

No proof of the originality of the kaleidoscope could be stronger

* The reader is requested to examine carefully the propositions in Harris's Opticks, which he will find reprinted in the Literary Journal, No. 10. He will then be convinced, that Harris placed both the eye and the object between the mirrors, an arrangement which was known 100 years before his time.

 than

than the sensation which it created in London and Paris. In the memory of man, no invention, and no work, whether addressed to the imagination or to the understanding, ever produced such an effect. A universal mania for the instrument seized all classes, from the lowest to the highest, from the most ignorant to the most learned; and every person not only felt, but expressed the feeling, that a new pleasure had been added to their existence.

If such an instrument had ever been known before, a similar sensation must have been excited, and it would not have been left to the ingenuity of the half learned and the half honest to search for the skeleton of the invention among the rubbish of the 16th and 17th centuries.

The individuals who have been most eager in this search, did not, perhaps, calculate the degree of mischief which they have done to those who have been led, upon their authority, to encroach upou the rights of others, and thus subject themselves to very serious consequences. The delay which has taken place in commencing legal proceedings, has not arisen from any doubt of the complete originality of the kaleidoscope, and of the defensibility of the patent. As soon as the patentee has made himself acquainted with the circumstance of the individuals who have invaded his patent, with the channels through which they have exported their instruments, and with the amount of the damage which they have done, he will seek for that redress which the law never fails to afford in cases of notorious and unprovoked piracy. We are well assured, that it never was the intention or the wish of Dr. Brewster to interfere with the operations of those poor individuals who have gained a livelihood from the manufacture of kaleidoscopes. We know that it will always be a source of no inconsiderable gratification to him, that he has given employment to thousands of persons, whom the pressure of the times had driven into indigence; and even if a decision in favour of his patent were given, he would never think of enforcing it, excepting against that class of opulent pirates, who have been actuated by no other motive but the exorbitant love of gain, in wantonly encroaching upon the property of another.

The patent kaleidoscopes are now made in London, under Dr. Brewster's sanction, by Messrs. P. and G. Dollond, W. and S. Jones, Mr. R. B. Bate, Messrs. Thomas Harris and Son, Mr. Bancks, Mr. Berge, Mr. Thomas Jones, Mr. Blunt, Mr. Schmalcalder, Messrs. Watkins and Hill, and Mr. Smith. An account of the different forms in which these ingenious opticians have fitted up the kaleidoscope, and of the new contrivances by which they have given it additional value, will be given in Dr. Brewster's Treatise on the Kaleidoscope, now in the press. The public

public will see, from the examination of these instruments, how much they have been imposed upon by spurious imitations, sold at the most exorbitant prices, and made by individuals entirely ignorant, not only of the principles and construction of the instrument, but of the method of using it.

LXXX. *On the Question "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?"* *By* HENRY UPINGTON, *Esq.*

[Continued from p. 327.]

Blair's Hill, Cork, June 10, 1818.

SIR, — THE paper which I now send you for insertion, has for its object the continuation of my letter of 12th April, which appeared in your Magazine for May.

The investigation of the ordinary language of the London gentleman habituated from infancy to the higher circles, was the only topic of that letter; and extended no further than the refutation of an ill-founded, though in some degree prevalent theory which originated, at least in these countries, with the author of an eccentric prosody called *Rationalis*, "that speech was necessarily carried on by no other medium than that of *slides*." It remains therefore that I pursue my inquiry with regard to the musical character of our language, as delivered by that portion of the politer circles acknowledged as our standard.

To avoid circuitous expression, I shall in future designate that gentleman who has consented to become the subject of my experiments, by the appellation of THE SPEAKER; while to that friendly assistant whose discriminating ear has been already reported, I shall assign the appellation of ASSOCIATE. This being mentioned, I follow up my analysis.

Examination of THE SPEAKER *continued.*

OF INTERVALS*.

Observation 3. Although the easy and even commanding

* The reader will probably imagine that the mensuration of speaking intervals must need the assistance of an instrument. The contrary is the truth: the musician whose ear is naturally good, and accustomed to this novel exercise, will find no difficulty in catching and retaining for a few moments (as persons unskilled in music will catch a song) any half a dozen or more consecutive sounds to which his attention has been exclusively directed. Those sounds are afterwards analysed and noted accordingly; the signs + or —, and in some cases an additional memorandum being annexed when the ordinary *lines* are insufficient for the notation of the minuter intervals. What further remains? This process being frequently repeated, the relative proportions within each individual essay are set down in order, and the frequency of occurrence is ascertained at leisure.

compass of this gentleman was found by experiment to exceed an *octave*; yet during the same sentiment, as well as could be judged, he scarcely ever surpassed the boundary of the *fifth*, from extreme to extreme; and very rarely, at any *one* time, expressed a more distant interval than the *fourth*.

He frequently repeated the preceding note (occasionally even three or four times); and, all cases considered, perhaps the one half or nearly the one half of his variations consisted of perfect and imperfect *semitones*. The *tone* usually styled a *second* was the next: then followed the *minor* 3d; and lastly, the *fourth*.

The minor 3d, singular as it may appear in theory, which confines it to the *serious*, was now and then distinguishable even in the cheerful though bordering on the *joyous*; while on the contrary, the major 3d, which evidently characterizes the joyous, was very rarely perceptible—the interval which may be said to represent it being too flat*. The *fourth* (which was nearly as frequent as this imperfect major 3d, although much less frequent than the minor 3d) was almost exclusively confined to the bold and majestic; and the *fifth* was hardly ever struck at all.

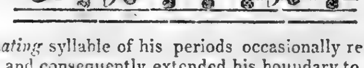
No governing key-note, to which as in music the various intervals are referrible, could in any one instance be discovered;—what is popularly styled the change of key appearing no more than an alteration of *pitch*, and a certain undefinable series appertaining to that sentiment which produced the alteration. Neither was there any particularly predominating note, in which the mass of his syllables could be said to have been expressed †.

To return to the *extremes*, or general boundary of the melody. Whenever, in the *progress* ‡ of a period, he surpassed the *fifth*, it was generally occasioned by some such word as *tolerable*, *usefulness*, *objectionable*, &c.—whose *stress* being seated too remote from the final syllable, produced an apparent difficulty of sustentation, which precipitated his voice much lower than good

* Whether the serious (shall I call it *tragic?*) disposition of the SPEAKER may have caused this seemingly inappropriate intonation, I shall not venture to decide. The sprightly joyous *comedian* who excels in his profession, has the major 3d at his command.

† Individual short *clauses*, abstractedly taken, did very often exhibit a predominating note—which note may not improperly be called the *common tone* of those clauses; a term familiar to the ancients.

Take an instance of this common tone exemplified in C natural; viz.



‡ The *terminating* syllable of his periods occasionally reached an additional semitone, and consequently extended his boundary to a minor 6th.

taste

taste could have authorized. The same may be said of words whose emphatic syllable was seated even on the penultimate itself, as *many, direction, relation*, &c.; that is, when those words were encumbered by the succeeding particles *of a, to a, of the, to the*,—such particles constituting in such cases, as it were an additional number of syllables, whose tendency (to use the scholastic epithet) was decidedly *enclitical*.

I shall terminate this series of observations by exhibiting to the reader a collective statement of 413 syllables or notes, which (exclusive of the commencing syllable of every separate essay) were struck by the SPEAKER, at different times and during the expression of various sentiments, from the commencement to the conclusion of this examination.

Table of Occurrence.*

Repetitions	178	Sometimes imperfect.
Semitones	113	Frequently imperfect.
Tones ..	69	Rather perfect.
Minor 3ds	28	
Major 3ds	13	Almost all too flat.
Fourths ..	10	Rather perfect.
Fifths ..	2	
	413 syllables.	

Remarks. The reader will no doubt recollect, that in the course of the preceding inquiry, the intervals employed in the *ordinary* sentiments alone, presented themselves for examination. As to the stronger sentiments, or more properly the *passions*, I have had no opportunity to analyse them; yet from casual observations in common life, I am thoroughly persuaded that there are not only certain *intervals* more appropriate than others for the expression of those passions—but also certain *qualities* of the voice, as *round, harsh*, &c. peculiar to every individual emotion.

The judicious application then of those intervals and these qualities may, as far as intonation is concerned, be styled *expression;* the former, as dependent on the knowledge of sounds, and *appropriate* cultivation of the ear and speaking organs, being the lot of few,—the latter, as more immediately connected

* It may be necessary to elucidate this table by an *example* which, agreeably to the adopted mode of computation, contains three repetitions, two semitones, one tone; viz.

with

with the mind, being comparatively the lot of all. *Nature*, so termed, can by no means command the intervals; neither can she at all times command the qualities: the frequently deep, yet sufficiently varied and expressive intonations of the enraged *Spaniard*, like those of the ancient *Roman* the gravity of whose speaking habits, or rather of whose *accentuation* he apparently inherits [some consider these habits *oriental*] are terrific as thunder when compared with the insignificant monotonous squeak of the *American savage**.

Reverting to that part of my observation which represented the apparently contracted scale of the SPEAKER, whose melody was almost confined to the limits of the *fifth*, called by the Greeks the *diapente*,—is there not in this case a curious coincidence between the polite usages of the modern and the ancient? *That* well-known passage of Dionysius of Halicarnassus, so twisted and perverted by modern theorists, is a memorable testimony of this interesting fact. The literal translation, from Upton's edition, sect. xi. is this:

" The melody of *discourse* [Διαλεκτου]," says Dionysius, " is measured by *very nearly* [ὡς εγγιστα †] *that* interval called the diapente [or fifth] neither strained to sharpness beyond three tones and a semitone, [which is the extent of that interval,] nor relaxed to gravity [after the ascent is accomplished] beyond that space."

But after all, is not every desirable tone (for speech) contained *within* the diapente? Does it not comprise our *second*, *thirds*, and *fifth*, with all the intermediate semitones? And for *majesty*, what interval can surpass the *fourth*? I shall give an example.

What a noble anapæst, and how indicative of war! depicting most strikingly to our imagination the steady determined onset

* Rousseau in his Musical Dictionary, article *Accent*, speaking of the habitual difference among Europeans themselves in expressing the various passions, says: " L'Allemand hausse également et fortement la voix dans la colère; il *crie* toujours sur le même ton: l'Italien, que mille mouvemens divers agitent rapidement et successivement dans le même cas, modifie sa voix de mille manières. Le même fond de passion regne dans son ame; mais quelle varieté d'expressions dans ses Accens!"

† These words, though in my opinion material to the sense, by acknowledging the impossibility of measuring our extremes with *accuracy*, have been omitted by Mr. Mitford, in his otherwise faithful translation. See "Harmony in Language." As to the general meaning of melodizing within the diapente, [or between C and G, these extremes being included,] it is sufficiently obvious.

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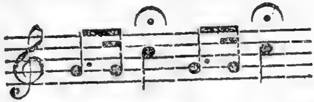
of a compact battalion; and so decided too in its character, that change its time or measure as we please, it is almost impossible to disguise it; for although we should degrade it to very nearly the level of a dance, as thus,



we shall, nevertheless, recognise the military *bugle* directing our movements in the field.

Evident as it may seem to the intelligent reader, that within the *diapente* every desirable speaking tone is to be found; yet I am greatly apprehensive, that a considerable portion of our *ranting* orators will inveigh against every species of restriction, which, without understanding the term, they will call the "shackling of *nature.*" Assuredly, I can have no objection to allow the passions a decent sway: *anger*, for instance, the most irregular of all, may express the majority of its tones on the higher keys*, and be as little systematic as it pleases: but I am certainly of opinion, that a judicious speaker will represent that passion † rather by vehemence and limited intonation, than by the monstrous transition of barbarian octaves. The *extremes*, it is true, may now and then be carried to the extent of this interval; but the reaching of those extremes, in the *same* member, or rather *clause* of a sentence, even by the interposition of other intervals, should never be attempted.

In *music* itself, to speak no longer of oratory,—of what superior melody is the *diapente* susceptible, proceeding at the same time by the smallest of our gradations! Almost all the professional musicians with whom I have conversed, have expressed their astonishment at the analysis of our incomparable national air "God save the King," which, for the gratification of the amateur, I shall repeat.

* The eleventh book of Quintilian's Institutes of the Orator (chapter iii.) contains a very sensible remark upon this topic. "In familiar discourse," says the judicious Roman, "the voice holds a certain medium between the tones that are flat and sharp. In vehement passions it rises, in more tranquil it falls; but higher or lower proportionately to the degree of either."

† This representation may be called *well-regulated* anger; such as the orator who preserves intelligibility and decorum is authorized to exhibit. I speak not of that overwhelming passion called *rage*, which in actual life so frequently brutalizes the herd of mankind. Neither do I speak of our *theatre*;—the enraged actor may certainly proceed somewhat further than the angry orator; and yet even the *actor*, in the very "whirlwind of his passion," must employ the octave with caution, this interval being certainly too *wide*, both for modulation and for command.

Analysis

Analysis of "God save the King."

This noble composition, as originally designed, viz. independently of the embellishment or grace which was afterwards introduced into the penultimate bar, will be found on inspection to surpass the diapente by a single semitone—no more! It contains, in its present form, 42 notes; and proceeds in the following manner by repetitions and gradations.

Repetitions	8
Semitones	12
Tones	18
Minor 3d	1
Major 3d	1
Fifth	1
	41
Add the commencing note	1
	42 notes, as stated.

Were it necessary to produce an additional subject for analysis, I would prefer for the purpose, above all others, that model of chaste composition "*Hope, thou Nurse*" in the opera of "Love in a Village," which song, when ably executed, never fails to delight. Of this, however, it will be sufficient to remark, that its widest *interval* is a *tone* (with the exception of one minor 3d), and that its *extremes* are limited to a minor 7th.

I shall proceed no further in defence of the *diapente*, which, if my judgement be not inaccurate, the well-formed orator will never covet, in any *one* clause of a sentence to surpass; but with regard to the very great *imperfection* of our notes, or rather the notes of the SPEAKER, is *this* defensible, either on the ground of chasteness or expression? Is it not rather probable, that it arose partly from imitation or habit inconsiderately called *nature*, and partly from the non-cultivation of his ear? And may we not thus account for the numerous undefinable intervals, which, in speech, are every instant discoverable throughout society?

Such then having become the general usage of our country, would it not appear that the cultivation of *music* in the *ordinary* way, can by no means remunerate the orator for his inevitable loss of time and trouble? Still, must not the exercise of the speaking voice under an experienced *master*, who, in addition to every other requisite, shall possess an exquisite ear, be highly beneficial? A person so qualified to teach, especially if a scientific musical composer, would avail himself of all the advantages of his art, without violation of decorum. He would design the most suitable *intervals* for well-selected passages; regulate, as far

far as language will admit, the most appropriate species of *time* for every individual sentiment; execute those passages for his own improvement, and impart their genius to the intended orator. But where is such a master to be found? View him in perfection—a *Chatham* and a *Handel* are his portrait.

[To be continued*.]

LXXXI. *Notices respecting New Books.*

On the Safety-lamp for Coal Miners; with some Researches on Flame. By Sir Humphry Davy. 8vo. p.p. 145.

The design of the present work is to exhibit in a connected form, an account of all the researches that Sir Humphry Davy has made "on the subject of explosions from inflammable air, and the modes in which they may be prevented, as well as the collateral investigations to which they have given rise, with the hope of presenting a permanent record on this important subject to the practical miner, and of enabling the friends of humanity to estimate and apply these resources of science, by which a great and permanently existing evil may be subdued."

The contents of the body of this work have of course been already before the public in different shapes, and any recapitulation of them is particularly unnecessary to our readers, to whom the greater part have been progressively communicated through the medium of the Philosophical Magazine as soon as they appeared.

The following brief Appendix is new.

"Since the Researches upon Flame contained in the foregoing pages have been published, M. Gay-Lussac has put into my hands a paper written some years ago by M. de Humboldt and himself, which contains some very interesting results, that may be adduced as confirmations of my principles, on the causes of combustion and explosion.

"MM. Gay-Lussac and de Humboldt have shown that when oxygen and hydrogen are mixed in proportions in which they cannot be fired by the electrical spark, they may be still made

[* There appeared in this Magazine for April, a letter signed "*Unus*," referring me to his "*Pocket Companion*" for an illustration of the first principles of music. I have not seen this "*Companion*;" but from Unus's own description of the work, I fear that it will not realise his expectation. *Rameau* and *Tartini* are by far the most able advocates of our present harmony; and *Rousseau* must be acknowledged its still more able opponent. See his "Musical Dictionary" *itself*, (article "Harmony,") not the *partial* and *misleading extracts*, which have been given us from time to time in modern publications.]

to

to combine in the proportions which can form water, by artificially raising their temperature.

"MM. Gay-Lussac and de Humboldt suppose, that the action of electricity in producing combination is owing to the heat it produces by the compression of the elastic medium through which it passes. This idea is very ingenious; but the phænomena of decomposition by electricity, show that there is some relation between the primary attractive powers of the chemical elements and their electrical energies.

"When the common electrical or Voltaic electrical spark is taken in rare air, the light is considerably diminished. I made some experiments to ascertain whether the heat was likewise diminished, and I found that this was certainly the case. Yet in a receiver that contained air sixty times rarer than that of the atmosphere, a piece of wire of platinum, placed in the centre of the luminous arc, produced by the great Voltaic apparatus of the Royal Institution, became white hot; and that this was not owing to the electrical conducting powers of the platinum, was proved by repeating the experiment with a filament of glass, which instantly fused in the same position.

"It is evident from this, that electrical light and heat may appear in atmospheres in which the flame of combustible bodies could not exist, and the fact is interesting from its possible application in explaining the phænomena of the *aurora borealis* and *australis*."

Journal of the Academy of Natural Sciences at Philadelphia. Part I. Vol. I. 8vo. pp. 218.

The Academy of whose earliest Transactions we are here presented with an account, has only been recently instituted. It consisted at first of a voluntary association for the encouragement and cultivation of the sciences, but on the 24th March 1817 was incorporated by an act of the legislature of Pennsylvania. In the list of its members and donators we observe some of the most eminent scientific names of North America; and it will be sufficient to mention the heads of the papers which have been selected for publication out of the mass already submitted to its consideration, to show that the portion of diligence, research, and ability embodied in its support, is such as to warrant us in entertaining the strongest hopes of its future usefulness. The following are the contents of this first part.

"Description of six new Species of the Genus Firola, observed by Messrs. Le Sueur and Péron in the Mediterranean Sea, in the months of March and April, 1809. By C. A. Le Sueur. —Account of a North American Quadruped, supposed to belong

to

to the Genus OVIS. By G. Ord.—Description of seven Species of American Fresh-water and Land Shells, not noticed in the Systems. By Thomas Say.—Descriptions of several new Species of North American Insects. By Thomas Say.—Observations on the Genus ERIOGONUM, and the Natural Order POLYGONEÆ of Jussieu. By Thomas Nuttall.—Notice of the late Dr. Waterhouse.—Characters of a new Genus, and Descriptions of three new Species upon which it is formed; discovered in the Atlantic Ocean in the months of March and April, 1816; lat. 22° 9′. By C. A. Le Sueur.—Description of three new Species of the Genus RAJA. By C. A. Le Sueur.—Some Account of the Insect known by the name of Hessian Fly, and of a parasitic Insect that feeds on it. By Thomas Say.—On a new Genus of the CRUSTACEA, and the Species in which it is established. By Thomas Say.—An Account of an American Species of the Genus TANTALUS or IBIS.—By George Ord.—An Account of the CRUSTACEA of the United States. By Thomas Say.—A short Description of five (supposed) new Species of the Genus MURÆNA, discovered by Mr. Le Sueur in the year 1816. By C. A. Le Sueur.—Description of two new Species of the Genus GADUS. By the same.—Description of a new Species of the Genus CYPRINUS. By the same.—An Account of an American Species of TORTOISE, not noticed in the Systems. By the same.—A new Genus of Fishes, of the Order *Abdominales*, proposed under the name CATOSTOMUS, and the characters of this Genus, with those of its Species, indicated. By the same.—An Account of two new Genera of Plants; and of a Species of TILLÆA, and another of LIMOSELLA, recently discovered on the banks of the Delaware, in the vicinity of Philadelphia. By Thomas Nuttall.—Descriptions of new Species of Land and Fresh-water Shells of the United States. By Thomas Say.—Descriptions of four new Species, and two Varieties, of the Genus HYDRARGIRA. By C. A. Le Sueur.—Observations on the GEOLOGY of the West India Islands, from Barbadoes to Santa Cruz, inclusive. By William Maclure.—Observations on several Species of the Genus ACTINIA; illustrated by figures. By C. A. Le Sueur. — Description of COLLINSIA, a new Genus of Plants. By Thomas Nuttall."

The manner in which these papers have been given to the world, is particularly deserving of praise. More desirous of doing extensive good than of giving splendid embellishment to their own merits, the Academy have not only published their Transactions in a very cheap form, but in progressive numbers, each consisting of two, three, or more sheets, according as there may be quantity of matter ready for publication at the time.

Just

Just published, A Refutation of the Fallacies and Misrepresentations contained in a pamphlet entitled "An Exposition of the New System of Musical Education, published by a Committee of Professors in London." By J. B. Logier, Inventor of the System.

Just published, a second Edition of Dr. John MacCulloch's Remarks on the Art of making Wine; with Suggestions for the Application of its Principles to the Improvement of Domestic Wines.

LXXXII. *Proceedings of Learned Societies.*

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

On Tuesday June 16th a deputation of Noblemen and Gentlemen of the county of Cornwall waited on Dr. Paris at his house in Dover-street, with a magnificent present of plate for his acceptance. The inscription, which is engraved on a massy silver waiter, records the services for which it was given. "To John Ayrton Paris, M.D. F.L.S. Fellow of the Royal College of Physicians of London, this plate is inscribed by the Noblemen, Representatives in Parliament, and Gentlemen of the county of Cornwall, in testimony of their grateful sense of his services, in originating the plan, and promoting the institution of the Royal Geological Society of the county, which has rendered their home the school of science, and their native riches increasing sources of prosperity."

CALEDONIAN HORTICULTURAL SOCIETY.

At the last meeting of the Caledonian Horticultural Society, an additional report was read from Messrs. Hay, Macdonald, and Neill, who, at the desire of the Society, paid a horticultural visit to some parts of the continent. The report stated, that grafts of nearly fifty new pears, and about forty new apples, of the most approved kinds, raised by M. Van Mons at Brussels, had been received last spring; that they had been grafted on suitable stocks, in the nurseries of Messrs. Dickson and Company, and Dicksons Brothers, and with perfect success. It also mentioned that a collection of cones of some of the new species of pinetree, originally brought from the heights of South America by the celebrated traveller Humboldt, and raised a good many years by M. Parmentier, Mayor of Enghien, had been received from the Mayor, and were now under trial.

LXXXIII. *In-*

LXXXIII. *Intelligence and Miscellaneous Articles.*

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for May 1818, it appears that during that month, the following was the work performed by the engines reported with each bushel of coals.

	Pounds of water lifted 1 foot high with each bushel.	Load per square inch in cylinder.
23 common engines averaged	23,608,329	various.
Woolf's at Wheal Vor ..	29,032,182	17·2 lib.
Ditto Wh. Abraham ..	31,520,346	16·8
Ditto ditto	29,702,703	5·68
Dalcouth engine	38,233,193	11·2
Wheal Abraham ditto ..	33,714,842	10 9
United Mines engine ..	33,967,127	13·6
Treskirby ditto	40,615,253	10·8

EXPLOSION IN A COAL MINE.

We regret to record the following fatal accident in the coal-pit, at the Newton Green, Ayr:—About seven o'clock in the morning of Thursday, 18th June, Mr. Miller, the manager of the coal works belonging to Messrs. Taylors, in the prosecution of his duty, went into the pit; and he and the oversman of the pit, after providing themselves each with a safety-lamp, proceeded to examine a part of the mine wrought some time ago, in order to open a door by which to increase the circulation of the air, and they were followed at a little distance by six of the ordinary colliers. When they had proceeded a considerable way, the lamps indicated the presence of hydrogen; but, having confidence in their efficacy, they proceeded until both lamps became red hot, and then the gas exploded, scorching and tossing them about. The oversman, although severely burnt, escaped with his life; but the manager was found dead, from all appearance suffocated by the choke damp which succeeded the explosion. The colliers escaped with little or no injury, and the oversman got out immediately; but the body of the manager was not found till an hour or two afterwards. Mr. Miller was a young man of exemplary conduct and promising abilities, and his loss is much deplored.

We cannot doubt that particular inquiries will be made into all the circumstances of this explosion. In the mean time let it serve as a caution to miners to be prudent and vigilant, and to act with great circumspection when visiting old workings. This, on the face of it, appears to be one of those cases in which the lamp with a double covering of wire-gauze ought to have

 been

been employed: and we would suggest that on every occasion of danger, or of exploration, a quite new lamp should be employed; for there is always a possibility of some particle of coal adhering to lamps that have been in use, which of course will be ignited when the lamp becomes red hot, and may communicate explosion. The lamp ought not, any more than gunpowder, to be exposed to avoidable danger.

IRON IN CAITHNESS.

There are strong appearances of iron upon the coast, about the Ord of Caithness, and in many places between the Ord and Wick, and to the northward of Wick. The coast of Caithness is remarkable for pretty high rocky cliffs, in which great numbers of veins or perpendicular mineral fissures appear, many of which contain iron ore. Some of these have been observed near the old ruinous castle of Girnigo, and the Castle of Arkergil. These veins appear to be bold and roomy, and to contain plenty of good iron ore, from the abundance of a bright red iron earth found in the surface of them. There is great abundance of bog ore over all the low country of Caithness. In many places it almost covers the whole face of the ground to a considerable depth. It is easily known and distinguished by the friable constitution of its misshapen masses, by its external blackish and rusty colour, and by its internal blackish gray colour and granulated porous texture. It is always found loose on the surface of the ground in the same manner as float ore, without any connexion with the vein or stratum.

DISCOVERY OF ANTIMONY IN BANFFSHIRE.

A promising appearance of antimony ore has been lately discovered on the estate of Lord Fife. This ore, we understand, has been examined by Professor Jameson, who finds that it is the radiated gray antimony, and contains 70 parts antimony and 30 of sulphur. We trust this very promising discovery will be vigorously pursued.

TWO NEW MINERALS.

We understand that Dr. MacCulloch (the geologist) has discovered two new minerals in Scotland, an account of which will be given in his work on the Hebrides. We have obtained the following sketches of their prominent characters.

The first is easily recognised by its resemblance to indurated steatite or noble serpentine, and by its green colour, on a fresh fracture, shortly turning to black, when it can scarcely be distinguished by the eye from jet or drycoal: it is also infusible before the blowpipe. Dr. M. has given it the name of chlorophaeite,

eite, from its most obvious property. It occupies amygdaloidal cavities in the trap rocks.

The second is a white powder, of a harsh feel, but incapable of scratching glass, and nearly as fusible as that substance, producing a transparent colourless bead; characters sufficient to distinguish it from any mineral hitherto described. It occupies similar cavities in trap, and he has given to it, from its leading character, the name of conite.

This notice may be of use to mineralogists, by directing their attention to these substances, so as to enable them to collect additional specimens for examination, and thus to extend our knowledge of their characters.

CADUCIUM.—ANOTHER NEW MINERAL.

M. Gay Lussac communicated, in the last sitting of the French Academy of Sciences, a note upon a new metal, which has received the name of *caducium*, discovered by Professor Stromeyer, of Gottingen. The caducium is white as tin, very ductile, combines easily with other metals, fuses and volatizes in less time than zinc. It is found in abundance in the mines of this last metal. Its specific weight is 8·65. This discovery, M. Gay Lussac expects, will be of great consequence to the arts, on account of the properties which the new metal possesses; and of those which it can communicate to metals with which it is capable of amalgamating.

EARTHQUAKE.

On the 9th inst. about twenty minutes part two o'clock, P.M. there was a smart shock of an earthquake, in the neighbourhood of Hayfield, on Loch Awe Side, Scotland. A gentleman who was writing in his parlour first heard a report like that of artillery, and afterwards a noise like that of rocks tumbling down from the neighbouring mountain of Cruachan. Having run out to see what was the matter, he was not sensible of the earthquake which succeeded; but his son and daughter, who were in another room, were nearly tossed over, and the servants, at their dinner in the kitchen, were astonished to see the plates on the table dance before them. The shock was felt by several families in a range of two or three miles, but no injury was sustained by it.

RUSSIAN VOYAGE OF DISCOVERY.

On the 17th of June the Russian brig Rusick, under the command of Lieut.Kotzebue, reached Portsmouth, last from the Cape of Good Hope, having been out two years and eleven months on a voyage of discovery. The crew had continued remarkably healthy, but she was obliged to put into Portsmouth for a supply of of provisons and water. During his voyage, which at first was directed to the North, Lieut. Kotzebue reached a very high latitude, but we are not yet able to state it with accuracy. He fell in with a most singular iceberg, of great magnitude, which not only had a portion of its surface covered with earth and mould, and bearing trees and vegetable productions, but a portion of its *water line* covered with a shore formed by the deposit of earthy matter washed down from the higher parts of the earth-covered iceberg. On this shore a landing was made, and considerable quantities of remains of the mammoth were found in such a state of putrefaction as to produce a most insupportable stench. The Rusick brought away a number of the tusks and other parts of these immense animals, which had probably been preserved in a frozen state for many ages, till the mass of ice which inclosed them, put in motion by some unknown cause, reached a more temperate latitude.

MEDICAL BLANK LABELS MADE BY MACHINERY.

A gentleman has invented a perfectly novel apparatus for making the labels used by apothecaries and chemists in a neat ornamental manner by machinery. These labels are known by a small raised or embossed line surrounding them, and taking a fanciful turn at the broader extremity; also by the accurate and uniform angle in which they are folded, and by the peculiarly good quality of the paper. On being opened they will be found tapering—not straight, like those made by the apothecaries, being cut one out of the other, which effects a great saving of paper. The two sizes are well apportioned for the services required, and upon the whole, they are considerably cheaper than persons requiring the article, however small the quantity, can make them for themselves. To the medical man, therefore, of extensive practice these labels must be very useful; for in a business where every leisure moment may be turned to profit, any one must be glad to relieve himself from an occupation so trifling, and at best so unsatisfactory, as that of making his own labels, when he can get them cheaper and better without any trouble at all.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Dyson, of Watford, in the county of Hertford, for certain apparatus for the culture and tillage of land.—26th May. —2 months.

To Charles Greenway, of Manchester, for his improvement in the operation of opening raw cotton or cotton-wool, previous to the carding and spinning the same, and by which improvements such operation will be fabricated.—26th May.—2 months.

To

To George Michael, of St. Austell, in the county of Cornwall, for certain improvements in the method of opening and shutting windows or sashes, and also in the application of machinery to the opening and shutting window-shutters, and in other cases where the aforesaid improvements may be applied.—26th May. —2 months.

To Henry Taylor, of Kington, Surrey, for certain improvements on machines or apparatus for catching and destroying of rats and other vermin.—26th May.—4 months.

To Thomas Homfray, of the Hyde, in the parish of Kinfare, Staffordshire, for his new kind of bobbin or bobbins, used in spinning and other manufactories.—28th May.—2 months.

To William Lester, of the Commercial-road, Middlesex, for his new method of increasing and projecting light, produced by lamps or other means.—2d June.—6 months.

To George Atkinson, of Leeds, for a combination of materials to produce an article resembling bombazeen.—10th June. —4 months.

To William Eaton, of Wiln Mills, in the county of Derby, for improvements in certain parts of the machinery employed in the roving and spinning of cotton and wool.—18th June.—6 months.

To Robert Winch, of Shoe-lane, in the parish of St. Andrew Holborn, London, and Richard Holden, of Stafford-street, St. Marylebone, Middlesex, for machinery to communicate motion and power to varions other machinery, which requires reciprocating or alternating motion.—18th June.—4 months.

ASTRONOMICAL PHÆNOMENA, JULY 1818.

D. H. M.		D. H. M.	
1. .	☾ in apogee	16. .	☾ in perigee
6.17.35.	☾ η ♌	16.14.30	☾ τ ♐
10. 0.41.	☾ γ ♍	19.23.34	☾ ε ♑
10.14.58.	☾ θ ♍	21.18.56	☾ 29 ♓
11.23.24.	☾ λ ♍	23. 1.16	☉ enters ♌
12 23.30.	☾ ι ♎	25.22.13	δ ♉
13.19.12.	☾ δ ♏	28. .	☾ in apogee
16. 7.10.	☾ φ ♐	31. 0.20	☾ υ ♊

Permit me to remind your astronomical readers, that the ring of Saturn is at this time nearly invisible, owing to our being almost in a line with its plain. On the 1st, its smaller apparent axis is to its greater as 12 to 1000, and on the 25th it will appear a little more opened, the two axes being as 19 to 1000.

ASTRONOMICUS.

Meteorological Journal kept at Walthamstow, Essex, from May 15 *to June* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twelve and Two P.M.]

Date.	Therm.	Barom.	Wind.
May			
15	52 63	29·50	SE—NW—SE. — *Cirrostratus*, and clear; fine day; clear moon-light.
16	57 67	29·50	W—NW.—Clear and *cirrostratus;* fine day; *cirrostratus* at night.
17	51 56	29·61	N—W.—Rain; a dark day, and small rain; showers; cloudy night.
18	49 67	29·90	N.—Sun and wind; clear and *cirrus*; night fine; clear and *cumuli.*
19	47 56	30·01	N.—Wind and *cirrostratus;* gray day; some sun after 2 P.M.; cloudy night.
20	50 61	30·01	N—NE.—Sun and wind; very fine day; clear night. Full moon.
21	50 57	30·10	NE.—*Cirrostratus*; very fine day; perfectly clear at 9 P.M.
22	51 58	30·20	NE—E.—Clear and *cirrostratus;* very fine day; star-light, and windy.
23	50 59	30·30	E.—*Cirrostratus* and wind; clouds and sun; red clear sunset; clear star-light.
24	54 62	30·30	E.—Wind and *cirrostratus;* very fine day; very windy; clear red sunset; cloudy and calm.
25	54 63	30·30	E.—*Cirrostratus* and calm; very fine day; sun and wind; beautiful sunset, and clear night.
26	52 67	30·30	NE.—Fine sun and wind; very fine day.—Moon last quarter.
27	52 63	30·30	NE.—*Cirrostratus* and wind; fine day; sun and *cumuli;* clear night.
28	49 68	30·30	N.—Sun and wind; very fine day; cloudy night.
29	50 60	30·09	N.—Cold and windy; very fine day; clear and *cirrostratus* at night.
30	49 59	30·10	N—E—SE.—Sun, *cirrostratus,* and wind; sun and clouds; clear night.
31	55 73	30·00	S—NW—W.—Clear sunshine; very fine day; drops of rain at 7 P.M.; clear and *cirrostratus* at night.

June

Date.	Therm.	Barom.	Wind.
June			
1		29·90	W—NW.—Fine sun and wind; fine hot day; red sunset; clear and *cirrostratus*.
2	62 76	29·99	W—SW.—Gray morn; fine hot day; star-light.
3	62 76	30·05	W—NW—W—S.—Hot sunny morn; fine day; sun and wind; clear night. New moon.
4	59 79½	30·20	S—SE—E.—Sun and *cirrostratus*; fine hot day; star-light (clear).
5	60 77	30·30	E—SE.—Clear and some wind; at 9 A.M. a *stratus* came on; clear afterwards; clear night.
6	59 74	30·40	NE.—Sun and wind; fine day; *cumuli*.
7	60 70	30·30	N—NE.—Sun, wind, and *cumuli*; fine day; clear and moon- and star-light.
8	64 73	30·25	NE.—Sun, and wind; very fine day; clear night.
9	65 72	30·30	Sun and wind; fine hot day; clear night.
10	65 72	30·30	E.—Hot sun; sun and wind; very hot day; clear calm night.
11	66 82	30·20	E.—Clear sun; very hot day; windy, (Thermometer 102 in the sun); clear night.—Moon first quarter.
12	62 86	30·10	N.—Sunshine and calm; clear and *cumuli*. (Thermometer 102 in the sun); clear and *cirrostratus*; moon in a *corona*.
13	63 86	29·95	NW—SW—W.—Clear sky; sun and *cumuli*; before 5 P.M. a slight shower, and thunder; clear night.
14	66 71	29·99	W—NW—N.—*Cirrostratus*; after 9 A.M. a great shower; fine day; night—wind, clear, and *cirrostratus*.
15	60 75	30·05	SE.—Sun and *cirrostratus*; wind; fine day; deep orange sunset; moon in a small *corona*; clear and *cirrostratus*.

In two fields some grass was begun to be cut on Friday the 5th of June, finished cutting the next day, and all carted before 7½ P.M. on Monday the 8th:—quite dry and good hay.

Peas were first gathered the 7th of June.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
May 15	10	56·	29·64	Cloudy—rain P.M.
16	11	55·5	29·66	Ditto—rain A.M.
17	12	55·	29·93	Fine
18	13	62·	30·05	Ditto
19	14	52·	30·15	Ditto
20	full	57·	30·23	Ditto
21	16	53·	30·33	Ditto
22	17	54·	30·40	Ditto
23	18	59·	30·43	Ditto
24	19	60·5	30·47	Very fine
25	20	61·	30·47	Cloudy
26	21	57·5	30·47	Very fine
27	22	57·	30·47	Ditto
28	23	57·	30·34	Cloudy
29	24	56·	30·26	Fine
30	25	59·	30·22	Ditto
31	26	66·	30·16	Very fine
June 1	27	74·	30·10	Ditto
2	28	71·	30·15	Ditto
3	new	79·5	30·22	Ditto
4	1	76·5	30·33	Ditto
5	2	77·	30·46	Ditto
6	3	71·	30·49	Ditto
7	4	70·5	30·41	Ditto
8	5	69·5	30·46	Ditto
9	6	71·5	30·43	Ditto
10	7	77·	30·36	Ditto
11	8	80·	30·27	Ditto
12	9	83·5	30·16	Ditto
13	10	85·5	30·	Ditto
14	11	66·5	30·11	Cloudy—shower at 4 A.M.
15	12	74·5	30·10	Ditto—rain at 3P.M.—thunder.

without a cloud. (May 31 – June 13)

Wind from N.E. to N.N.E.—not any rain from the 16th May to 14th June. (May 17 – June 13)

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For June, 1818.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May 27	55	66	54	30·23	64	Fair
28	54	66	48	·10	42	Fair
29	50	59	47	·09	40	Fair
30	50	60	49	·02	43	Fair
31	56	70	59	29·88	54	Fair
June 1	60	74	58	·90	59	Fair
2	60	76	62	·92	64	Fair
3	62	79	62	30.01	86	Fair
4	60	77	61	·09	98	Fair
5	60	75	61	·25	76	Fair
6	61	75	57	·24	57	Fair
7	59	75	62	·15	76	Fair
8	66	72	60	·22	70	Fair
9	66	74	61	·19	74	Fair
10	65	75	65	·18	78	Fair
11	68	78	63	·02	102	Fair
12	66	81	69	29·92	82	Fair
13	69	86	68	·82	112	Fair
14	66	76	66	·95	62	Cloudy
15	66	76	64	·90	60	Fair
16	67	74	63	·85	57	Fair
17	66	76	64	·62	72	Showery
18	66	70	60	·69	54	Cloudy
19	60	64	57	·67	50	Showery
20	57	64	55	·66	46	Showery
21	56	68	60	·82	40	Cloudy
22	60	67	55	·70	47	Showery
23	60	68	58	·80	56	Fair
24	57	69	60	·92	61	Fair
25	60	74	66	·98	65	Fair
26	66	76	67	30.02	59	Cloudy

N.B. The Barometer's height is taken at one o'clock.

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END OF THE FIFTY-FIRST VOLUME.

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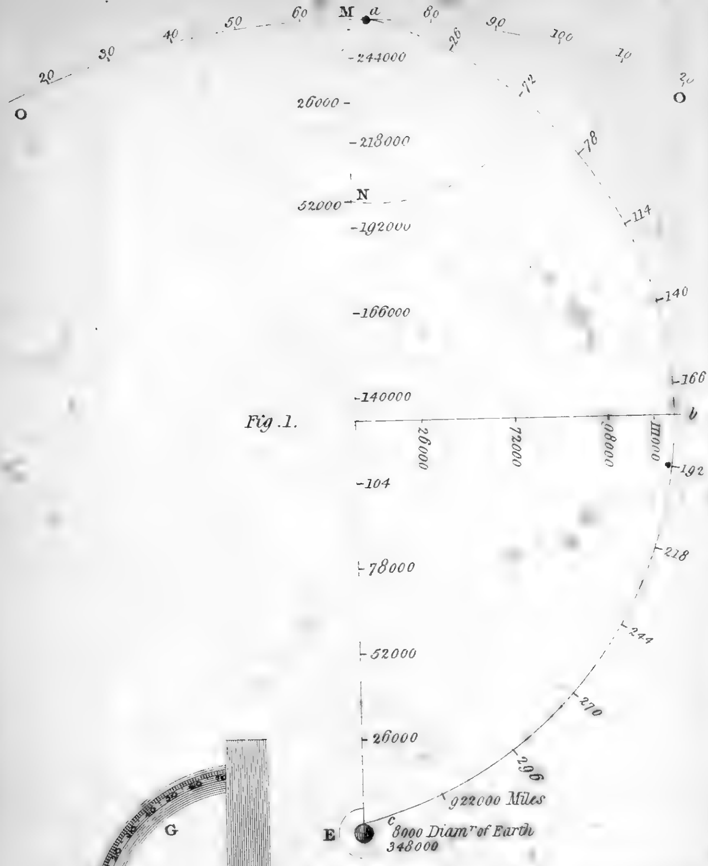

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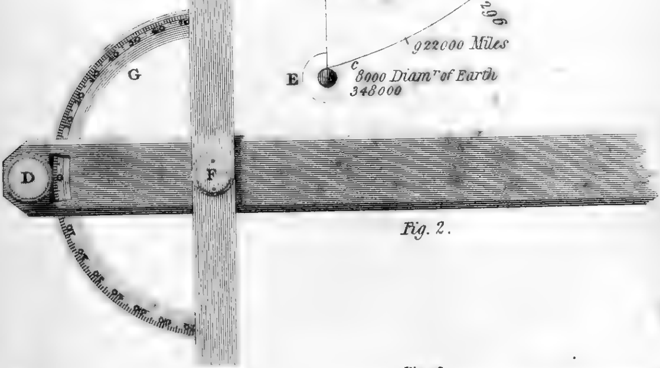
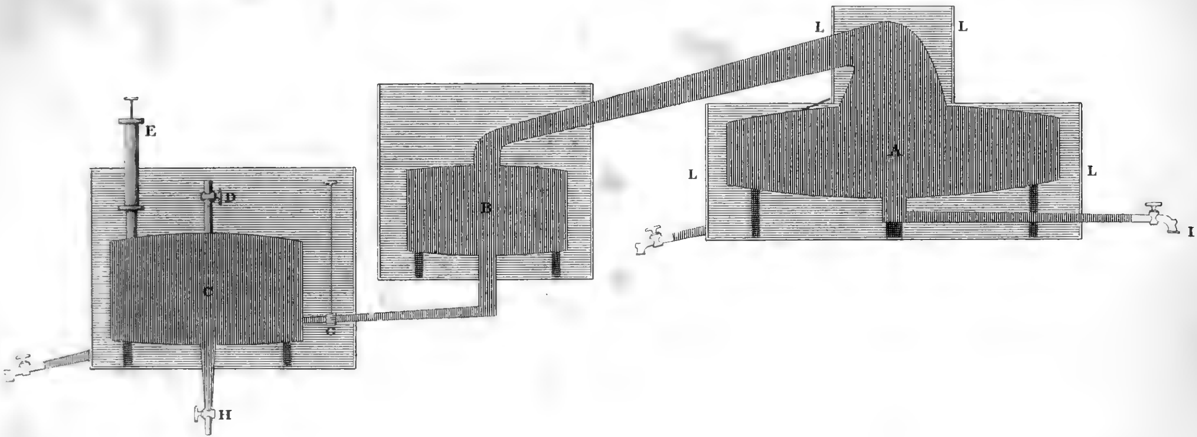


Fig. 2.

Fig. 3.

S. Porter sc.

Mr. H. Tritton's Improved apparatus for Distillation



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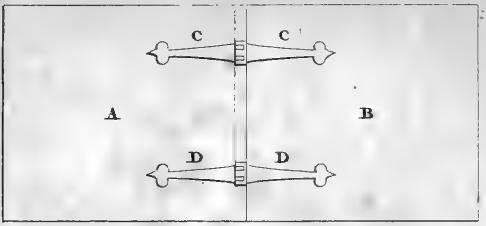

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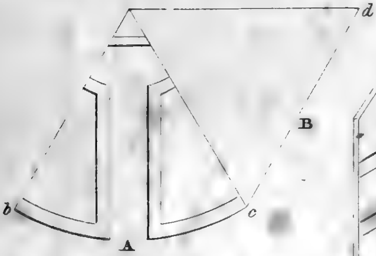

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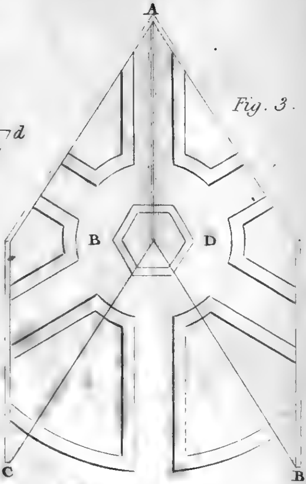

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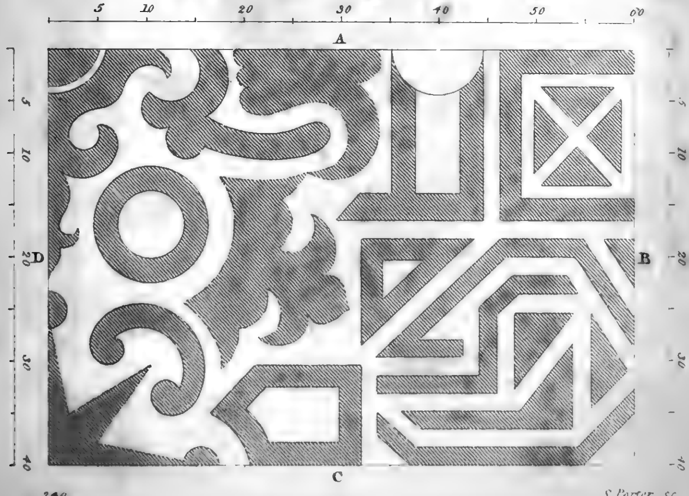

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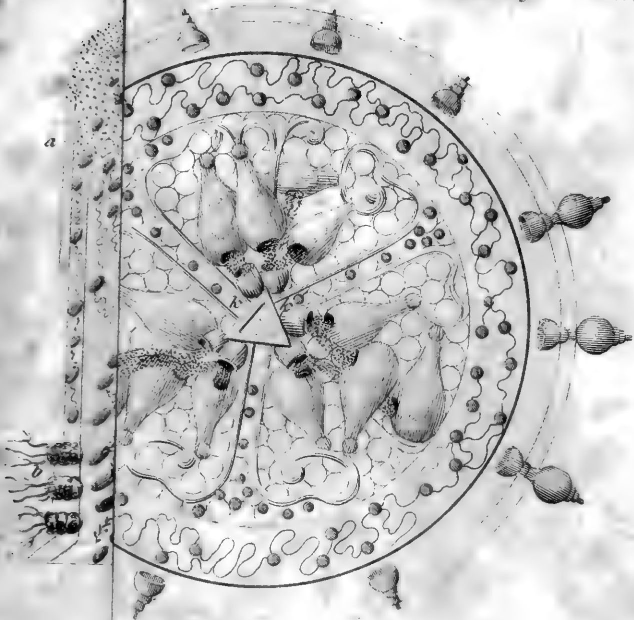

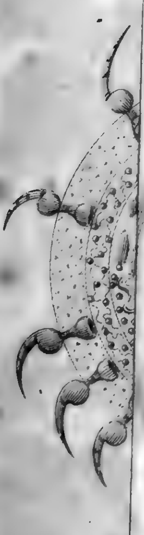

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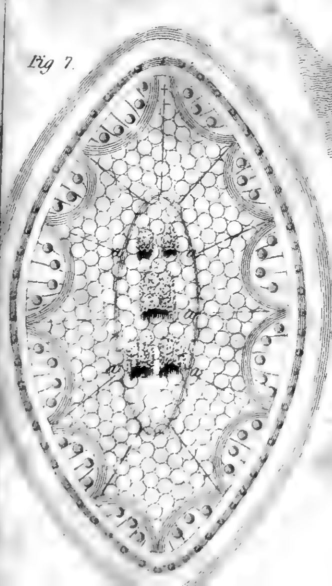

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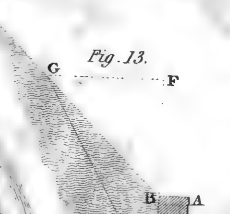

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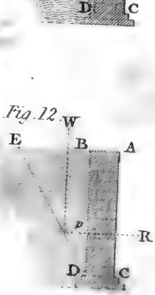

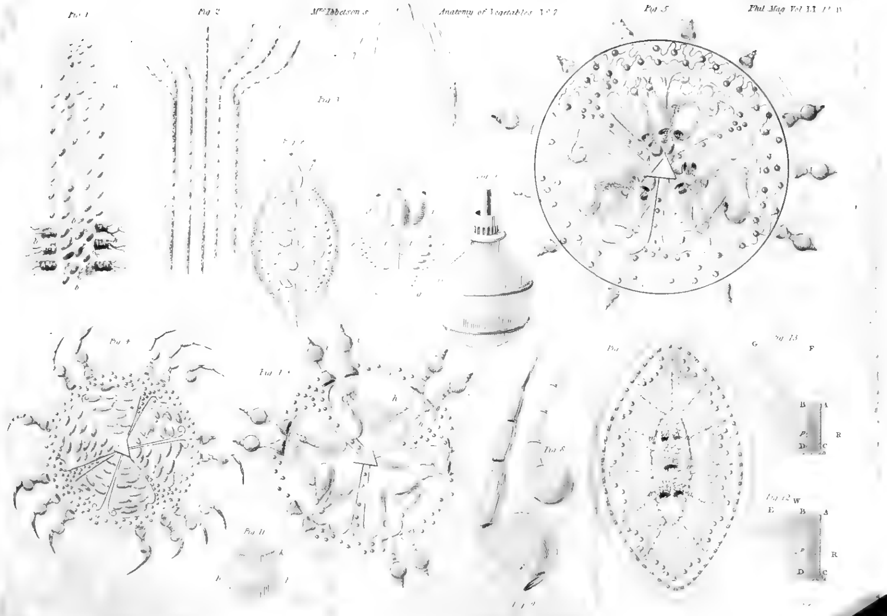

Mrs Ibbetson s
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