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VOL. XI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers in the present Volume, are Mr. Thomas Reid; Mr. James Scott; Mr. Boswell; Mr. John Gough; Mr. Irvine; Mr. Cuthbertson; Mr. William Henry; John Bostock, M. D.; W. Brande, Esq.; Mr. Matthew Murray; Mr. J. C. Hornblower; Mr. A. F. Thoelden; Mr. John Clennell; J. P.; W. N.; W. F. S.; Mr. William Wilson; A. Thomson, Esq.; Amicus; Mr. O. Gregory; Mr. Ezekiel Walker; Mr. James Stodart; Count Rumford, V. P. R. S.; Mr. Thomas Harrison. A Constant Reader.

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Of

PREFACE.

Of the Engravings, the Subjects are, 1. A very simple and perfectly safe Gun Lock, by Mr. Dodd. 2. The framing of a Field Gate with a considerable Accession of Strength, by Mr. Waistell. 3. A Compensation Curb for a Time Piece, by Mr. Scott. 5, 6. Plans and Sections of the Bavarian Salt Works. 7. Captain Brodie's Method of connecting Iron Bars, and coating them with Lead. 8. Mr. Rawlinson's Mill for grinding Colours. 9. Apparatus for grinding Indigo and other dry Materials. 10. Method of banking the Balance of a Time Piece, by Mr. Hardy. 11. A Pyrometer for meaſuring the Heat of a Furnace, by Mr. J. G. F. Schmidt. 12. A Portable Steam Engine, by Mr. Murray. 13. Apparatus for making the Gaseous Oxide of Carbon, by Mr. Baruel. 14. A Topographical Plan by Mr. Churchman, in which the Elevations and Depressions of the Surface of the Earth, are by a new Method correctly delienated. 15. Mr. Gregory's Apparatus for measuring the Power of Horſes. 16. The American Borer. 17. Diagram illustrative of Optics, by Mr. Ezekiel Walker. 18. Mr. Sepping's Blocks for supporting and suspending Ships. 19. Count Rumford's Apparatus for illustrating his Experiments in Proof, that the greatest Density of Water is higher than the freezing Point. 20. A Valve for Steam Engines, by the Chevalier Edelcrantz. 21. Figures and Diagrams, by Edward Pigott, Eſq. to illustrate the Causes of Periodical Decrease in Diminution of Light, in a Star in Sobieſki's Shield. 22. A Press for Botanical Specimens, by Mr. Harrison.

Soho Square, London, September 1, 1805.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY, 1805.

ARTICLE I.

Letter from Mr. THOMAS REID, *on the Construction of Time-keeping Machines.*

To Mr. NICHOLSON.

SIR,

IN your interesting and useful Journal of December last, I was glad to see the improvement of compensation pendulums for astronomical clocks, so zealously taken up by such an able hand, as that of Mr. Edward Troughton's. Compensation pendulum of Mr. Edward Troughton.

But an excellent clock of this sort becomes so very valuable and necessary an appendage in an observatory, to those astronomical instruments with which he is so happily engaged in daily constructing and improving; that he must more readily see their advantage than even those whose business it is to make such clocks. If Mr. Berthoud, a celebrated author on every part that regards the improvement of time-keeping machines, is correct, it would appear, that the steel wires of Mr. Troughton's pendulum must be too slender. Mr. Berthoud, by his experiments, saw, that there was a certain strength of materials necessary, in order to render the compensation complete having found, that on the pendulum rods (if too small) being lengthened by heat, the contraction by cold would not bring Probability from Berthoud's experiments that the steel wires may be too slight.

bring the ball again quite up to the place where it ſet out from when the heat was firſt applied, and this with a ball of a moderate weight; no doubt the weight of the ball may be made ſubſervient to any ſize of wires.

Mr. Troughton ſeems to have attended to this.

Whether Mr. Troughton has attended to theſe circumſtances, I know not, but ſuſpect from his propoſing, yet, to make ſome ſort of pyrometrical apparatus for the further proving of his pendulums, that the complete compenſation has not been fully aſcertained, only in ſo far as regards the calculation of the relative expanſions of braſs and ſteel.

The improvement of clocks, and time-keeping machines of every deſcription, more particularly thoſe deſtined for aſtronomical purpoſes, is a ſubject that has not a little engaged my time and attention.

The pendulum of Ludlam with a wooden rod examined.

There is a pendulum, having a wooden rod, the conſtruction of which is deſcribed with great neatneſs, perſpicuity, and mechanical knowledge, by its author, (Mr. Ludlam, late an eminent profeſſor at Cambridge) in his eſſays, and recommended by him, *who was no mean judge*, as one of the beſt in almoſt every reſpect, particularly in ſo far as regards the impulſe from the clock taking place through the middle line or centre of the rod, to be thence communicated in the ſame line to that of the ball, that hence no circumgyratory motion ſhould take place. Now, although the principle ſet out with *here*, ſeems completely adapted to prevent this ſort of motion, it will be found on trial, as I did, that of all pendulums yet made, it is the moſt liable to generate this very ſort of motion.

It is liable to a ſide oſcillation of the ball round the rod as an axis.

The ball being the middle fruſtum of a globe, a form whoſe matter is much ſpread out from the centre to the edge, and having a large hole, for the rod to paſs through; *this* taking away much of the matter from the centre, tends much more than the lenticular form, to produce the motion Mr. Ludlam wiſhed to avoid.

The ſcrews which are acted on by the crutch do not preſerve a conſtant diſtance,

Another great fault of this pendulum, is, that of putting in ſcrews through the wooden rod, to clip the flat part from the crutch; now in changes of weather from moiſt to dry, or by heat and cold, theſe ſcrews will accordingly be found, ſometimes to pinch the flat part of the crutch, and at other times, to leave it at liberty, or even to allow it to have a conſiderable degree of ſhake between the ſcrew points: hence will ariſe very different degrees of impulſe communicated to the pendulum ball. Wood has a very ſenſible latitudinary

titudinary alteration, by the effects of heat and cold, or by dry and moist, yet these effects on its length are, rather but very imperceptible, or at least, are, in so small a degree, as have not been well ascertained, to what extent they are; even by those who have made experiments with it on the pyrometer. because wood varies across the grain by temperature and moisture, &c.

Mr. Berthoud condemns wood as being unfit for pendulum rods, and although he, and others have given tables of the expansion of various materials, yet none of them have condescended to say, what were the effects of heat and cold on wood of any sort. The end expansion of wood yet unknown.

I am well convinced, that a pendulum may be so fitted up with a wooden rod, as to perform with such a degree of correctness, that it would be a very difficult matter to say, whether it, or the best compensation pendulum yet constructed, when both comparatively tried, was the nearest to accurate measuring of time. Wooden pendulums seem equal to the compound pendulums,

There has been one circumstance attending all those pendulums fitted up with a wooden rod, that their errors have been imputed to the rod, when in fact, they ought to have been imputed to that of the ball, and these errors have arisen from the manner by which the ball is hung on the rod, resting on its lower edge on the regulating nut; and lead having a considerable degree of expansion, clocks having such pendulums have been found, by those who attended properly to their going, to have gone constantly faster in summer than in winter. Let the ball therefore be hung by its centre on the rod, and a much greater degree of accuracy in time-keeping will be seen to follow. but are often ill constructed.

In consequence of my trials with Mr. Ludlam's pendulums, they were found to be extremely troublesome to put on beat, from their strong tendency to this gyratory sort of motion, it being some while, before they would come to move steadily; I hit not only on a method of putting a clock, as it were mechanically on beat, (the common way being by the ear) but was led to think on a way of constructing a pendulum, in which this gyratory motion could hardly take place, even although the pendulum should be but indifferently fitted up. This last was by following a method quite the reverse to that of Mr. Ludlam's, in making the pendulum ball, which I made in the usual or lenticular form, and in order that it should have as much of its matter preserved at the centre, there were two Improved pendulum.

ſteel wires put through the ball, paſſing parallel to each other, and each put a little to one ſide of the centre, through which pendulum rods are uſually made to paſs, and when the rod is wood, it neceſſarily takes away much of the matter from the centre of the ball.

A pendulum of this ſort of mine, and which has a degree of compenſation in it, I made to a clock, which my brother got, and which he has at his houſe, No. 31, Roſamon Street, Clerkenwell.

The experiment applied to watches, &c.

I mentioned this mode of putting clocks mechanically on beat, to my ingenious friend, Mr. Pennington, who has ſince very happily applied the ſame ſucceſsfully in his practice to watches, &c.

Pendulum for regulating the ſtriking part of clocks; not new.

I ſee you have mentioned in your Journal of July laſt, the application of a pendulum to regulate the ſtriking part of clocks, from the Society for encouraging Arts, &c. having given a premium for it to Mr. Maſſy.

This is not a new thing. Mr. Berthoud mentions it as his invention, and you will ſee a drawing, and the deſcription of it, in his *Eſſai ſur l'Horlogerie*, publiſhed in 1763. Julien Le Roy, in my humble opinion, is certainly intitled to the merit of it, as it appears to me, that Berthoud has taken the idea of it from Le Roy's method of regulating the ſtriking train of his repeating watches, which he *invented*, and applied to them about the year 1754.

The crank ſcapement, not new;

There was a premium given alſo in 1799, by the honourable ſociety above mentioned, for a new 'ſcapement by Mr. Goodrich*; now this 'ſcapement was made prior to the year 1740, and invented by the Abbé Soumille; and another nearly of the ſame ſort was made at Rome before that period, as may be ſeen in the collection publiſhed by Thiout, in the year 1741. Surely nobody would think of adopting ſuch a 'ſcapement as this, whoſe principle ſeems to be that of depriving the pendulum of the moſt valuable property it poſſeſſes, viz. that of having the liberty to operate freely under the influence of gravitation. This 'ſcapement keeping the pendulum, as it were in leading ſtrings.

and bad.

I am ſurpriſed that none of the members of this honourable and uſeful ſociety, ſhould not have known, that theſe

† See Philoſ. Journal, quarto ſeries, III. 342, 416. It is a crank.—N.

things

things for which premiums have been adjudged were not new, perhaps they were neverthelefs *fo*, to thofe to whom the premiums were adjudged.

Among Thiout's collection may be feen a 'fcapement, which he gives to the ingenious Dutertre, about the year 1724. Peter Le Roy gave into the Royal Academy of Arts and Sciences in the year 1727, a defcription of the fame 'fcapement which Dutertre claimed, or pretended to fay was his invention. The miftake lays with Thiout, for Dutertre's 'fcapement is an improvement of a very old one, ufed by the Germans in large clocks, perhaps long before the year 1600, though neither the author of it, nor the time *when*, can now be traced.

A 'fcapement made long ago.

However it was before Dr. Hook's time, who invented one of the fame kind before 1658. I would not have entered fo minutely into this difcuffion, but to fhow the progrefs of the duplex, which in its prefent form, was firft made fo by Peter Le Roy, who afterwards gave it up for a bad one; yet it ought to be allowed that that of Dutertre's muft have led him very eafily to it. So much for the duplex 'fcapement, fo called by the workmen, and now in fuch general ufe. A celebrated Philofopher in the fupplement to the Edinburgh Encyclopedia, under the article watch work, has given it a French furname, that of Dupleix, for what reafon, I know not.

leading to the duplex 'fcapement which was firft made by Peter Le Roy.

Thiout has given alfo among the number, a 'fcapement of his own, a fort of detached one, and which may be confidered as the foundation of the detached one of the prefent day, now fo much improved, and of fuch general ufe in all our pocket and box chronometers: indeed it feems a fcapement indifpenfably neceffary for thefe purpofes. Yet Berthoud in his famous time-keeper, No. 8, ufed a very different one from that of the detached fort.

The origin of the common detached 'fcapement afcribed to Thiout.

I am,

Sir, with much efteem, Your's,

THOMAS REID.

Edinburgh, 25*th March*, 1805.

An

II.

An Analysis of the Magnetical Pyrites; with Remarks on some of the other Sulphurets of Iron. By CHARLES HATCHETT, *Esq. F. R. S. From the Philosophical Transactions for* 1804.

(Concluded from page 276.)

§ VII.

Whether the artificial pyrites with minimum of sulphur be, like the natural, magnetical.

SO far, therefore, as can be proved by similarity in chemical properties and analysis, the magnetical pyrites is indisputably a natural sulphuret, completely the same with that which till now has been only known as an artificial product; but, that the mind may be perfectly satisfied, another question must be solved, namely, how far do they accord in receiving and retaining the property of magnetism? common pyrites do not appear to affect the magnetic needle, or, if some of them slightly act by attraction, (which however I never could perceive, nor recollect to have read in works expressly relating to magnetism,) yet they do not possess, nor appear capable of acquiring, any magnetic polarity. As, therefore, the iron of pyrites is undoubtedly in the metallic state, and in a considerable proportion, the destruction of this characteristic property of metallic iron, must be ascribed to the other ingredient, sulphur.

The artificial compound is not a mere mixture.

But we have lately seen, that a natural combination of iron with 36,50 or 37 *per cent.* of sulphur, is in possession of all the properties supposed hitherto to appertain (in any marked degree) almost exclusively to the well known magnetic iron ore; and that the combination alluded to is strictly chemical, and not (as at first might have been imagined) a mixture of particles of magnetic iron ore with common pyrites *.

The compound directly formed at red heat.

This is certainly very remarkable; and it induced me to examine the effects produced by sulphur, on the capacity of metallic iron for receiving and retaining the magnetic properties. I therefore prepared some sulphuret of iron, by adding a large quantity of sulphur to fine iron wire, in a moderate red heat.

* This has been sufficiently proved, by the facts which have been stated; I shall however add, that upon digesting a mixture of the powder of common pyrites and iron filings in muriatic acid, I only obtained hydrogen gas, exactly as if I had employed the iron filings without the pyrites.

The

The internal colour and lustre of the product, were not very unlike those of the magnetical pyrites; and, after the mass had been placed during a few hours between magnetical bars, I found that it possessed so strong a degree of polarity, as to attract or repel the needle completely round upon its pivot; and although several weeks have elapsed since it has been removed from the magnetical bars, it still retains its power, with little diminution; like the magnetical pyrites, however, in its natural state, it is not sufficiently powerful to attract and take up iron filings. resembled common pyrites; but was capable of magnetism.

But this sulphuret did not contain so much sulphur as the magnetical pyrites; I therefore mixed some of it, reduced to powder, with a large quantity of sulphur, and subjected it to distillation in a retort, which was at length heated until the intire bulb became red. It contained less sulphur than the magnetical pyrites. More sulphur added and low ignition.

The sulphuret, by this operation, had assumed very much the appearance of the powder of common pyrites, in respect to colour; but, in its chemical properties, such as solubility in muriatic acid, with the production of sulphuretted hydrogen gas, as well as in the nature of the precipitates it afforded with prussiate of potash and with ammonia, it perfectly resembled the magnetical pyrites. Moreover, by analysis, it was found to consist of 35 parts of sulphur and 65 of iron; and although (being in a pulverulent state) its power, as to receiving and retaining the magnetic property, could not so easily be examined, yet, by being powerfully attracted by the magnet, with some other circumstances, there was every reason to conclude, that in this respect also it was not inferior. It was attractable by the magnet.

Another proportion of sulphuret was formed, as above described; it was placed between magnetical bars, and, in like manner, received and retained the magnetic power.

It is certain, therefore, that when a quantity of sulphur equal to 35 or 37 *per cent.* is combined with iron, it not only does not prevent the iron from receiving the magnetic fluid, but enables it to retain it, so that the mass acts in every respect as a permanent magnet. Hence 36 or 37 per cent. does not prevent iron from becoming a magnet.

Black oxide of iron, by one operation, does not appear to combine with sulphur so readily as iron filings; a second operation, however, converts it into a sulphuret, very much resembling that which has just been described, including the Black oxide of iron combines with sulphur less readily.

chemical

chemical as well as the magnetical properties; but, undoubtedly, by these processes, it is progressively converted, perfectly or very nearly, into the metallic state.

Iron combined with a larger proportion of oxygen, such as the fine gray specular iron from Sweden, will not form a sulphuret by the direct application of sulphur, in one operation; although it becomes of a dark brown colour, partly iridescent, and is moderately attracted by a magnet.

Magnetical pyrites combined with 9 *per cent.* more of sulphur,

Fifty grains of the magnetical pyrites, reduced to powder, and mixed with three times the weight of sulphur, were distilled in a retort, until the bulb became moderately red-hot. After the distillation, the pyrites weighed 54.50; consequently, the addition of sulphur was 9 *per cent.* making the total = 45.50 or 46 *per cent.* The powder was become greenish-yellow, very like that of the common pyrites: it did not afford any sulphuretted hydrogen, when digested in muriatic acid; but it nevertheless was partially dissolved, and the solution, when examined by prussiate of potash, and by ammonia, was not different from that of the crude magnetical pyrites.

—was still attracted by the magnet.

The powder which had been distilled with sulphur, and which had thus received an addition of 9 *per cent.* to its original quantity, *was still capable of being completely taken up by a magnet.*

Iron ceases to be acted on by the magnet when the dose of sulphur is at some point between 46 and 52 *per cent.*

From the whole of the experiments which have been related, it is therefore evident, that iron, when combined with a considerable proportion of sulphur, is not only still capable of receiving the magnetic property, but is also thereby enabled to retain it, and thus (as I have already remarked) becomes a complete magnet; and it is not a little curious, that iron combined (as above stated) with 45 or 46 *per cent.* of sulphur, is capable of being taken up by a magnet, whilst iron combined with 52 *per cent.* or more, of sulphur, (although likewise in the metallic state,) does not sensibly affect the magnetic needle; and hence, small as the difference may appear, there is reason to conclude, that the capacity of iron for magnetic action is destroyed by a certain proportion of sulphur, the effects of which, although little if at all sensible at 46 *per cent.* are yet nearly or quite absolute, in this destruction of magnetic influence, before it amounts to 52. But, what the exact intermediate proportion of sulphur may be, which is adequate to produce this effect, I have not as yet determined by actual experiment.

As

As carbon acts on soft iron, (which, although it most readily receives the magnetic influence, is unable to retain it so as to become a magnet, without the addition of a certain proportion of carbon, by which it is rendered hard and brittle, or, in other words, is converted into steel, so, in like manner, does sulphur seem to act; for it has been proved, by the preceding experiments, that the brittle mass formed by the union of a certain proportion of this substance with iron, whether by nature or by art, becomes capable of retaining the magnetic virtue, and of acting as a complete magnet. Sulphur acts on iron like carbon in causing it to contain magnetism,

This remarkable coincidence, in the effects produced on iron by carbon and sulphur, induced me to try the effects of phosphorus; and my hope of success was increased by the remark of Mr. Pelletier, who says, that "the phosphuret of iron is attracted by the magnet;" * and therefore, although certain bodies may be thus attracted, without being capable of actually becoming permanent magnets, I was desirous to examine what might be the power, in this respect, of phosphuret of iron. —and so likewise does phosphorus.

I therefore prepared a quantity of phosphuret of iron, in the direct way, *viz.* by adding phosphorus, cut into small pieces, to fine iron wire made moderately red-hot in a crucible. The usual phenomena took place, such as the brilliant white flame, and the rapid melting of the iron, which, when cold, was white, with a striated grain, extremely brittle, hard, and completely converted into a phosphuret. The fragments of this were powerfully attracted by a magnet; and, after I had placed two or three of the largest pieces, during a few hours, between magnetical bars, I had the pleasure to find that these had become powerful magnets, which not only attracted or repelled the needle completely round, but were able to take up iron filings, and small pieces, about half an inch in length, of fine harpsichord wire; and, although they have now been removed from the magnetical bars more than three weeks, I cannot discover any diminution of the power which had thus been communicated to them. Experiment. Phosphuret of iron was made; —and by the touch formed a powerful magnet.

The three inflammable substances, *carbon*, *sulphur*, and *phosphorus*, which, by their chemical effects on iron, in many respects resemble each other, have now therefore been proved

* "Le Phosphure de Fer est attirable a l'aimant." Annales de Chimie, Tome XIII. p. 114.

alike

alike to poſſeſs the property of enabling iron to retain the power of magnetiſm; but I ſhall conſider this more fully in the following ſection.

§ VIII.

From the whole which has been ſtated we find,

General reſults.

Magnetic pyrites is a Britiſh mineral,

1. That the ſubſtance called magnetical pyrites, which has hitherto been found only in Saxony and a few other places, is alſo a Britiſh mineral, and that, in Caernarvonſhire, it forms a vein of conſiderable extent, breadth, and depth.

—containing about 37 ſulphur and 63 iron.

2. That the component ingredients of it are ſulphur and metallic iron; the former being in the proportion of 36.50 or 37, and the latter about 63.50 or 63.

It differs in its properties from common pyrites which contains more ſulphur.

3. That the chemical and other properties of this ſubſtance are very different from thoſe of the common martial pyrites, which however are alſo compoſed of ſulphur and iron, varying in proportion, from 52.15 to 54.34 of ſulphur, and from 47.85 to 45.66 of metallic iron; the difference between the common pyrites which were examined being therefore 2.19, and the mean proportions amounting to 53.24 of ſulphur, and 46.75 of iron; conſequently, the difference between the relative proportions, in the compoſition of the magnetical pyrites and of the common pyrites, is nearly 16.74, or 16.24.

It is identically the ſame as the artificial ſulphuret.

4. That, as the magnetical pyrites agrees in analytical reſults, as well as in all chemical and other properties, with that ſulphuret of iron which hitherto has been only known as an artificial product, there is no doubt but that it is identically the ſame; and we may conclude, that its proportions are moſt probably ſubjected to a certain law, (as Mr. Prouſt has obſerved in the caſe of the artificial ſulphuret,) which law, under certain circumſtances, and eſpecially during the natural formation of this ſubſtance in the humid way, may be ſuppoſed to act in an almoſt invariable manner.

In common pyrites the ſulphur predominates.

5. That, in the formation of common martial pyrites, there is a deviation from this law, and that ſulphur becomes the predominant ingredient, which is variable in quantity, but which, by the preſent experiments, has not been found to exceed 54 34 *per cent.* a proportion, however, that poſſibly may be ſurpaſſed in other pyrites, which have not as yet been chemically examined.

6. That

6. That iron, when combined naturally or artificially with 36.50 or 37 of fulphur, is not only ftill capable of receiving the magnetic fluid, but is alfo rendered capable of retaining it, fo as to become in every refpect a permanent magnet; and the fame may, in a great meafure, be inferred refpecting iron which has been artificially combined with 45.50 *per cent.* of fulphur.

Limits of the fulphur in magnetic pyrites.

7. That beyond this proportion of 45.50 or 46 *per cent.* of fulphur, (in the natural common pyrites,) all fufceptibility of the magnetic influence appears to be deftroyed; and, although the precife proportion which is capable of producing this effect, has not yet been determined by actual experiment, it is certain that the limits are between 45.50 and 52.15; unlefs fome unknown alteration has taken place in the ftate of the fulphur, or of the iron in the common martial pyrites.

Proportion beyond which the magnetic influence is loft.

8. That, as carbon, when combined in a certain proportion with iron, (forming fteel,) enables it to become a permanent magnet, and as a certain proportion of fulphur communicates the fame quality to iron, fo alfo were found to be the effects of phofphorus; for the phofphuret of iron, in this refpect, was by much the moft powerful, at leaft when confidered comparatively with fulphuret of iron.

As carbon renders iron tenacious of magnetifm, fo alfo do fulphur and phofphorus.

9. And laftly, that as carbon, fulphur, and phofphorus, produce, by their union with iron, many chemical effects of much fimilarity, fo do each of them, when combined with that metal in certain proportions, not only permit it to receive, but alfo give it the peculiar power of retaining, the magnetical properties; and thus, henceforth, in addition to that carburet of iron called fteel, certain fulphurets and phofphurets of iron may be regarded as bodies peculiarly fufceptible of ftrong magnetical impregnation.

Conclufion.

Having thus, for the greater perfpicuity, reduced the principal facts of this Paper into a concife order, I fhall now make fome general obfervations.

It is undoubtedly not a little fingular, that a fubftance like the magnetical pyrites, which, although not common, has been long known to mineralogifts, fhould not hitherto have been chemically examined, efpecially as mineralogical authors have mentioned the analyfis of it as a defideratum. The refult of this which I have attempted, proves that it is really deferving of notice; for thus we have afcertained, that the fulphuret of iron hitherto known only as an artificial product, is alfo formed

Remarks.
The magnetical pyrites is an interefting product.

by

by nature; and that the compofition of this laft, agrees with thofe proportions of the artificial fulphuret which have been ftated by Mr. Prouft.

No intermediate natural product between the common and the mag. pyrites.

But, from this fulphuret or magnetical pyrites, I have not, by analyfis, as yet been able to difcover any regular or immediate gradations into the common pyrites; for the leaft proportion of fulphur in thefe amounted to 52.15, and the greateft proportion to 54.34; fo that, between the magnetical and the common pyrites, the difference is confiderable, in the proportions of their component fubftances, as well as in their phyfical and chemical properties; whilft the difference which I have hitherto been able to detect in the proportions of fome of the common pyrites (very diffimilar in figure, luftre, colour, and hardnefs,) has only amounted to 2.19.

Remarks on Prouft's experiments.

Mr. Prouft, in a general way, confiders common pyrites to differ from the firft fulphuret, or that compofed of 60 parts of fulphur and 100 of iron, (= 37.50 *per cent.*) by containing a farther addition of half the above quantity of fulphur, or 90 parts of fulphur and 100 of iron, (= 47.36 *per cent.*) but this opinion he appears to have formed, in confequence of refults obtained by fynthetical experiments made in the dry way.—Now, when we confider how difficult it is to regulate the high degrees of temperature, and what a numerous chain of alterations in the relative order of affinities moft commonly refult from alterations in thefe degrees of heat, it feems to me that we cannot rely, with abfolute certainty, on fynthetical experiments made in the above way, unlefs they are corrected, and contrafted with analytical experiments made on the fame fubftances. But it does not appear, from the two memoirs publifhed by Mr. Prouft, to which I have fo frequently alluded, that that gentleman did more, in refpect to analyfis, than diftil the cubic and dodecaedral pyrites found near Soria, from which he obtained about 20 *per cent.* of fulphur; and, having obferved that the refiduum poffeffed the properties of the fulphuret which has been commonly prepared in laboratories, he concluded that the fulphur obtained from the pyrites, is the excefs of that proportion which is requifite to form the fulphuret, the proportions of which, therefore, he by fynthefis afcertained to be, as I have above ftated, = 37.50 of fulphur, and 62.50 of iron, or 60 of fulphur combined with 100 of iron; and laftly, having formed 318 grains of this fulphuret from 200 grains of iron

iron filings, he diftilled the fulphuret with an additional quantity of fulphur, in an inferior degree of heat, and obtained 378 grains of a fubftance which, excepting denfity, was fimilar to the common martial pyrites.*

It is however to be regretted, that Mr. Prouft did not make a regular analyfis of the pyrites of Soria, and of the refiduum after diftillation; for (unlefs thefe pyrites are very different from thofe which I have examined) he would moft probably have found the proportion of fulphur greater than that which he has affigned to natural pyrites in general. This at leaft there is great reafon to fuppofe, if we allow that moft or all of the pyrites have been formed in the humid way, by which, we may conceive, a larger proportion of fulphur may be introduced into the compound, than can take place in high degrees of temperature. And this opinion is corroborated by the refults of my analyfes; for, inftead of finding the general proportions to be 47.36 of fulphur and 52.64 of iron, the mean refult of thefe analyfes is very nearly the reverfe, being 53.24 of fulphur and 46.76 of iron.

He did not make a regular analyfis.

Mr. Prouft is alfo of opinion, that the pyrites which contain the fmalleft quantity of fulphur, are thofe which are moft liable to vitriolization; and, on the contrary, that thofe which contain the largeft proportion, are the leaft affected by the air or weather.† This opinion of the learned profeffor, by no means accords with fuch obfervations as I have been able to make; for the cubic, dodecaedral, and other regularly cryftallized pyrites, are liable to oxidizement, fo as to become what are called hepatic iron ores, but not to vitriolization; whilft the radiated pyrites (at leaft thofe of this country) are by much the moft fubject to the latter effect; and therefore, as the refults of the preceding analyfes fhow that the cryftallized pyrites contain lefs fulphur than the radiated pyrites, I might be induced to adopt the contrary opinion. But I am inclined to attribute the effect of vitriolization obferved in fome of the pyrites, not fo much to the proportion, as to the ftate of the fulphur in the compound; for I much fufpect, that a predifpofition to vitriolization, in thefe pyrites, is produced by a fmall portion of oxygen being previoufly combined with a part, or with the general mafs,

Prouft apprehends that pyrites holding the leaft fulphur are moft liable to vitriolization:

But this difpofition moft probably arifes from a commencement of oxidation.

* *Journal de Phyfique*, Tome LIV. p. 92.

† *Journal de Phyfique*, Tome LIII. p. 91.

of the ſulphur, at the time of the original formation of theſe ſubſtances, ſo that the ſtate of the ſulphur is tending to that of oxide, and thus the acceſſion of a farther addition of oxygen becomes facilitated. We have an example of ſimilar effects in phoſphorus, when (as is commonly ſaid) it is half burned, for the purpoſe of preparing the phoſphorus bottles; and the propenſity to vitriolization, obſerved in many of the half-roaſted ſulphureous ores, appears to me to ariſe from this cauſe, rather than from the mere diminution of the original proportion of ſulphur, or the actual immediate converſion of part of it into ſulphuric acid; nevertheleſs, I offer this opinion, at preſent, only as a probable conjecture, which may be inveſtigated by future experiments and obſervations.

The magnetical properties of the ſulphuret is a remarkable fact,

The magnetical properties of the ſulphuret of iron which forms the principal ſubject of this Paper, muſt be regarded as a remarkable fact; for I have not found, in the various publications on magnetiſm which I have had the means of conſulting, even the moſt remote hint, that iron when combined with ſulphur, is poſſeſſed of the power of receiving and retaining the magnetic fluid; and, judging by the properties of common pyrites, we might have ſuppoſed that ſulphur annihilated this power in iron, as indeed ſeems to have been the opinion of mineralogiſts, who have never enumerated magnetical attraction amongſt the phyſical properties of thoſe bodies; and, although Werner, Widenmann, Emmerling, and Brochant, have arranged the magnetical pyrites with the ſulphurets of iron, yet the magnetical property could not with certainty be ſtated as inherent in the ſulphuret, for, at that time, this ſubſtance had not been ſubjected to a regular chemical analyſis, and the magnetical property might therefore be ſuſpected to ariſe from interſperſed particles of the common magnetical iron ore. This probably has been the opinion of the Abbé Haüy; for, in his extenſive Treatiſe on Mineralogy lately publiſhed, I cannot find any mention made of the magnetical pyrites, either amongſt the ſulphurets or amongſt the other ores of iron.

little or ſcarcely noticed.

The magnet ſaid to conſiſt of iron with 10 to 20 oxigen.

In the mineral kingdom, a great variety of ſubſtances, and even ſome of the gems, exert a feeble degree of attraction on the magnetic needle, and ſometimes alſo acquire a ſlight degree of polarity;* but, as this wonderful property has only been

* Cavallo on Magnetiſm, page 73.

obſerved

obſerved conſpicuouſly powerful in one ſpecies of iron ore, this has been always emphatically called *the Magnet,** and is ſaid to conſiſt of metallic iron combined with from 10 to 20 *per cent.* of oxygen.

From the facts, however, which have been recently ſtated, we now find that there is another natural ſubſtance, apparently very different from the magnet in chemical compoſition, but nevertheleſs approaching very nearly to it in power, which is found in ſeveral parts of our globe, and particularly in a province of this kingdom, where it conſtitutes a vein, running north and ſouth, of a conſiderable extent, and ſeveral yards in width and thickneſs. The ſulphuret is a different kind of magnet.

From the experiments alſo, which have been made on the artificial preparation of this ſubſtance, we find, that it is capable of receiving the magnetic properties when the proportion of ſulphur amounts to 37 *per cent.* and is ſtill powerfully attracted when a much larger quantity of ſulphur is preſent. There is, however, ſome point at which all theſe effects ceaſe, and this point appears to be, when the ſulphur is in ſome proportion between 45 or 46 and 52 *per cent.* The preceding experiments have alſo proved, that iron when combined with phoſphorus, likewiſe poſſeſſes the power of becoming a magnet to a very remarkable degree; and, by the ſimilarity, in this reſpect, of the carburet of iron called ſteel, to the above ſulphuret and phoſphuret, a very remarkable analogy is eſtabliſhed between the effects produced on iron, by carbon, ſulphur, and phoſphorus. The phoſphuret magnet.

Carbon, when combined in a very large proportion with iron, forms the carburet of that metal called plumbago; a brittle ſubſtance, inſoluble in muriatic acid, and deſtitute of magnetical properties. But, ſmaller proportions of carbon, with the ſame metal, conſtitute the various carburets included between black caſt iron and ſoft caſt ſteel;† bodies which are more or leſs Of carburets, plumbago is not magnetical: other ſteel compounds form powerful magnets.

* In a future Paper, it is my intention to give an account of ſome comparative analyſes of the varieties of this ſubſtance.

† "When the carbon exceeds, the compound is carburet of iron "or plumbago: when the iron exceeds, the compound is ſteel, or "caſt iron, in various ſtates, according to the proportion. All "theſe compounds may be conſidered as ſubcarburets of iron."— Thomſon's Syſtem of Chemiſtry, Vol. I. p. 165.

Mr.

lefs brittle, foluble in muriatic acid, and more or lefs fufceptible of magnetical impregnation; fome of them form the moft powerful magnets hitherto difcovered.

Sulphur and iron have fimilar habitudes.

Sulphur, in like manner, combines with iron in a large proportion, forming the common pyrites, which are brittle, almoft or quite infoluble in muriatic acid, and devoid of magnetical properties. Sulphur in fmaller proportions, forms fulphurets which are alfo brittle, but are foluble in muriatic acid, and ftrongly fufceptible of magnetical impregnation.

So likewife phofphorus and iron.

Phofphorus alfo, when combined with iron, makes it brittle, and enables it powerfully to receive and retain the magnetical properties; fo that, confidering the great fimilarity which prevails in other refpects, it may not feem rafh to conclude, that phofphorus (like carbon and fulphur,) when combined with iron in a very large proportion, may form a fubftance incapable of becoming magnetical, although, in fmaller proportion, (as we have feen,) it conftitutes compounds which are not only capable of receiving, but alfo of retaining, the magnetical properties, even fo far as, in fome cafes, to feem likely to form magnets of great power; and, fpeaking generally of the carburets, fulphurets, and phofphurets of iron, I have no doubt but that, by accurate experiments, we fhall find that a certain proportion of the ingredients of each, conftitutes a maximum in the magnetical power of thefe three bodies. When this maximum has been afcertained, it would be proper to compare the relative magnetical power of fteel (which hitherto has alone been employed

Mr. Mufhet, in the following Table, exhibits the proportion of charcoal which difappeared, during the converfion of iron to the different varieties of fubcarburet known in commerce.

" Charcoal abforbed.	Refult.
$\frac{1}{120}$ - - -	Soft caft fteel.
$\frac{1}{100}$ - - -	Common caft fteel.
$\frac{1}{90}$ - - -	The fame, but harder.
$\frac{1}{50}$ - - -	The fame, too hard for drawing.
$\frac{1}{25}$ - - -	White caft iron.
$\frac{1}{20}$ - - -	Mottled caft iron.
$\frac{1}{15}$ - - -	Black caft iron.

" When the carbon amounts to about $\frac{1}{60}$ of the whole mafs, the " hardnefs is at the maximum." Thomfon, Vol. I. p. 166; and Phil. Magazine, Vol. XIII. pp. 142 and 148.

ployed to form artificial magnets) with that of fulphuret and phofphuret of iron; each being firft examined in the form of a fingle mafs or bar of equal weight, and afterwards in the ftate of compound magnets, formed like the large horfe-fhoe magnets, by the feparate arrangement of an equal number of bars of the fame fubftance in a box of brafs.

The effects of the above compound magnets fhould then be tried againft others, compofed of bars of the three different fubftances, various in number, and in the mode of arrangement; and, laftly, it would be interefting to make a feries of experiments on chemical compounds, formed by uniting different proportions of carbon, fulphur, and phofphorus, with one and the fame mafs of iron. Thefe quadruple compounds, which, according to the modern chemical nomenclature, may be called carburo-fulphuro phofphurets, or phofphuro-fulphuro-carburets, &c. of iron, are as yet unknown as to their chemical properties, and may alfo, by the inveftigation of their magnetical properties, afford fome curious refults. At any rate, an unexplored field of extenfive refearch appears to be opened, which poffibly may furnifh important additions to the hiftory of magnetifm, a branch of fcience which of late years has been but little augmented, and which, amidft the prefent rapid progrefs of human knowledge, remains immerfed in confiderable obfcurity.

An extenfive field for experiments of intereft and importance.

III.

Extract of a Memoir of Mr. ERMAN, *entitled Obfervations and Doubts concerning Atmofpheric Electricity.*

(Concluded from p. 300, Vol. X.)

AN electrometer furnifhed with a rod three feet in length, and placed in the open air, does not fhew any divergence; but when the bent point of another electrometer, which alfo exhibits no divergence, is moved above the firft, and even when the motion is parallel to the horizon, the leaves of the latter will be feen to diverge negatively, without the fecond giving any fign of electricity.

Two electrometers which do not indicate divergence alone, fhew it when one is paffed above the other.

It is very probable that the effect of thefe vapourous and aqueous meteorological maffes is manifefted at the points of the electrometer,

This effect probably arifes from the divifion of the electricity.

electrometer, by this same action, which depends on the division of the electricity, and this explains the sudden changes which supervene in the positive or negative electric state of the earth: it is not even probable that the clouds possess in themselves a negative electricity, or that the vapours of water should be always in this state. Mr. Erman thinks he can prove that the vapours of water are only negative when the body from which they proceed is insulated; but that they become positive as soon as it is brought into contact with the earth. Rain, after its fall, leaves bodies in the state of *minus* electricity, which is agreeable to the preceding experiments; snow produces this effect so much the better, as it leaves the surrounding air in a state of dryness, which augments its insulating properties.

The positive or negative state of aqueous vapour depends on insulation.

Influence of the clouds on the earth.

The clouds which have a tendency to rain or snow must necessarily produce their influence on the ground, and it is for this reason that the opposition of the cloud and the ground is so quickly manifested. It would be interesting to explain the complication arising from the variations which the electricity of the earth undergoes, as well with respect to its species as to its intensity, and Mr. Erman is at present engaged in this subject.

The different phenomena of the divergence of an insulated point explained by the same law.

With respect to the variable degree of the positive divergence of an insulated point fixed in the earth, it may perhaps be attributed to the greater or less conducting quality of the surrounding air: the point, when it is very well insulated from the earth, shows a certain degree of positive charge when the air is insulated so that electricity cannot be communicated to it; it shows zero when the conducting power of the air is nearly equal to the rapidity with which the charge is made; and negative when this power is very strong. These phenomena are thus explained by the same law.

Probability that there is no free electricity in the atmosphere.

Mr. Erman has also supposed in his memoir that there is not any quantity of electricity disengaged in the atmosphere; but he does not however assert that this is strictly the case: he has only sought to draw the attention of philosophers to this subject, and to show that these phenomena are equally well explained by the sphere of activity of the electricity.

Convenience of the instruments.

The experiments are very easily made, and the electrometers he uses are very portable; for when the leaves of gold are placed on one side of the cylinder, motion cannot occasion any accident. With respect to the sticks, they may be made of several feveral pieces, and be fcrewed together when they are wanted. Such were the inftruments Mr. Erman took with him over feveral hundred leagues; the ftick was three lines in diameter at one end, and one line at the other. This philofopher does not yet know what may be the influence of the different thicknefs of the conductor. One of thefe conductors happened to be of brafs, the other of fteel; it did not appear to him that the difference of the metal occafioned any in the refults. But he purpofes making new enquiries on this fubject.

Mr. Erman offers his conclufions with great diffidence: he does not pretend to have formed a new theory, but only to ftate his doubts on the opinion of thofe philofophers who have attributed the phenomena hitherto obferved to a difengaged electricity in the atmofphere. He notices feveral other interefting experiments which he is employed in making upon fmoke, and particularly on electricity in a vacuum. We fhall haften to publifh the refults of them as foon as they come to hand.

IV.

Defcription of a Compenfation Curb. By Mr. JAMES SCOTT.

To Mr. NICHOLSON.

SIR,

INCLOSED I fend you a fketch of an inftrument I have conftructed, much wanted in the profeffion of watchmaking, for the purpofe of publifhing in your Philofophical Journal, which I prefume will be of much benefit to the public, and at the fame time may prevent any other perfon from claiming it. Introductory letter.

I fhall call it a compenfation curb. The conftruction of this inftrument is for the purpofe of expanding and contracting in the different temperatures, fo as to counteract the error which the pendulum-fpring is liable to by the fmalleft variation of heat or cold.

The inventions that have hitherto been put in practice for the fame purpofe, I beg leave to make a few remarks on.—The compound balance, when carefully made and adjufted, is certainly a very complete counteracting expanfion, and will anfwer exceedingly well on board a fhip, if there be no material difference in the denfity of the air; if otherwife, the balance being loaded, will have to encounter a confiderable deal more friction, and confequently be impeded in its vibrations: if worn in the pocket, it is alfo liable to error, as exercife will alter its diameter. The compound balance has heretofore got the preference, becaufe artifts have not been able to invent a compenfation curb adjuftable to the exact expanfion required (which, by many experiments, I have proved the inclofed to be fully competent to); therefore, the plain balance having no projections on the furface, muft certainly have the advantage. As I am not in the habit of expreffing my ideas to the public, I hope you will have the goodnefs to rectify any errors in the ftile, and alfo curtail any part which may appear unneceffary to you.

I remain, Sir,

Your much obliged humble fervant,

JAMES SCOTT.

39, *Grafton Street, Dublin.*

The following is a Defcription of the Compenfation Curb.

Defcription of a compenfation curb.

The fteel index, *Pl. II. Fig.* 2, letter A, is for the purpofe of fupporting the curb, which is faftened by a fcrew and fteady pin at R, the circle of which at A is turned with a dovetail, as fhewn at H, and is flit fo as to fnap into the frame-plate, by which means it may be turned, and will carry the curb, fo as to regulate the machine in the common way. D and E are two circles compofed of brafs and fteel foldered together, the outfide of E brafs and the infide fteel; but the outfide of D is fteel and the infide brafs; fo that the one circle expands when the other contracts; by which means the acting part of the curb at C will fhift towards the index with heat, and prevent the vibrations being flower, which the expanfion of P, the regulating fpring and the balance, would otherwife occafion; and on the contrary with cold, it will fhift its pofition nearer the ftud I, which the regulating fpring is pinned to; fo that let it receive heat or cold, the acting part of the curb at C will at all times keep the regulating fpring the exact length, to counteract the expanfion of the balance and pendulum-fpring. V is a piece of fteel, with a notch cut in it to receive the expanfion circles D and E. F is a fcrew for faftening V at any part of the circles; fo that, by ftoving the watch,

 you

you will immediately afcertain towards what part of the circles you muft fhift V. If you find its rate flow with heat, you muft fhift V to lengthen your circles, and if faft, the contrary; and by marking your circles each time you have occafion to fhift V, yon will be enabled to adjuft the curb to the exact expanfion required. W is a loofe piece of fteel, which fits in the notch of V between the two expanfion circles, to keep them faft in their proper pofitions when fcrewed by F. S S are two fteady pins made faft in the frame-plate, which receive the circle E between them to prevent the action of P, the regulating fpring, from affecting C, the curb, by moving it to or from the center during the going of the watch. The two expanfion circles require to be made very delicate; it is therefore to be obferved by the manufacturer of this inftrument, that the brafs in each circle is to be the thicknefs of the fteel; fo that when the two bodies are foldered together, they will make two thickneffes of the edge of a main-fpring of a watch. The larger your watch will admit the diameter of the curb, the better. It muft be at leaft the fize of the balance.

Defcription of a compenfation curb.

V.

Letter from Mr. BOSWELL, *in Anfwer to* AN OLD CORRESPONDENT.

To Mr. NICHOLSON.

SIR, *April* 10, 1805.

THE perfon figning *your Old Correfpondent*, has thrown fome very undeferved reflections on my laft communication in your Journal.

Mr. Bofwell vindicates himfelf from confidence.

In anfwer to his remarks, I beg leave to obferve that the introduction to my paper contains fufficient to juftify me fully from his imputations.

I have there afferted no more than that "I have difcovered "a method of coming fo *near the truth*, that fhould it turn "out to be in reality *not fo exact* as appears to me, yet it pro-"mifes to be fo ufeful for *common computations* that I am "induced to fend it for publication, *if you approve of it*."

I know

I know not, Sir, how I could have written in any way farther from " announcing my difcovery with confidence" than the above, or indeed with more diffidence.

His methods were ftated as *nearly approaching the truth.*

n the firft place, I ftate it only as a *near approach* to the exact truth, which both your and his criticifms have fully proved it to be: In the next place, I exprefs my doubt of its being even *fo exact* as it appears to me: And thirdly, I have left its publication to depend *on your approbation.*

If this is not enough to remove all fufpicion of confidence, I have to add, that the fentence which concludes that fubject in the paper, ftates, that an unperceived error might arife from the fmallnefs of the circles which I ufed; and befides this you can teftify for me, that I wifhed the paper to be fuppreffed altogether before publication, when you fhewed me that it even wanted an hundredth part of being exact, though with a much greater inaccuracy the matter contained in it would be ufeful for the purpofes to which I ftated it might be applied.

As to the proof of the fecond *fact*, it was not from the " conviction of its obvious accuracy not requiring proof" that I did not infert any, but becaufe I concluded it muft be fufficiently obvious after what I had ftated of the firft, that it was the fame fort of *experimental* proof I had ufed for both: And if any gentleman will try the experiment as I did, he will find I have not misftated the matter.

There may indeed be fome little impropriety in ufing the word *fact* in a popular fenfe, in any thing like a mathematical ftatement; but to notice fuch a trifle with inverted commas, only appears to indicate a fpirit of cavilling on the part of your Old Correfpondent.

None of the quadratures of the circle are more than approximations.

But with all his precifion he has forgot one fact, that puts his computations more on a level with my experiments, which is, that no method has ever yet been difcovered of computing with *perfect exactnefs* the relative proportions of the circumference to the diameter, and of courfe to the other lines he mentions; and that it is only a far-laboured *approximation* to the truth that has been inferted in the work from whence he has extracted the proportions which he has ufed; and that therefore what he has " announced with fo much confi-" dence," (to ufe his own words) is not precifely demonftrative truth, but only an approach to it.

I did

I did not ever intend to recommend my method as a perfect and infallible geometrical problem applicable to the more sublime mathematical speculations, but as a more ready, and, permit me to add, more exact way than that in general use, for the humbler purposes of common life, such as measuring round timber, conduit pipes, engine cylinders, &c.

Mr. B.'s methods have practical utility.

I am, &c.

J. WHITLEY BOSWELL.

VI.

Description of an improved Gate for Fields. By Mr. CHARLES WAISTELL *.

DEAR SIR,

THE various methods used in bracing common gates for fields, prove that not one of them is greatly superior to the rest; for, if it was, that method would have been generally adopted. Most gates are loaded with superfluous timber in some of their parts, and are constructed upon such bad principles, that they are frequently broken by their own weight, aided by the concussion of the head against the falling-post; and this, long before any part of the wood has begun to decay. I have for some time given this subject considerable attention, being impressed with the idea, that if common gates could be constructed with less timber, and upon better principles, the saving of timber only would be of national importance; for we have many millions of gates to uphold in Britain, and their numbers are annually increasing. The result of my labours has been the plan which accompanies this letter. Gates made according to it, possess great strength, are very light, and of easy and simple construction. Although uniformity of appearance be not essential in a common gate, yet is worth having when it can be obtained, as in this gate, without additional expense.

Imperfections of common gates.

Great saving in timber if they were made less cumbersome.

New plan.

My gate is made with short, and consequently less valuable, oak or ash timber, than those of the commonest construction; its strength is much greater than any other gate made with a like

Account of the author's improvements.

* Communicated in a letter to Charles Taylor, Esq. Secretary of the Society of Arts, who returned their thanks for the same.

quantity

quantity of timber, there being at four diſtant points between the head and the heel, two bars and a brace croſſing each other: and I doubt not that it will be found proportionably more durable: it is, beſides, very eaſy to conſtruct, and requires leſs labour than moſt other common gates. Twenty-nine years ago I deſigned plans for ornamenſal gates, with ſemi-oval and ſemicircular braces, and had them executed; the plans were ſent to my friends in various diſtant parts of this kingdom, as alſo to Ireland; and I have the pleaſure to obſerve, that they are become almoſt the only ornamental gate in many parts of England. The plans of them I never publiſhed, although they were prepared for engraving fifteen years ago; and I ſhould be as indifferent about my preſent deſign, of a common field gate, if I did not conceive that its publication would materially benefit the public; the introduction of this form being, I conceive, of ſome national importance, as timber has been lately greatly enhanced in price, and is rapidly on the advance.

This gate was deſigned for the approach to a country reſidence; but for common purpoſes, the wicket on one hand, and the ſhort length of rails on the other, may be omitted. I ſhall thank you, if you will have the goodneſs to lay my plan before your reſpectable Society, of which I have, for many years, had the honour to be a member. And ſhould this plan be approved of, I may probably furniſh ſome deſigns for park gates on an improved conſtruction.

I am, Dear Sir,

Your very humble Servant,

CHARLES WAISTELL.

March 22, 1803.

Mr. Charles Taylor.

Reference to the Engraving of Mr. Waiſtell's Gate.

DIMENSIONS.—*(Plate II. Fig. 1.)*

Deſcription and dimenſions of the new conſtructed field-gate.

The heel of the gate to be about	$3\frac{1}{2}$	inches ſquare.
The head of ditto - -	$2\frac{1}{2}$	by 3 inches.
The top rail or bar - -	$3\frac{1}{2}$	by $1\frac{1}{2}$ inches.
The bottom bar - -	$3\frac{1}{2}$	by $1\frac{1}{4}$ inches.
The bar in the middle of the gate	3	by $1\frac{1}{4}$ inches.
The other bars, and the 4 braces	$2\frac{1}{2}$	by $1\frac{1}{4}$ inches.

Obſervations

Obſervations on its Conſtruction.

The head and heel of the gate may be of oak, and the bars and braces of fir. Narrow and thick bars, when braced as in this deſign, are ſtronger than broad and thin ones, containing the ſame quantity of timber, and they alſo oppoſe a leſs ſurface to the wind. The two points in the heel of the gate, to which the thimbles are faſtened, may be conſidered as firm or fixed points. From theſe points, viz. 1 and 2, two braces to proceed to 4 and 3, in the middle of the bottom and top bars, and being there ſecured, theſe become fixed points, and from theſe two points, viz. 4 and 3, two braces proceed to 5 and 6, fixing thoſe points. The gate is thus doubly braced, viz. from the top of the heel to the top of the head, by means of the braces 1, 4, and 4, 5; and from the bottom of the heel to the bottom of the head, by means of the braces 2, 3, and 3, 6. On each ſide of the gate are two braces, and thoſe parallel to each other. The brace proceeding from the bottom of the heel of the gate, and that which is parallel to it, as alſo the bottom bar, are all ſtrained in the way of compreſſion, and the brace proceeding from the top of the heel, and the other brace which is parallel to it, and alſo the top bar, are all ſtrained in the way of extenſion. The ſtrains in this gate being none of them tranverſe, but all longitudinal, it would ſupport a vaſt weight at its head without having its form altered. The braces all ſerve the double purpoſe of keeping the gate in its true form, and of ſhortening the bearings of the bars, and ſtrengthening them. Few gates have leſs timber in their braces; and perhaps in no other way can a gate be ſo firmly braced with ſo ſmall a quantity of timber.

Deſcription and dimenſions of the new conſtructed field-gate.

At 3, 4, 7, and 8, two braces and a bar of the gate are firmly ſcrewed together by means of iron pins and ſcrew nuts. At the other points, where only one brace croſſes a bar, common gate-nails are uſed.

If, in ſome caſes, a ſtrong top-bar be wanted, to reſiſt the preſſure of heavy cattle, a bar or board, about ſix inches broad, and one inch thick, may be laid with its broad ſide upon the top bar, and fixed thereto by means of the ends of the braces in the middle, and by the heel and head of the gate at the two ends of it. This board will, in this poſition, reſiſt exactly the ſame

ſame preſſure as a thick top bar, three inches broad, by four inches deep, although it contain no more than half the timber.

In the ground plan, or horizontal ſection, *Fig*. 7. repreſents a piece of wood, about four inches cube, pinned to the falling poſt, a little below the catch, to ſtop the gate from ſwinging beyond the poſt: another ſtop near the ground may be uſeful.

When gates are hung to open one way only, their heels and heads generally reſt againſt the hanging and falling poſts; but when they are hung according to this deſign, gates may be made about one foot ſhorter for the ſame opening, and conſequently they muſt be lighter, ſtronger, and leſs expenſive.

Of the hanging of Gates.

Hanging of gates.

When the two hooks in the hanging-poſt are placed in the ſame perpendicular line, a gate, like a door, will reſt in any direction in which it may be placed. But, in order that a gate may ſhut itſelf when thrown open, the hooks are not placed exactly perpendicular; the upper hook declining a little towards the falling-poſt, or a few feet beyond it. In whatever direction that hook declines the fartheſt, in the ſame direction will the gate reſt, if unobſtructed, and its head cannot then ſink any lower. Make the head deſcribe half a circle, and it will thus have attained its utmoſt elevation, and will be equally inclined to deſcend either to the right or to the left*.

Particular deſcription of the method of hanging a gate.

The following method of fixing the hooks and thimbles, will, I think, be found to anſwer very well for a gate that is intended to open only one way. Suppoſing the face of the hanging-poſt to be ſet perpendicular, and the upper hook driven in near its inner angle, as is repreſented in the preceding deſign, and that the lower hook muſt be four feet and a half below it; ſuſpend a plumb-line from the upper hook, and at four feet and a half mark the poſt; then at one inch and a half farther from the gateway than this mark, drive in the lower hook; this hook muſt project about half an inch farther from the face of the poſt than the upper hook. In the ſection or ground-plan of the

* See Chap. II. of Mr. Parker's Eſſay on the Hanging of Gates; and alſo the Agricultural Report for Northumberland, by Meſſrs. Bailey and Culley.

gate,

Particular description of the method of hanging a gate.

gate, the two white circles near the hanging-poſt repreſent the places of the two hooks when brought to the ſame horizontal line; that neareſt the gateway repreſents the place of the upper hook. A line drawn through the middle of theſe two circles, and extended each way, will, on one hand, repreſent the gate's natural line of reſt, and, on the other, the line of its higheſt elevation. A gate thus hung will, when thrown open nearly to the line of its higheſt elevation, return to the falling-poſt with a velocity ſufficient to reſiſt a moderately ſtrong wind. This velocity will be either increaſed or diminiſhed, accordingly as the upper hook declines more or leſs from a poſition perpendicular to the lower hook. In order to adapt the thimbles to theſe hooks;—as the lower hook is one inch and a half farther from the gateway than the upper hook, the lower thimble muſt have its eye an inch and a half farther from the heel of the gate than the eye of the upper thimble, in order that the bars of the gate may be in a horizontal poſition when it is ſhut. And, as the upper hook projects half an inch leſs from the hanging-poſt than the lower hook, the upper thimble ſhould be fixed half an inch nearer the farther ſide of the heel of the gate than the lower thimble, in order that the gate may be in a perpendicular poſition when ſhut. If the thimbles have ſtraps embracing the heel of the gate, and proceeding a few inches along each ſide of the bottom and top bars, and if they are fixed to the heel bars and braces, by means of iron pins and ſcrew nuts, great firmneſs will be given to the gate at thoſe two points, which are thoſe that ſuffer the greateſt ſtrains.

*** To this communication are annexed a certificate from Mr. Edward Simpſon of Wooden Croft Lodge, near Barnard Caſtle, in favour of the advantage of theſe gates in ſaving and durability; and alſo a letter from Mr. T. N. Parker, author of a well-known Treatiſe on the hanging of Gates, expreſſing his approbation of the ſame.

Deſcription

VII.

Defcription of an improved Gun-Lock, by Mr. GEORGE DODD.*

Defcription of an improved gun lock, by reference to the drawings.

THE figures in *Plate* I. reprefent Mr. Dodd's improved gun-lock with its parts in their feveral fituations and pofitions. The fhaded drawing, *Fig.* 1. reprefents the exterior parts of the lock; and *Fig.* 2. reprefents its interior. *Fig.* 6. exhibits, in perfpective, the tumbler, the fear and the fear fpring in the pofition of whole cock. The outline plans 3, 4 and 5 fhew the feveral pofitions of the parts, at full cock, half cock, and immediately after the difcharge. The tumbler A terminates on the lower fide in a tail, as ufual; upon which the main fpring acts: but, on the upper or oppofite fide, it is formed fo as to have two notches or bents, one very deep for the half cock, and the other fhallower for the whole cock, as is feen in the figures. The circle *a*, *Fig.* 3, fuppofed to be defcribed by the extremity of the bearing face of the tumbler at whole cock, is larger than that through which the extremity of the bearing face of half cock paffes, fee *Fig.* 5. and the center of the fear B is placed in the outer of thefe two circles, having the under fide of its nofe fafhioned in the arc of the circle *b b*, defcribed by the motion of its extremity. The bearing furfaces of the bents or notches of half and of whole cock are made to fit this face; or, in other words, they form parts of the fame circle, when refpectively at whole or half cock. D is the trigger, fo formed and placed that, at whole cock it trips or draws out the fear, with great facility and quicknefs, by the action of an inner flope or face lying in the direction of a radius of the circle it defcribes; (fee *Fig.* 4.) But when at half cock, *Fig.* 3, its action, by means of an outer flope or furface (which lies intermediate between radii drawn from the centers of the fear and of the trigger to the inner point of their contact) is fo far from difcharging the motion, that it tends to keep the fear more ftrongly in its place. Thefe actions and properties are fufficiently evident from the figures.

Enumeration of the good qualities of this lock.

The advantages of this lock are, 1. It is fully as fimple in its conftruction, or rather more fo, than the common lock, and is therefore no lefs cheap and eafy to be cleaned by a common

* From the Tranfactions of the Society of Arts for 1804.

foldier

ſoldier or workman: 2. It is diſcharged very ſpeedily, and cannot poſſibly catch or hang at the half cock in the act of diſcharging: 3. The bearing parts at half cock are extremely ſtrong and cannot miſs their hold or be thrown out of taking by any accident. In particular the trigger cannot be made to remove the ſear; becauſe its action at half cock is in the contrary direction. Hence it is much more ſimple and its means of ſafety are nearly as ſecure as any bolt, that is to ſay it is perfectly effectual as far as regards the trigger; though it does not, like ſome of the bolt ſtops, prevent the full cock being made. But on the other hand, as the inventor remarks, its ſecurity in no reſpect diminiſhes its ready uſe. For bolts, excluſive of the additional expence, have the diſadvantage of requiring a previous operation before the piece is fit for ſervice. Few people when alarmed have the preſence of mind to unbolt, but they inſtantly attempt to cock. Diſappointment tends to produce that agitation and confuſion of mind which, at ſuch a juncture, may occaſion the loſs of their lives from opponents who are little diſpoſed to ſhew mercy to an enemy from whom they had no reaſon to expect any.

Certificates of the utility and novelty of this invention, from reſpectable makers, with letters of approbation from the Board of Ordnance, were exhibited to the Society of Arts; and it is probable that this apparatus will obtain the encouragement it appears to deſerve. The Society expreſſed their ſentiments by awarding the ſilver medal with the ſum of ten guineas to the inventor.

VIII.

Inveſtigation of the Properties of the Lines drawn in a Circle by Mr. BOSWELL *in the Tenth Volume of this Journal. By Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR, *Middleſhaw, April* 17, 1805.

Propoſitions reſpecting the circle's area, &c.

THE theorems reſpecting the circle, given in your number for March laſt by the ingenious Mr. Boſwell, will undoubtedly prove uſeful to the artiſt and practical philoſopher. On this account

Propositions respecting the circle's area, &c.

account they ought to be made as correct as possible; which has not been as yet done, either by Mr. Boswell, or by his more scientific commentator in your number for the present month. This declaration in a manner compels me to undertake the following investigation of the subject, in which I shall refer to *Fig.* 4. *Plate* XI. of your Journal for April, requesting the reader to place the letter T at the upper extremity of the vertical diameter, and C at the opposite end.

Theorem 1st. Let the circle I T F C have unity for its diameter; draw the diameters I F, T C at right angles to each other; bisect the radius T O in W; join I W, and produce it until it meets the circle again in B: these things being done, the square upon I B is equal to .8000; which exceeds .7854, or the common expression for the area I T F C by the fraction .0146.

Demonstration. Put the radius $IO = R = .5$; then $OW = \frac{1}{2}R$; since the triangle I O W is right angled at O, by hypothesis, $IW^2 = R^2 + \frac{1}{4}R^2 = \frac{5R^2}{4}$, *Euc.* 47.1. Now the triangles O I W, B I F are equiangular; because the angle at I is common to both; and the angle F B I is equal to W O I, being right, *Euc.* 31. III; consequently, as $WI : IO :: IF (= 2IO) : IB$; hence as $WI^2 \left(= \frac{5R^2}{4}\right) : IO^2 (= R^2) :: IF^2 (= 4R^2) : IB^2 \left(= \frac{16R^2}{5}\right)$; but $R^2 = .25$; therefore $IB^2 = \frac{16 \times .25}{5} = 16 \times .05 = .8000$. But $.8000 - .7854 = .0146$. Q. E. D.

Lemma. The area of the circle I T F C is equal to the rectangle under the radius O I, and the semi-circumference I T F; for this area is equal to a triangle having I O for its altitude, and the whole circumference I T F C for its base. (*Archimides de Circulo Prop.* 1st.)

Theorem 2nd. If the right line I B be the side of a square, which is equal to the area I T F C; and B G be drawn perpendicular to the diameter I F; the segment I G of the diameter I F, cut off by B G, is equal to $\frac{1}{4}$ of the circumference I T F C or the arc T B F.

Demonstration. The triangle I B F is right angled at B, 31 *E.* III. and B G is perpendicular to I F by hypothesis therefore

therefore the rectangle F I, I G = I B^2, 8. *E.* VI.; but I B^2 = the rectangle under I O and the arc I F F by lemma; consequently as F I (= 2 I O) : I O : : arc I T F: right line I G; 14 *E.* VI. hence I G = ½ the arc I T F, = the arc T B F, Q. E. D. Propofitions refpecting the circle's area, &c.

Corollary 1. If I G, a fegment of I F be equal to the arc T B F; draw G B perpendicular to I F; and let it meet the circle in B; the line I B is the fide of a fquare; which is equal to the area I T F C. This is the converfe of the Theorem.

Cor. 2. If any angle at the centre of the circle as I O B, be given in parts of the right angle T O F; and I G be equal to the arc T B F; find a right line N; which fhall be a fourth proportional to the angles T O F, I O B and the right line I G; this line N is equal to the arc I T B; and a mean proportional betwixt I O, and $\frac{N}{2}$, is the fide of a fquare which is equal to the area I O B I; join I B, and from the laft mentioned fquare take the trinangle I B O; the remaining magnitude is equal to the circular fegment I T B.

Cor. 3. The fquare upon B F is equal to the difference of the areas of the circle and its circumfcribing fquare.

Problem. If the circumference of a circle, whofe diameter is unity be denoted by 3.1416; it is required to find a right line which fhall approximate very nearly to ¼ of this number, or .7854.

Conftruction. Draw I B, as in Theorem 1ft, and make B G perpendicular to I F; then I G × 1 = I B^2 = .8000; confequently I G = .8000, which is greater than .7854. Let the reader take *g* in I G, fo that I *g* may be of the required length; then as 8000 : 7854 : : I G : I *g*; but 8000 is to 7854 nearly as 55 to 54; therefore divide I G into 55 parts and I *g* will be 54 of thefe parts. Draw *g b* perpendicular to I G; join I *b*; and the fquare upon I *b* will be nearer the truth than that upon I B. If a more complete approximation be required, it may be difcovered by the method given in the ninth Problem of Emerfon's Arithmetic. Q. E. F.

It is the bufinefs of the practical geometrician to determine the value of thefe propofitions in practical geometry. The ingenious Mr. Bofwell confiders the firft theorem to be of utility; for which reafon I imagine any improvement in the

difcovery

discovery will prove acceptable not only to the inventor, but to several of your readers who are artists and mechanical geometricians.

JOHN GOUGH.

IX.

On the Culture, Properties, and comparative Strength of Hemp, and other Vegetable Fibres, the Growth of the East Indies. By Dr. WILLIAM ROXBURGH, *of Calcutta* *.

DEAR SIR,

YOUR letter of the 16th of May, 1799, I received on my return to Bengal in October last; but that from the Society established in London, for the Encouragement of Arts, &c. of which you are a member, is not to be found.

Ill advised experiments for the culture of hemp in India.

I was rather surprised, on my return to Bengal, to find the directors had sent out a person (Mr. Sinclair) to establish the cultivation of hemp, a thing I had begun some time before. Even on the coast of Coromandel, ten or twelve years ago, I made a most successful trial, the result of which was laid before that government, to be sent to the Honourable Court of Directors; and again in Bengal, since my appointment to the station I now hold. Mr. Sinclair is dead, and the experiment is still carried on in a most expensive manner; whereas it could be continued where it was first begun, in the botanic garden, at no expence, and with more prospect of success. Should government continue to be the cultivator, the price will be enormous. Eighty pounds weight is all, I believe, that is yet forthcoming, and costs from 10,000 to 20,000 rupees. Such experiments throw a complete check in the way of all attempts to introduce new, or improve old, branches of agriculture and commerce.

Best method of promoting its cultivation.

A small premium should be offered to the natives, and honorary rewards to Europeans, after the example is set on a small and not expensive scale.

Botanic garden at Calcutta.

The botanic garden was at first made very large, four times more than was necessary for such a garden, the intention of which

* Communicated to the Society of Arts, 1804.

which was merely to make experiments, and to invite the natives to fee and profit by the examples in hufbandry carried on there.

A quantity of my bow-ftring flax was, I underftand, fent from the coaft about two years ago, for the directors. I could wifh to know what was done with it; for, to me, it feems to be the ftrongeft vegetable fibre we are acquainted with. I mean to fend fome by the January fhips from hence, through the medium of this government, and wifh it may fall into your hands, and that its qualities may be properly examined by the Society for the Encouragement of Arts. I am really forry that the letter, inviting me to become an honorary correfponding member of that Society, fhould have been loft. I beg you will affure the Society, that I am fenfible of the honour they have done me, and fhall be very happy to have it in my power to contribute my mite to promote the views of that laudable inftitution.

Bow ftring flax the ftrongeft of vegetable fibres.

When any new object, promifing to become ufeful in the arts or manufactures of our country is difcovered, and reported to your Society (for example, the bow-ftring flax,) the Society will probably addrefs the Court of Directors, and recommend the cultivation and importation into England of the commodity itfelf.

Another object, of more national importance, which I recommended to this government, before I went to the Cape, was the growth of one of the moft noble of the palms, the *arrow*, mentioned in Marfden's Hiftory of Sumatra, page 77, and faid to yield at an early age (from five to feven years,) fibres ready prepared by nature, flexible, ftrong, and moft durable, and the moft convenient for cables and cordage of all kinds, that can be defired. It alfo yields great abundance of palm wine, which can be converted into fugar or ardent fpirits; and when the tree is old, its pith is the bafis of the fago we fo much value. I have diftributed many hundred plants, and have ftill a great number in the garden *, befide many thoufand feeds in the ground. Drawings, and a defcription of the moft valuable tree, were fent to the directors, under the name of *faguerus rumphii*; but as the trees from which they were taken have advanced in fize and age, a new

Excellent qualities of the *arrow* palm; for its fibres; its wine; and its fago.

* Feb. 1801. About 100,000 plants have been reared in this botanic garden fince the date of this letter.

ſet of drawings, and a new deſcription, of the old one corrected, becomes neceſſary. They will be ſent to the Directors this ſeaſon. The object may be ſuch as your Society wiſh to attend to; and on that account I have ventured to trouble you with the above ſtatement.

I am, dear Sir,

Your moſt obedient Servant,

W. Roxburgh.

Calcutta, Dec. 24, 1799.
Robert Wiſſet, Eſq.

Dear Sir,

Experiments on hemp.

MY letter of the 24th of December, 1799, I am afraid, has not reached you. The experiments on hemp, therein mentioned, have not, I believe, thrown much additional light on the ſubject. My friend, Capt. Burrows, of the Earl Howe, has done more to make our own indigenious ſpecies (the *ſun* of the Bengaleſe) better known, than any other perſon I am acquainted with.

Comparative examination of vegetable fibres.

For theſe laſt twelve months my attention has been much taken up in collecting and comparing the various vegetable fibres of Aſia, &c. uſed for cordage, cloths, and paper. The reſult of theſe I have lately preſented to the ſupreme government, to be ſent to the Hon. the Court of Directors, in reply to the 79th paragraph of their general letter of the 7th of May laſt. This paper may be intereſting to your Society, particularly at this time, when the attention of all good patriots is drawn towards the diſcovery of a ſubſtitute for Ruſſian hemp. This paper, with my former eſſays, contain much information on the ſubjects therein mentioned.

Neceſſity of a ſubſtitute for Ruſſian hemp.

The *ſun* plant is moſt promiſing.

The *ſun* of the Hindoos, which is the prepared fibres of the bark of a well-known Indian plant, the *crotalaria juncea* of Linnæus, ſtill appears to me to be the moſt promiſing ſubſtitute for hemp which has come to our knowledge; I mean, when every circumſtance, relative to its quickneſs of growth, its being already univerſally known and cultivated by all the nations of India, its low price, pliableneſs, ſtrength, durability, &c. &c. are taken into conſideration. All that can be neceſſary for the procuring and tranſporting to England any quantity of this material, is to enſure the cultivator a certain

certain price, and ready market for the commodity; and to have it properly cleaned and packed, to render the freight as low and convenient as poſſible. Cleaned ſamples of this very ſubſtance were ſent by me, ſix or ſeven years ago, to the Directors; ſo that the fault is not mine, if it is not already better known than it ſeems to be.

The diſcovery of a ſubſtitute for Ruſſian hemp is certainly an object of the firſt magnitude. If *ſun* is found to be the beſt ſubſtitute yet diſcovered, and coſts in India, ſay, when properly cleaned, ten pounds per ton, and the freight ſixteen pounds, there will ſtill be a conſiderable profit to the merchant, particularly in times of war; for, I believe, it rarely happens that hemp ſells ſo low in London as thirty pounds per ton. Should the ſubject appear of conſequence to you, I beg you will call the attention of the members of your Society to it. It can be afforded at a reaſonable rate.

That I may not encroach too much on your time, I will cloſe this letter by referring you to my friend Mr. Boſwell, the late Marine Storekeeper and Naval Paymaſter, for any farther information you may want. I ſend this package by him. Mr. Babb, late a Member of the Board of Trade here, can alſo give you much information.

I am, dear Sir,
Your moſt obedient Servant,
W. Roxburgh.

Calcutta, Feb. 27, 1801.
Robert Wiſſet Eſq.

Obſervations on the Culture, Properties, and comparative Strength of Hemp, Sun, Jute, and other Vegetable Fibres, the Growth of India, communicated by Dr. Roxburgh.

HEMP, conſidered merely as an article of trade, is an object of the firſt importance to the merchant; but, when we reflect on its various uſes, and obſerve that hardly any art can be carried on without its aſſiſtance, or of ſome other ſubſtitute; the objects it embraces are immenſe, and there are few that better deſerve the attention of the philoſopher or intelligent artiſt. Cordage makes the very ſinews and muſcles of a ſhip, and every improvement which can be made in its preparation, either in reſpect to ſtrength, pliableneſs, or Importance of hemp and its very extenſive uſe in ſociety.

or durability, or in bringing to light ſubſtitutes equally good or better, particularly where hemp itſelf cannot well be had muſt be of immenſe ſervice, particularly to the mariner, and to the commerce and the defence of nations.

Its cultivation in our colonies intitled to ſupport and encouragement.

The cultivation of this important plant in our colonies has not only, at all times, met with encouragement from the government, but alſo of late from the Eaſt-India Company in Bengal, where extenſive experiments were begun by the late Mr. Sinclair, and after his death carried on by Thomas Douglas, on the culture of hemp and flax, on account of the Honourable Company. A clear and impartial ſtatement of theſe trials is to be wiſhed for, as it will, no doubt, throw much light on the cultivation of hemp in India, and enable us to proceed with greater proſpects of ſucceſs than ever.

It may be very beneficially produced in India.

My own experiments and inquiries on the ſame ſubject, both on the coaſt of Coromandel and Bengal, have been many, though not extenſive. Their reſult leads me to think, that hemp may be cultivated to great advantage over the interior parts of Bengal and Behar, where the ſeed ſhould be ſown about the beginning of the periodical rains, or earlier, if there have been frequent ſhowers, on elevated ſpots of rich loamy ſoil, ſuch as the Ryots cultivate tobacco, ſun, and paat on, near their habitations. In ſituations of this nature it thrives well, and will be eaſily attended to. At firſt, ſome encouragement will certainly be neceſſary, to induce the Ryots to undertake this new branch of agriculture. For, although the plant is perfectly familiar to every Hindoo, yet the cultivation on an extenſive ſcale, for the fibres of its bark, is perfectly unknown to them. I would therefore ſuggeſt, that they ſhould pay no rent for the ground ſo occupied for a certain period; that ſeed ſhould be given gratis; that they ſhould be enſured a certain price for the hemp; and finally, a reward or premium to the perſon or perſons who produced the greateſt quantity of the beſt hemp within a ſtated period.

The Hindoos cultivate it only for the ſeed.

Seaſon for growing it.

In many parts of Bengal, particularly where the land is ſo low as to remain humid through the dry-weather ſeaſon, hemp thrives luxuriantly during the cold ſeaſon; but the water is then too cold for macerating the plants to the greateſt advantage: one day in June, July, or Auguſt, has more effect in looſening the bark, than eight in December, January, or February; conſequently, the prolonged immerſion injures the quality

quality of the hemp much. The rainy ſeaſon is therefore preferable for the cultivation and maceration, even if the plant grew better during the cold, which is by no means the caſe, particularly on lands elevated above the level of the annual inundation of the low rice-fields. We muſt therefore content ourſelves with one crop in the year; for it is a very falſe notion, and a very prevailing one, that the fertile fields of Aſia produce at leaſt two crops annually; as well might we ſay, that the fertile lands of England yield at leaſt two, becauſe a well-managed garden, near London, or ſome other large city, will produce repeated crops in the year: ſo in India, by great care and induſtry, a ſpot here and there will produce two or more crops. The burning heats of Aſia, while they laſt, are as unfavourable for vegetation, as the froſts of winter in Europe.

One annual crop only can be had.

Beſides hemp and flax, the vegetable kingdom, particularly that natural diviſion called by Linnæus, Columniferæ, abounds in plants which produce materials fit for cloths, paper, and cordage: almoſt every nation or country poſſeſſes ſomething of the kind peculiar to itſelf. To aſcertain what theſe are, as well as to find out new ones, to try their comparative ſtrength, durability, texture, &c. has, at various leiſure hours, employed my attention for many years paſt. Drawings and deſcriptions of many of them have been already laid before the honourable Court of Directors. There are, however, ſome others which remain to be brought under view and compared with the kinds we are beſt acquainted with, which I have attempted in the following experiments. Many other ſorts are alſo mentioned by various authors and travellers, of which I know nothing more than the names. Two of theſe are mentioned by Marſden, at pages 75 and 76 of his hiſtory of Sumatra. Others are peculiar to Pegu, &c. &c. Theſe I muſt omit for the preſent, and confine myſelf to ſuch as I am more intimately acquainted with. The better to enable me to proceed in this inquiry, I have cultivated, in the Botanic Garden at Calcutta, many of the plants themſelves which produce the materials hereafter mentioned; prepared their fibres in general by maceration, &c. as with hemp and flax in Europe. And, to compare their ſtrength (plain, tanned, and tarred,) had then made into cords, compoſed of three ſimple yarns, as nearly of the ſame ſize and hardneſs as a

Other vegetables particularly the columniferæ, fit for the uſes of hemp.

Deſcriptive outline of the author's experiments on various plants for cordage, &c.

Preparation of the fibres.

 Hindoo

Cordage manufactured,

Hindoo rope-maker could make them; but, in ſpite of my utmoſt care, they were always too hard twiſted to be of the greateſt poſſible ſtrength *. Of each ſort there were ſix, when there was a ſufficient quantity of the fibres. Three of them were about the ſize of a log-line, and three a ſize larger than a whip-cord; one of each ſize and ſort was kept white: the average number of pounds which broke them, (for repeated trials were made, and always with lengths of exactly four feet), will be found in the firſt and ſecond columns on the right. One of each was tanned † with the aſtringent fruit called *gaub* by the Bengaleſe, (Embryopteres Glutinifera, Roxb. Coromandel Plants, Vol. I. No. 70.) Their ſtrength is repreſented in the third and fourth columns of the following table. And, laſtly, one of each ſort was tarred: their reſpective ſtrengths will be found in the fifth and ſixth columns.

and tried in the white, and alſo tanned and tarred.

Tanning ſtrengthens animal fibres, but quere vegetable?

We know the tanning principle ſtrengthens the fibres of leather, (animal fibres) but are not ſo clear that its operation on vegetable matter is uniformly the ſame. The attention beſtowed to aſcertain this point in theſe experiments will, at leaſt throw ſome light on the ſubject; and may induce others, better qualified, to extend the inquiry (here in India), where tanning materials abound.

* The experiments of Reaumur, Sir Charles Knowles, and Du Hamel, uniformly prove, that when hemp-rope is twiſted to the uſual hardneſs, which is that which brings them to two-thirds of the length of their reſpective yarns, their ſtrength is leſſened by nearly one-fourth, when compared with ropes conſiſting of the ſame number of the ſame yarns twiſted up to only three-fourths of their length. There will be no difficulty in accounting for this difference, if we conſider that a ſkein of fibres may be twiſted ſo very hard, as to break with any attempt to twiſt it harder. In this ſtate the fibres are already ſtrained to the utmoſt, and cannot ſupport any weight or additional ſtrain.

† The idea of tanning cordage is far from being new; for the fiſhermen of Aſia, as well as of Europe, not only tan their nets and lines, but alſo their ſails, to give them additional ſtrength and durability. The ſame proceſs might be productive of the ſame effects, if employed on cordage made of the materials (No. 2, 5, 6, 7, 8, and 15), ſpecified in theſe experiments, which induced me to recommend its being tried with ſun cordage, in my letter to the Board of Trade, in Auguſt, 1797.

Another

Another point of the utmoſt importance to be aſcertained is whether tanned ropes will be preſerved by the tan, with which they are impregnated, when ſtowed away wet; as tarred ropes are preſerved by the tar when ſo circumſtanced. It is nevertheleſs a well-known fact, that tarred cordage, when new, is weaker than white; and that the difference increaſes by keeping. Tar can therefore only be employed to preſerve cordage, and not to ſtrengthen it; ſo that if tan will add ſtrength, or even not weaken vegetable fibres, and at the ſame time preſerve them; of how great advantage to the nation would the diſcovery of a convenient practicable proceſs be. For all cordage, expoſed to be alternately very wet or dry, requires to be impregnated with a preſervative. And, to conclude this long note, I beg leave to obſerve, that tar is not the produce of the warmer parts of Aſia. Are we, therefore, to conclude, that no material, the produce of theſe parts, can be applied for the ſame end? Let us not entertain any ſuch idea. Nature is abundantly kind, and furniſhes every country and climate with what is moſt proper for the uſe of its inhabitants.

Whether tan will preſerve wet rope?

Tar does not ſtrengthen cordage; but the contrary.

Tar not produced in tropical Aſia.

The annexed ſtatement of the experiments made on the ſubſtances there ſpecified, can only be deemed an attempt towards aſcertaining their relative ſtrength; and though they are the average reſult of ſeveral trials made on the ſtrength of each cord, plain, tanned, and tarred, yet I muſt acknowledge they cannot be deemed any thing more than a firſt eſſay, chiefly owing to the lines being in general ill laid, ſome more and ſome leſs twiſted, and by no means to be compared with thoſe of Europe. For in ſome inſtances, I found a ſmall one ſuſtain a greater weight than a much larger, made exactly of the ſame materials. It is therefore my intention to repeat them on a larger ſcale, and, if poſſible, with better made lines; for every thing depends on their being exactly of the ſame ſize and degree of twiſt.

Statement of experiments on the cordage.

COMPARATIVE

COMPARATIVE STATEMENT OF THE EXPERIMENTS.

NAMES of the PLANTS, and brief REMARKS on the various MATERIALS employed in these EXPERIMENTS.	Average weight at which each sort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small.	Large.	Small.	Large.	Small.
No. 1. English Hemp: a piece of new tiller-rope, opened out and made into cords, like the reft, - - - - - - - - - - - - - -	105	65	. .	. .	. .	. .
2. Hemp (Cannabis), the growth of this feafon, from the Company's Hemp Farm, near Calcutta, - - - - - - - - - - - - - -	74	50	139	60	45	46
3. Coir: the fibres of the hufk of the Cocoa-nut; much ufed for cables and cordage over Afia, - - - - - - - - - - - - -	87	60	. .	. .	. .	.
4. Ejoo (Saguerus Rumphii): the black horfe-hair-like fibres, which grow round the trunk of this fpecies of Sago Palm, - - - - - - - -	96	79	. .	. .	. .	. .
5. Robinia Cannabina, Danfha of the Bengalefe: the fibres of its bark, prepared by maceration from the plants that had nearly ripened their feed, they are then of a dufky grey colour, and harfh nature, - - - - - -	88	64	101	55	84	39
6. The fibres of the bark of No. 5, prepared by maceration, from plants coming into bloffom; at which time they are beautifully white, foft, and gloffy, -	46	20	61	35	48	30
* 7. Crotolaria Juncea, Sun of the Bengalefe: the fibres of its bark, and univerfally known over India, - - - - - - - - - - - - -	68	47	69	55	60	37
8. Corchorus Olitorius, Bunghi-paat of the Bengalefe: the fibres of its bark, called Jute by the fame people, - - - - - - - - - - - -	68	47	69	59	61	36
9. Corchorus Capfularis, Ghee-nalta-paat of the Bengalefe: the fibres of its bark they call Nalta Jute, - - - - - - - - - - - - -	67	47	. .	. .	. .	. .
10. Flax (Linum Ufitatiffimum): the growth of the Company's Hemp Farm, near Calcutta - - - - - - - - - - - - - - -	39	37	. .	. .	. .	. .

* A cord, a very little thicker than a log-line, made of fun fail twine, broke with 148 pounds when dry; but, on being foaked in cold water for 24 hours, it bore, while wet, 222 pounds. This difference requires to be farther inquired into.

COMPARATIVE STATEMENT OF THE EXPERIMENTS—*continued.*

Names of the Plants, and brief Remarks on the various Materials employed in these Experiments.	Average weight at which each fort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small.	Large.	Small.	Large.	Small.
11. Agava Americana: the fibres of its leaves. They are of a coarfe harfh nature, and white, - - - - - - - - - - - -	110	71	79	78	78	38
12. Aletres Nervofus—In Sanfcrit, Murva; Murga of the Bengalefe: the fibres of its leaves made into thefe cords, after having been kept above one year, -	120	52	73	42	48	43
13. Theobroma Augufta, Linn.—Abroma Augufta, Hort. Kew.—Abroma Wheelerii, Kæn.—Woollet-comal of the Bengalefe: the fibres of its bark prepared by maceration, &c. like hemp, - - - - - - - - -	74	61	58	51	44	29
14. Theobrama Guazuma, Baftard Cedav: the fibres of the bark of fome ftraight, luxuriant, young plants, - - - - - - - - - - -	52	48	..	47	45	30
15. Hibifcus Tiliaceus, Bola of the Bengalefe: the fibres of its bark, and employed for cordage by the inhabitants of the South-Sea Iflands, &c. - - -	41	..	62	39	61	..
16. Hibifcus Manihot, a tall white-flowered variety: the fibres of its bark, which are beautifully white, gloffy, and foft, - - - - - - - -	61	..	..	..	..	..
17. Hibifcus Mutabilis: the fibres of its bark, and they are of a harfh nature and ill coloured, - - - - - - - - - - - - - -	..	45	53	46	..	..
18. Hibifcus, a new fpecies, from the Cape of Good Hope, faid to be a native of Caffraria, where the fibres of its bark are fpun, - - - - - -	..	22	..	..	..	..
19. Bauhinia, a large fcandent fpecies: the fibres of its bark, cleaned without maceration, and ufed to make ropes, &c. of by the people of Napaul, where the plant is common, - - - - - - - - - - - -	69	39	..	..	..	..
20. The fame as No. 19, only maceration was ufed to help to take the bark off the twigs with more care, - - - - - - - - - - - -	56	41	..	..	..	..
21. Sterculia Villofa: the fibres of its bark. Cords are made of them by the natives of the eaftern frontier of Bengal, to bind wild Elephants, when firft taken,	53	..	..	..	..	..

The

The cords, when the trials were made, had been kept about six months after they were prepared, chiefly with the view of allowing the effects of the tan and tar to take place. The result of these experiments show, that tan has in general added strength, while tar has had a contrary effect; and in no instance is this more clearly evinced, that in the common hemp (Cannabis) cultivated in Bengal.

Ejoo and Coir.

To Ejoo and Coir, neither tan nor tar seem applicable; and in several of the other experiments, I had not a sufficient quantity of the materials to make the necessary number of cords, viz. six of each sort, to try with tan and tar, as well as in their natural state. At some future period, I hope to be more fortunate in procuring larger supplies of the materials, and also to add some other other sorts, such as the—

Bow-string fibre.

Rajemahl bow-string fibre, the produce of a new species of Asclepias, discovered by William Roxburgh, junior, amongst the Rajemahl Hills;

New Zealand hemp.

New Zealand Hemp;

Hisbiscus Cannabinus, and some other of the same natural order (Columniferæ); (for in general their barks abound with strong fibres; witness the foregoing table, where six of them are to be found);

The leaves of a new species of Andropogon, &c. &c.

APPENDIX,

Containing Remarks on some of the Plants mentioned in the foregoing Table.

Remarks on hemp.

No. 2. Hemp, or Cannabis Sativa.—Banga, in Sanscrit; Bunga, Bungh, or Bung, of the Hindoos; Bang, of the Persians; Kinnub, of the Arabians, is no doubt our own famous plant, now so common and useful in Europe. I have at different times examined various figures and descriptions, as well as the plants reared from Europe seed, comparing them with our Indian plant through its various stages, and can discover no difference whatever, not even to found a variety on. Perhaps few vegetables, so widely diffused over almost every part of the known world, and under the immediate manage-

It is well known in India; but not for its fibres;

ment of man, have undergone less change. It is perfectly familiar to all the nations of India, I may say of all the warmer parts

parts of Aſia; yet I cannot diſcover that the fibres of the bark have ever been employed for any purpoſe. It is cultivated in ſmall quantities every where, on account of its narcotic qualities.—The leaves of the male plant, and flowers of the female, are the parts in moſt general uſe.

I have repeatedly applied for the ſeeds of all plants reared in China, and other countries to the Eaſtward of the Bay of Bengal, as well as to almoſt every other part of India we have any communication with, for an account of the plants employed to ſupply materials for clothes and cordage, and for their ſeeds; but could never learn that Cannabis was one of them; nor were its ſeeds ever ſent to me as ſuch. nor in China, &c.

No. 4. The great ſtrength of this ſubſtance makes it a very deſirable object. For a deſcription and drawing of the tree I refer to thoſe * which accompanied my letter to the Moſt Noble the Governor-General in Council, under date the 2d of January, 1800. The fibres employed in theſe experiments were taken from trees growing in the Botanic Garden at Calcutta, where they thrive well. I could obſerve, during the trials made in breaking the cords of this ſubſtance, that they were not ſo elaſtic as thoſe of Coir, which will probably render it leſs fit for cables, but better for many other uſes. Coir is certainly the very beſt material yet known for cables, on account of its great *elaſticity* and ſtrength. Ejoo; very ſtrong, but not elaſtic.

Nos. 5, 6, 7, 8, and 9. Theſe four plants have already been figured and deſcribed by me, in a memoir ſent through the Governor-General in Council, to the Hon. the Court of Directors, in December, 1795. Since writing that paper, I have learned, that *ſun* (Crotolaria Juncea) is almoſt univerſally employed, over the warmer parts of Aſia, for cordage. On the Malabar coaſt, I find it is generally named by the gentlemen at Bombay after the province where reared. It is uſed in that place for lacing their cotton bales, on account of its great ſtrength. Samples of three ſorts, viz. Malwan, Rajapore, and Salſette, were ſent to me, from them, by Dr. William Hunter; and am induced to think, little or no maceration is Other plants: The *ſun* or crotolaria juncea.

* A former ſet, No. 1179, ſent in to Government on the 23d of November, 1797, were not ſo correct as could be wiſhed, on account of their having been taken from young trees, juſt coming into bloſſom the firſt time.

employed

employed in taking the bark from the ſtalks, or in cleaning the fibres, which may add to its ſtrength: for certainly maceration, particularly if long continued, muſt weaken freſh vegetable fibres conſiderably. The ſame gentleman ſent me ſeeds of the Salſette ſort: they have produced plants now in bloſſom, and from them have aſcertained the identity of this ſpecies.

In ſome parts of Bengal, a moſt luxuriant variety is cultivated immediately after the rains, which often grows to the height of twelve or fourteen feet; while the common ſort is generally reared in Bengal during the early part of the wet ſeaſon, and grows to only about half the height of the former.

Damſha of the Bengaleſe;

I muſt further obſerve, that the fibres of No. 5, poſſeſs great ſtrength, and it ſeems to me to be one of the moſt fit of any of our Indian productions for cables and cordage. The plant grows generally to the height of from ſix to ten feet, the fibres long, but harſher than thoſe of hemp, if not cut at an early period. It is very generally cultivated about Calcutta during the rains. An acre yields of the half-cleaned ſubſtance (the ſtate in which the natives carry it to market), about 600 lbs. weight, and ſells for about a rupee and a quarter per maund of 80 lbs.

beautiful but weak fibres.

No. 6. By cutting No. 5, the laſt-mentioned plant, when beginning to bloſſom, we have the moſt beautiful ſhining white fibres that can well be conceived, but (by my experiments) greatly weaker that when the ſeed is ſuffered to be nearly ripe before the plants are cut.

Flax cultivated in India for its ſeeds only.

No. 10. Flax, the plant, is very generally cultivated during the cold ſeaſon, over the interior parts of Bengal and Behar, merely for the ſeeds, from which oil is obtained. The flax itſelf the Hindoos ſet no value on; for, after they have gathered the ſeed, they throw away the ſtalks as uſeleſs, having no knowledge of the fibres which their bark yields. Samples of the flax have repeatedly been procured by the Board of Trade, and ſent to England to the Hon. Court of Directors; ſo that it is from England we may expect to learn its properties. If the flax has been found good, large quantities may be reared at a ſmall expenſe; as the ſeed alone, which the crop yields, muſt be more than equal to the charges, to render it profitable to the farmer.

No.

No. 11. This Agava is of flow growth; on that account I doubt if ever it can be advantageoufly cultivated; but, where found wild in plenty, it may be manufactured at a trifling expenfe. Its great ftrength renders it an object worthy of attention. The fibres are coarfe, confequently rope made of them harfh to the feel. Agava Americana; of no great promife.

No. 12. Drawings, and a defcription of the plant Aletris Nervofus Roxb. the method of extracting the fibres, with a quantity of the fubftance itfelf, were fent to the Hon. the Court of Directors, through the Madras Government, above ten years paft. I alfo gave a large quantity to Mr. Bebb, when he left Bengal in January, 1800, to take to England with him for trial there. The plant grows fully as well, and is as common as on the Coromandel coaft. There has lately been about a biggah (third of an acre) planted out with it in the Botanic garden, the better to determine the expenfe, and the annual produce of any given quantity of ground. Aletris nervofus.

No. 13. This plant, a native of various parts of India, New South Wales, Philippine Iflands, &c. has been long known to botanifts; yet I cannot, with all the attention that I have been able to beftow on the fubject, find that the fibres, fo abundantly interwoven through its bark, have ever been ufed or even taken notice of by any other perfon; fo that I think we may look upon it as a new difcovery, deferving of more than common attention, on account of the beauty, finenefs, and ftrength of thefe fibres. Abroma augufta, very promifing.

It is perennial, grows luxuriantly in the Botanic garden, and has been cut down twice within thefe fix or feven months; fo that I think it will, at leaft, annually afford two or three crops of fhoots fit for yielding this fubftance. My experience does not yet enable me to ftate how much may be the yearly produce of an acre, but can venture to prognofticate as large a produce as can be obtained from ~~an acre of Dancha,~~ Jute, Sun, Hemp or Flax. Its growth.

To render this bark feparable from the half-ligneous fhoots it covers, to foften its external lamina, or epidermes, and the parenchymatous fubftance which firmly connects the fibres in their natural ftate, maceration in ftagnant water for from four to eight days, during the warmer parts of the year, anfwers well whilft three times as many days are fcarce fufficient during the cold feafon; indeed, the procefs is fcarcely practicable then; befides, the fibres are greatly weakened by the length of the maceration. Treatment to obtain the fibre.

Immediately

Immediately on being taken out of the water, and while wet, the ſhoots are ſingly taken in the hand, rubbed with ſome coarſe materials, ſuch as a little dry graſs or gunny, to remove the exterior pulpy lamina or epidermis of the bark, which is deſtitute of fibres. This part of the proceſs is eaſily effected; and when done, the clean ſhoots are to be made up into ſmall bundles, and placed under weights, or ſome other equal preſſure, to keep them firm at the middle and top, either under the ſurface of the waters, or out of it; the fibrous bark is then ſeparated with the fingers from a ſmall portion at the end of the ligneous ſhoot or ſtalk, which the operator takes hold of, and draws out one by one; when theſe are removed, the pulp, or parenchyma, which fills the interſtices between the fibres, and connects them together, forming in the living plant that part of the bark which may be called its inner lamina, or cellular tiſſue, is immediately waſhed out in cold water, and the clean fibres ſpread out in the ſun to dry. Such was the ſimple proceſs by which this ſubſtance (which may very properly be called Indian Hemp or Flax) was prepared.

I have now under cultivation about the third of an acre (a biggah) of ground in the Botanic garden with this plant; the reſult ſhall be carefully noted from time to time. It might have been prudent to have withheld this account until that time; but the ſtrong deſire of making known a diſcovery, which may in a ſhort time become beneficial to the public, induces me to be thus precipitate.

For a farther account of this plant, I beg leave to refer to my drawing and deſcription thereof, ſent to the Hon. the Court of Directors ſome years ago, and numbered 415.

W. ROXBURGH.

Botanic Garden, near Calcutta,
Jan. 31, 1801.

Additional Experiments on the Strength of Sun, (No. VII.)

Additional experiments on the ſtrength of *ſun.*

Some tanned ſail-twine, made of this ſubſtance four years ago, for the inſpection of the Marine Board, was made into a cord of three ſtrands; each of the ſtrands compoſed of four threads of the ſail-twine.

Some more of the ſame ſail-twine, tanned twelve months ago, was made into a ſimilar cord, and another was made of the white unprepared twine.

The

The firſt, which had been tanned four years, broke with 110 lbs. when dry, and with 130 lbs. after having been ſteeped in water 24 hours.

The ſecond, which had been tanned one year, broke with 123 lbs. when dry, and with 140 lbs. after ſteeping 24 hours.

The third, or plain white, broke with 148 lbs. when dry, and with 222 lbs. after ſteeping 24 hours.

X.

Deſcription of the Bavarian Method of evaporating Saline Waters. By M. BONNARD.

THIS new method, practiſed in Bavaria, has been introduced into the ſalt work of Moyenvie, by M. Cleiſs, inſpector of the ſalt-works of Bavaria. Bavarian method of evaporating ſalt waters.

The pans are compoſed of ſquare plates of caſt iron, of 4 millimetres in thickneſs, and 4.76 centimetres on each ſide. Theſe plates are joined by their edges, which are turned downwards, and conſequently without the pan: they are ſolidly united by a piece in the form a ſquare gutter which receives the edges, and is ſecured by a great number of ſcrews.

An evaporating houſe is compoſed of ſix pans, of this conſtruction, diſpoſed in two rows; but theſe pans have different uſes, which require a particular arrangement.

That in the middle of the back row is the ſmalleſt; it has no particular fire-place, but it is heated by the junction of the chimnies from the other fire-places. The ſalt-water depoſits its impurities in it; it is called the ſmall pan.

From the ſmall pan the ſalt water paſſes into the graduating pan, which is lower than the firſt, and placed in the middle of the front row; it is there kept in a ſtate of conſtant ebullition: the water is concentrated in it to 20 degrees, and depoſits a part of its ſulphated lime.

From the graduating pan the ſalt water paſſes into the preparing pans, which are lower than it, and ſituated at the two extremities of the back row: there it is alſo kept conſtantly boiling, Bavarian method of evaporating falt waters.

boiling, it is completely concentrated, and depofits all its fulphate of lime; it is then paffed into the cryftallizing pans, ftill lower than thofe of preparation, and placed at the two extremities of the front row: there the water fcarcely boils, and the falt cryftallizes.

Each pan, with the exception of the fmall pan, has a particular fire-place, the chimnies of which pafs round the fides of the pan: they unite under the fmall pan, by which means there is little heat loft.

Thefe pans are placed two and two in chambers of wood, the joinings of which are well fecured, which clofe them hermetically: thefe chambers are low, and their ceilings are perforated in the middle with holes terminating in a tube, by means of which the aqueous vapour is difengaged with rapidity. The chambers for the preparing and cryftallizing pans have their ceiling pyramidal, or in the form of a reverfe hopper, while that for the fmall pan and the graduating pan is horizontal.

The faline waters are paffed fucceffively into thefe four kinds of pans; the workmen penetrate into the chambers, in the midft of the vapour, to open the communications. This operation is performed every fix hours, and the water in each pan is reftored to the level at which it ftood fix hours before. Every three hours the falt in the cryftallizing pans is collected, it is brought with fcoops to elevations on the front edge of the cryftallizing pans, where it drains; it is afterwards carried into drying rooms, which furround the outfide of the chambers: thefe are fpaces covered with iron plates; they are warmed by heat-tubes leading from the fire-places.

Every eight days they take away the fulphate of lime, throw out the mother-waters, and break the fhell, that is to fay, the incruftations of falt which adhere to the bottoms of the pans: every twenty-four days the work is entirely ftopped to repair the pans; an operation which is performed by the workmen themfelves.

Economy of fuel.

It has been found that this method of evaporation affords a faving of more than one-third of the fuel.

An improvement has lately been made in this procefs at Dieufe: the fmall pan has been fuppreffed, and the drying rooms have been replaced by auxiliary pans, in which a coarfe falt is made.

The

The heated drying rooms are ufelefs when the humidity of the falt arifes from the muriate of lime it contains. Bavarian method of evaporating falt waters.

Explanation of the Plates III. and IV.

Fig. 1. Plan of the pans.

No. 1. Small pan.

No. 2. Graduating pan.

No. 3. Preparing pan.

No. 4. Cryftallizing pan.

The difpofition of the plates of iron which compofe thefe pans, is fhewn in No. 2.

a, a. Elevation on which the falt is placed to drain, as it is taken from the cryftallizing pans.

b, b, b, Wooden partitions which feparate the chambers.

c, c, c, A raifed wooden ledge which furrounds the pans.

Fig. 2. Section of the evaporating chamber which contains the pans 1 and 2.

d, d, d. Heat tubes which give heat to the fmall pan, and contribute to heat the others.

e, e, e, Fire place for the pans.

i, i, i. Pillars of caft iron under the gratings *g, g, g,* which fupport the bottom of the pans.

h. Wooden chamber which contains the two pans.

k. Opening by which the vapours efcape.

Fig. 3. Section of the evaporating chamber which contains the pans 3 and 4.

a. Elevation on which the falt from the cryftallizing pans is placed to drain.

The other letters indicate the fame parts as in the preceding figures.

Fig. 4. Method in which the plates of iron are joined to form the pans.

a. The iron plate.

b. The iron gutter which receives the edges of the plates, and is ftrongly faftened with fcrews.

i, i. Pillars of caft iron which fupport the bottom of the pan.

XI.

Letter of Inquiry respecting the late Dr. IRVINE'S *fundamental Experiment on the relative Capacities for Heat of Ice and Water. With an Answer by Mr.* IRVINE.

Request that Mr. Irvine would defcribe his father's experiment on the capacities of ice and water.

A CORRESPONDENT would be much indebted to Mr. Nicholfon if he would convey by publication in his Journal, or otherwife, the following requeft to Mr. Irvine. That gentleman in his paper in the Journal of laft month * mentions his having in his poffeffion the experiments of Dr. Irvine on the capacities of bodies. It has always been a wifh with thefe chemifts who have attended particularly to this fubject to know in what manner Dr. Irvine made the experiment to afcertain the comparative capacities of ice and water. If either of the bodies during the experiment change its form, if the ice were melted, or the water congealed, it becomes lefs decifive, as it may be objected by thofe who maintain the opinion that latent caloric exifts in bodies either in whole or in part in a ftate of chemical combination, that the refult might arife from fuch a combination, and not from a change of capacity. But if the experiment were made in fuch a manner that no change of form took place, which though difficult is poffible, Dr. Irvine's theory, which is fo much fuperior to the other, is unequivocally eftablifhed. It would be conferring a favour on the chemical world, if Mr. Irvine would take the trouble of faying in what manner the experiment was conducted, providing fuch a notice would not interfere with the intention he has announced of giving a more full account of his father's inveftigations, an account which would be eagerly received by chemifts.

Change of form would influence the refult.

September 24th, 1803.

Reply by Mr. IRVINE.

The experiments may be made without being liable to the objections before ftated;

WITH regard to the enquiry of Mr. Nicholfon's correfpondent, it would give me pleafure to inform him of any circumftance within the fphere of my own knowledge that fhould tend to add to the illuftration or proof of my father's theory.

* Sept. 1803. See our Addrefs to Correfpondents of laft month.

There

There does not ſeem to me any difficulty in explaining ſatisfactorily how experiments on the capacities of ice and water may be conducted without being expoſed to the objections above ſtated; at the ſame time, though theſe experiments may be concluſive as far as they go, I have not been accuſtomed to conſider them as altogether ſo deciſive of all arguments upon this ſubject. —but may not be unexceptionable.

In a general way then the experiments of Dr. Irvine were conducted in the following manner. The capacity of water being taken at unity, pains were uſed to aſcertain the capacities of mercury, river ſand, pounded glaſs, and iron filings, with reſpect to water, and conſequently to each other in the ordinary manner. It is at preſent of no importance what proportional quantities of the materials were employed. This being done, the capacity of one of theſe ſubſtances was experimentally compared with that of pounded ice or ſnow.— Every precaution was uſed to enſure ſucceſs. The weight and capacity of the veſſel was determined, and the colder and hotter body alternately added to the other. The temperature of the air was always below 32, as was that of all the materials and of the veſſel. No water could therefore be formed. In his firſt experiments I believe Dr. Irvine uſed mercury, but afterwards I know that he preferred iron filings and ſand. For example, if the veſſel and room were at 11°, let half an ounce of powdered ice from diſtilled water, at temperature 30° be poured on four ounces of iron filings at 11°, let the temperature of the mixture be noted after ſtirring, the due allowances made for the heat gained by the veſſel and the air, and a proper calculation made. Let this experiment be reverſed by cooling the ice to 11°, and pouring the iron filings at 30° upon it, and let a calculation from this be compared with the former and corrected by it. Finally, let theſe experiments be compared with others where different quantities of materials are uſed, and of different temperatures, and you have a view of the method employed for determining this point by Dr. Irvine, which does not appear to be objectionable upon other grounds than all experiments for aſcertaining capacities are, none of which have any pretenſions to perfect accuracy. Dr. Irvine was far from being ſatisfied that his experiments were mathematically preciſe. But he uniformly found the capacity of ice to be leſs than that of water, and that in a greater ratio than is generally allowed.

Account of Dr. Irvine's method of experimenting.

The capacities of mercury, ſand, pounded glaſs and iron filings were taken with reſpect to water.

One of theſe was then examined againſt pounded ice at a temperature of the place always under 32°.

Example.

The common notion, of ice and water being directly applied to each other, unfounded and impracticable.

With respect to the common notion that experiments on the capacity of ice are made by mixing it with water, it is altogether false in the case of Dr. Irvine. No doubt this may be done theoretically. It is easy to say that ice loses so many degrees which heat water only so many. But then no ice must be melted, which if not impossible is extremely difficult, or what is equally so, the quantity melted must be found, and an allowance made for the latent heat, which is itself not precisely ascertained, at least not with sufficient accuracy for this purpose. I have only to add that this gentleman's enquiries would have been earlier noticed if I had been informed of them, which I was only a few days ago.

W. IRVINE.

April, 1805.

XII.

Short Account of some of the most remarkable Facts and Observations in an Aerostatic Voyage, made from Petersburgh, by Messrs. ROBERTSON *and* SACHAROFF, *under the Sanction of the Imperial Academy.* W. N.

Aerostatic voyage from Petersburgh.

THE notice of an aerostatic voyage performed by Messrs. Robertson and Sacharoff from Petersburgh, under the direction of the Imperial Academy of Sciences, June 30, 1804, of which an abridged account was given to the National Institute of France*, is very interesting, from the scientific views and conduct of the managers Lowitz and Robertson, as well as for its other particulars.

Ascent of the balloon with two observers.

Their balloon was a sphere of 30 feet diameter, and rose at a quarter past seven in the evening, with the ascensional force of one pound, the whole weight of solid matter (including 110 pounds of sand for ballast) being 622 pounds. When they were over the river Neva, at the elevation of 108 toises or 620 feet, they descended a little by the condensation of the gas; but rose again by throwing out a little of the ballast. The usual phenomenon of a slow rotation of the balloon presented itself, which doubtless arose from the unequal action of the air against an irregular surface, as we see in most other bodies rising or falling in a fluid.

* Inserted in the Annales de Chimie, LII. 121.

But

But one of the moſt ſtriking circumſtances attending this voyage is the rational means which theſe philoſopers made uſe of to determine, and in a certain degree to regulate their courſe. They made uſe of two inſtruments, a log and a teleſcope. The log conſiſted of two ſheets of very thin paper, blacked, and fixed at right angles to each other by a very light croſs of wood. This was ſuſpended from their car by a ſtring of ſixty feet in length, and affording a different reſiſtance to the air from that of the balloon itſelf,† it was found to draw the ſtring out of the perpendicular direction, or as the narrators ſay, *to follow the balloon:* ſo that by its poſition determined by compaſs they could aſcertain what direction they were purſuing. It alſo ſhewed by its relative riſe and fall whether the apparatus was deſcending or aſcending, before their barometer had indicated the ſlighteſt change.

Their direction obſerved by a floating log; —of paper, &c.

Their teleſcope was applied to ſhew the direction of their courſe, and muſt have been much leſs ſubject to doubt than their log. Its application would be univerſal and perfect, if the earth could be ſeen at all times from the elevated regions of the air. It was directed perpendicularly downwards by means of a plumb-line, and having a conſiderable magnifying power, the objects upon the ſurface of the earth were ſeen moving acroſs its field of view, and their direction would moſt clearly aſcertain that of the car itſelf, and alſo its velocity. If, for example, the magnifying power were fifty times and the field of view one degree, the viſible ſpace included in that field from an elevation of two miles would be about 180 feet in diameter, in which objects of ſix or ſeven inches broad might be very well diſtinguiſhed through a favourable atmoſphere; and at ſo low a velocity as one mile an hour the whole field of view would be paſſed over in about twelve ſeconds. Hence we ſee that the method affords a conſiderable degree of accuracy, and will not in general require any great power of magnifying or delicacy of obſervation. The computation would be founded on the following problem, which will not preſent any difficulty to thoſe who are acquainted with theſe ſubjects, if the phyſical allowances for temperature in barometrical admeaſurements be admitted to be correct enough for this purpoſe.

Perpendicular teleſcope by which their courſe and velocity were ſeen on the ground.

Inveſtigation of the degree of accuracy of this method.

† Or rather, perhaps, becauſe not exactly in the ſame current of the atmoſphere. N.

Given

Problem for computing the velocity of a balloon by obſ. through a teleſcope.

Given the temperature on the earth and in the car, the height of the barometrical column, and the time employed in the apparent tranſit of an object on the earth through a given angle or field of view; to find the velocity of the obſerver.

Tabulated reſults for practice.

For practice it would perhaps be ſufficiently exact and convenient to compute a ſmall table, in which, neglecting the temperature, the velocity in miles per hour might be had by inſpection, when the height of the mercury, the time of tranſit, and the magnifying power were known.

The obſerved courſe, &c.

The aeronauts having noticed by their inſtruments what were the direction of the currents of air at different heights, found themſelves in one which carried them directly towards the Baltic. They therefore deſcended till they ſaw by the barometer that they had returned to a current which carried them inland; and afterwards again roſe much higher, and ſaw with great preciſion by their teleſcope the inſtant of their quitting the gulph. When the barometer ſtood at 24 inches they let go a pigeon, who flew with difficulty and would not quit the balloon; but upon being precipitated he in vain endeavoured to regain it, and at length deſcended rapidly towards the earth.

A pigeon let go.

The dip of the magnetic needle is leſs at great heights in the air.

At ten at night the balloon had riſen to an height indicated by 22 inches of the mercurial column, the thermometer ſtanding at $4\frac{1}{2}$ degrees (I ſuppoſe centigrade). Here it was that M. Sachatoff carefully obſerved a phenomenon which had been before remarked by M. Robertſon in his aſcent from Hamburgh, but at a much greater elevation. Their dipping needle was deranged; but on inſpecting the common compaſs, its needle was found to be no longer horizontal, the north end being elevated near 10 degrees. On this phenomenon they remark that the magnetic attraction probably diminiſhing as the ſquare of the diſtance may afford additional means of directing future obſervers in the atmoſphere, and even determine the elevations independently of the barometer. From the preſent elevation a pigeon being thrown down, fell ſo directly that it was doubted whether he could have reached the earth alive.

This may afford means of ſhewing the heights, &c.

Singular echo of the voice heard from the earth at the diſtance of two miles.

Darkneſs coming on, it became neceſſary to deſcend, during which the obſerver repeatedly made an experiment which alſo promiſes to be of great utility to voyagers in the air, as well as to enlighten our concluſions reſpecting the phenomena of ſound. When they ſpoke through a trumpet directed towards the earth, the voice was returned with extreme preciſion and

without

without feeming to have loft any part of its intenfity. No repetition was made except when the trumpet was directed to the earth; and the intervals of reflection were different according to the elevation of the obfervers. The percuffion impreffed on the air by the found every time produced a flight undulation in the aeroftat; whence they deduce an inference in favour of the fuppofed efficacy of cannon in partly modifying or averting the difcharge of ftormy clouds. In one of their experiments, the found employed ten feconds in its return, which would give a diftance of about two miles out and home, if the fame law of the velocity of found were fuppofed to prevail in the perpendicular courfe as along the furface of the earth, which however does not feem likely. The barometer ftood then at 27 inches, and at their outfet it was at 30 inches on the ground. It would be eafy, and it is furely defirable to make experiments with cannon and ftop watches on the velocity of afcending, and if poffible, defcending found.

No echo but when the trumpet was directed to the earth.

This reflection of found or echo is a fubject of very great curiofity. There is perhaps no other inftance in nature where fo extended a wall of reflection can be had. I am difpofed to think that the apparent intenfity of the returned found may in fome meafure have depended on the perfect filence in which the fpeakers were placed. In a ftill night the centinels on the ramparts at Portfmouth may be heard at the Ifle of Wight, over a diftance of five miles, and there are numerous inftances of low founds, fuch as the beating of a clock or watch, or the founds of footfteps being heard to confiderable diftances, when other founds do not act on the organ of fenfe.*

The ftrength of the found perhaps depended on the ftillnefs of the fituation.

In their defcent to the earth they paffed through various ftrata of vapours, all of different temperatures, and at the inftant the earth came in fight, the thermometer ftarted up through feveral degrees, probably becaufe they had quitted a cold mafs of vapour which obfcured their view, or perhaps becaufe the radiant heat of the earth's furface might at that moment have reached them unimpaired.

Sudden rife of the thermometer in defcending.

* See a curious paper of M. Perrole on found, with the annotations thereon, in the firft Vol. of our quarto Journal, page 411.—N.

Letter

XIII.

Letter from Mr. CUTHBERTSON, *containing Remarks on Mr.* WM. WILSON's and *Mr.* HAUY's *Experiments on the Electricity of Metals.*

To Mr. NICHOLSON.

London, 54, *Poland-Street*,
DEAR SIR, *April* 24, 1805.

Mr. Wilfon's experiments on electricity; IN your valuable Journal for January, page 42, you have favoured us with a letter from Mr. Wilfon, containing fome experiments which he calls, *exhibiting the electricity of metals.*

of copper fifted through zinc, Mr. Wilfon fays in his table of experiments, that copper filings fifted through zinc is pofitively electrified: this is an *error* which I thought myfelf obliged to take notice of, as it is a direct contradiction to what I have afferted in *my examination of Sig. Volta's experiments, which he calls fundamental, and on which his theory of galvanifm refts; fee this Journal, vol.* II. *page* 281; I have faid therein, that if a plate of zinc be feparated from a copper one, it will be pofitive, confequently leave the copper negative if infulated: whether this experiment be performed as I have therein mentioned, or according to Mr. Wilfon, the one reduced into filings, and afterwards fifted through holes made in the other metal, the electricity excited muft fall under the fame denomination, the difference will be that the quantity of electricity excited will be more by Mr. W.'s method than mine, becaufe he has multiplied the feparations, a fact well worth notice.

controverted by another experiment of contact and feparation.

Contact and feparation produce the electric ftate; Mr. W.'s chief object in view feemingly, is to prove that the feparation of metals is the caufe of the electric fluid being excited and not touching. I cannot perceive that thefe experiments throw any light upon that fubject, as both touching and feparating are here employed.

but neither act will do alone. In my fecond volume on electricity, publifhed in Amfterdam, anno 1782, I have proved by experiment that neither *touching* nor *friction* feparately employed excites electric fluid, friction and feparating jointly employed, is a powerful exciter of electricity on glafs; touching and feparating jointly employed on glafs excite electric fluid in a flight degree, and only when the ftate of the atmofphere is favourable.

In

In the Philofophical Magazine for Nov. 1804, page 120, we have Mr. Hauy's obfervations on the electricity of metallic fubftances: he does not inform us what fhape his *filver* pieces were, or whether they were pure or with alloy; he however does not hefitate in pronouncing of it to be pofitively electrified by friction; fo that it does not feem that he has entered very wide into the fubject, or he would have perceived fome remarkable changes to take place in that metal by friction, and particularly in coins. They will change from pofitive to negative, and *vice verfa*, without any vifible caufe. If a dollar and a half-crown be ftuck to the ends of two fticks of fealing-wax, and rubbed feparately upon woollen cloth, they will be found, after the friction, fometimes pofitive and fometimes negative, and fometimes one pofitive and the other negative, without varying the manner of friction. If pure filver, or filver with different proportions of alloy, be melted down to a button, and ufed in that fhape, or hammered flat, reprefenting coins, they are for the moft part pofitive by friction. Thefe experiments upon metals are not new; I believe they were firft begun by Mr. Henly, and inferted in the Philofophical Tranfactions; but I have not the data at hand, and I do not not remember that he had obferved the above-mentioned property of this metal.

Hauy's obf. on metallic fubftances, imperfect.

Singular effect of rubbing coins, &c.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Note on the New Planet Juno.

I HAVE not had any late account of the new planet announced at page 301 of our 9th volume (Dec. 1804) in a letter from M. Bode. The difcoverer's name, who is Mr. Harding of Lilienthal, near Bremen, was not then mentioned.—For the prefent I give the following notes from the Journal de Phyfique, Thermidor laft.

Planet Juno.

On the 5th of September, 1804, its right afcenfion was 1° 52′; declination 0° 11′ north. M. Burckhardt obferved it on the 23d of September at 359° 7′ and 4° 6′, whence he concludes that the duration of its revolution is five years and a half. Its inclination 21°; excentricity one quarter of its radius; mean diftance from the fun three times greater than that of the earth.

Its

Its diameter could not be meafured, but it appeared like a ftar of the eighth magnitude. It feems nearly equal to that of Ceres, or the planet difcovered by Piazzi.

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*Difcovery of Fluoric Acid in the Topaz.**

Saxon topaz analyfed by Vauquelin. Brafilian by Defcotils.

IN the year 1797, M. Vauquelin analyfed the Saxon topaz, and found its conftituent parts to be, filex 31, alumine 68. Mr. Defcotils foon after examined the Brafilian topaz; but as there was a lofs of 18 *per cent.* in his firft analyfis, and 12 in his fecond, he did not think fit to publifh the refults of his labours at that time; and other circumftances prevented him from purfuing the inquiry, as he had intended.

Klaproth difcovered fluoric acid in the Saxon topaz.

Sulphuric acid incapable of expelling it.

Not long ago Mr. Klaproth wrote to Mr. Haüy, that he had found fluoric acid in the Saxon topaz. Mr. Laugier made feveral experiments to verify this difcovery, but without fuccefs. It is true in the analyfis with potafh he found a deficiency of 16 per cent. but though he reduced the topaz to an impalpable powder, and did all he could to expel the fluoric acid from it by means of the fulphuric, he was unfuccefsful. Mr. Vauquelin on his return applied himfelf to the fame refearch, and we here prefent the refult of his labours. Not knowing what procefs Mr. Klaproth had employed, he tried that which feemed to him moft likely to fucceed. He firft heated the topaz with cauftic potafh in a filver crucible in the ufual way. After he had diluted the mafs with water, he introduced it into a retort, and poured on it fulphuric acid. White fumes foon arofe, and thefe, being collected, exhibited all the characteriftics of fluoric acid combined with filex. The latter came almoft wholly from the ftone, as the retort was not perceptibly attacked by the acid.

Vauquelin treating the topaz with potafh and fulphuric acid expelled the fluoric.

The Brafilian topaz gives the fame refult.

The fame experiment with the Brafilian topaz gave the fame refult; and there is every reafon to believe, that the Siberian, which Mr. Vauquelin is now analyfing, will afford the fame products. Thus we may now confider this gem as a filiceous compound, confifting of fluoric acid, alumine, and filex, or a true alumino-filiceous fluate; and the difcovery muft be confidered as of the higheft importance in mineralogy.

Topaz a true alumino filiceous fluate.

* Bulletin des Sciences, No. XC. p. 282, Sept. 1804.

Mr.

Mr. Vauquelin next inquired what might be the circumstance, that led him into an error when he first analysed the topaz; and he imagines, that it was his treating the alcaline mass with muriatic acid, instead of employing the sulphuric; and that probably not heating it to a sufficient degree to expel the fluoric acid, from fear of decomposing the muriate of alumine, he precipitated the fluoric acid combined with the alumine, when he added ammonia to separate the alumine from its muriatic solution. Circumstance that misled Vauquelin in his first analysis.

Examination of Crude Platina, by Messrs. Tennant and Wollaston.*

THE editors of the *Annales de Museum* observe, that platina, when taken from the mine, appears from the experiments of Descotils, Fourcroy, and Vauquelin, to be mixed with iron, chrome, and other metals, among which may be one or two that are new. Mr. Tennant has given names to two, and Dr. Wollaston to two others; but the French journalists congratulate their countrymen for being less hasty, and waiting till they are assured these new metals really exist. Platina.

Prize Questions in France.

The Society of Agriculture and the Arts, in the department of the North, proposes for the subjects of two prizes, which shall be adjudged, the first in the first of Fructidor, in the year XIII. and the second in the first fortnight of Fructidor, in the year XIV. the two following questions: Prize questions respecting wool; and an insect on the colsa.

First Question.—" What method of propagating, rearing, feeding, and housing the sheep of the race now existing in the department of the North, ought to be followed in this department, to obtain wool from these animals, equal in quality to the best wools from English sheep?"

Second Question.—An insect known in the country under the improper denomination of *puceron*, has this and several preceding years attacked and destroyed the greater part of the flowers of the colza.—" What is this insect? Under what generic and specific name have the most celebrated naturalists described it? What

* Bulletin des Sciences, No. XC. p. 234, Sept. 1804.

is its life, either in the ſtate of coleoptera in which it is found on the colza, or in the ſtate of larva? What natural enemies, what artificial means of deſtruction can be oppoſed to it in either ſtate? Generally, what care can preſerve the colza from its ravages?"

Each of theſe two prizes will be a gold medal of the value of 150 francs.

Ruſſian Circumnavigation.

Ruſſian voyage round the world.

By a courier, expedited by the governor of Kamſchatka, news was received at Peterſburgh on the 21ſt of December, that the veſſels belonging to the expedition round the world under the orders of M. de Kruſenſtern, had arrived, on the 26th of July laſt, at the harbour of Peter and Paul, in Kamſchatka, and that, up to this time M. Kruſenſtern had not loſt a ſingle man of his retinue, nor had he any ſick in the ſquadron. In his voyage he had viſited the Marqueſas and Sandwich Iſlands. M. Kruſenſtern purpoſed ſailing for Japan, towards the end of Auguſt.

Europeans found at the Marqueſas, who had forgotten their own language.

At one of the Marqueſas, where the veſſels ſtopped a few days, he had taken on board a Frenchman and an Engliſhman, to bring them back to Europe. Captain Kruſenſtern ſays he had not yet been able to diſcover how theſe two individuals had come to this iſland; both of them having completely forgotten their original language. He thought however that he could comprehend that they had arrived there on board an American veſſel which had been ſhipwrecked on the coaſt. The Frenchman ſpeaks the language of the iſlanders very well, and has adopted all their cuſtoms, habits and manner. There is no doubt that they will ſoon recover the uſe of their language, in a daily intercourſe with Europeans; and that they will then be able to give an account of their adventures, as well as information reſpecting the iſlanders, among whom they have lived ſo long. At leaſt, this is expected with impatience.

Geological Journey from the Academy at Warſaw.

Journies of diſcovery in mineralogy and natural hiſtory.

The Society of the Friends of the Sciences at Warſaw have charged two of its members, M. Carteau and Stacio, to undertake a mineralogical and phyſical expedition into the Carpathian mountains. Another member of the ſociety has already examined

examined the eaſtern part of them, with relation to mineralogy, geogony and oryctognoſy. At this time he is travelling over the mountains in the interior of Auſtria, from whence he will proceed to upper Italy, and to the Alps of Switzerland. When this journey is completed, he will undertake another to Caucaſus.

Lalande's Propoſal of a new Scale of the Thermometer.

M. de Lalande propoſes to adopt a thermometer-ſcale, which ſhall remedy all the inconveniences of thoſe now in uſe. His mean point is taken from the natural ſtate of the globe, which he fixes at $9\frac{1}{2}$ degrees of Reaumur's thermometer, and he takes the 10 millionth part of the volume of mercury for the meaſure of a degree. Among the advantages of this inſtrument he reckons a ſimplification of expreſſion, which will give a facility to comprehending what was before without meaning to the public. For example, the degree of heat of common ſummers, and the degree of cold of our mean winters, will be both expreſſed by 30: The degree of 40 will indicate a hot ſummer and a ſevere winter, &c.; another advantage will be derived from the ſmaller interval of the degrees, which will remove the neceſſity of having recourſe to fractions in the greater number of obſervations. The boiling point of water is $+ 132°.8$ of the propoſed thermometer, and $- 74°.4$ is the point of the congelation of mercury. Ice melts at $- 17°.9$ and $- 44°.2$ is the zero of Fahrenheit. New ſcale of thermometer.

Two Kinds of Honey.

In a note to Dr. Delametherie, Prouſt announces the diſcovery of two kinds of honey; the one liquid, the other dry, not deliqueſcent, cryſtallizable in its manner, and leſs ſaccharine than ſugar: they are ſeparated by ſpirit of wine, to which end granulated honey muſt be operated on. Prouſt on honey.

J. de Phyſique.

Palladium.

Experiments made by Meſſrs. Roſe and Gehlen, and others by Richter, to obtain palladium, are given at length in the Journal of Chemiſtry, publiſhed in German by Klaproth and Richter. Attempts to form palladium with mercury and platina.

Theſe

These philosophers followed Mr. Chenevix's process with great care, but did not produce that metallic body. In the precipitation of muriate of mercury and muriate of platina, they had a black powder, which always afforded the metals separate from each other.

Richter was not more succefsful. He verified that the green sulphate of iron does not decompose either the muriate of mercury or that of platina. The other facts he observed were to the same effect as those of Rose and Gehlen. He always found the mercury of his precipitate separated from the platina by heat.

Traité élémentaire d'Histoire Naturelle, &c. *An elementary Treatise on Natural History, by* A. M. CONSTAT DUME'RIL: *Composed by Order of Government for the Use of the National Lyceums,* 1 *Vol.* 8vo. *Paris* *.

Elementary works on natural history very defective.

There is not one of the sciences, the elementary works of which have been so long neglected as natural history. Sometimes this title has been given to collections of tales fit only to amuse children, but not calculated to make them acquainted with nature as a whole, and with the progress of the science: at other times authors have entered into discussions too abstruse, or contented themselves with a mere nomenclature, always dry and sterile to beginners, to whom names give no idea of objects with which they are unacquainted. Mr. D. has preserved a just medium between these extremes, while he observes an accurate and methodical arrangement. He makes us acquainted with the whole of the productions of nature, and the method of studying and classing them, choosing for examples in every section such as are most remarkable for their uses or singularity; he continually excites the attention and curiosity of his pupils; and he presents to them a number of facts necessary to be known.

Mr. D. has pursued a better method.

His arrangement proceeds from the most simple things to the more complex.

In this work Mr. D. has adopted an arrangement, the reverse of what is usually employed in books of natural history; that is, he always proceeds from the most simple to the most complex. He begins with unorganized substances, proceeds hence to plants, and lastly to animals; and in these he commences

* Bulletin des Sciences, No. 90, p. 236, Sept. 1804.

with

with zoophytes; and ends with man. This arrangement has the advantage of inftilling ideas gradually into the mind of the fcholar, and avoiding a number of repetitions and anticipations. The hiftory of organifed fubftances gives him an idea of bodies unmixed with any other ideas: that of vegetables fhows him organization and life in their moft fimple ftate: and thefe he perceives gradually become more complicated as he afcends through the different claffes of the animal kingdom, fo that the hiftory of each clafs is but little more than an expofition of the organs and faculties it enjoys beyond thofe of the preceding.

Its advantages.

Though the difcuffion of any new idea feems contrary to the effence of an elementary work, it is obvious, that fuch a work cannot be well executed but by a man capable of confidering the whole of a fcience in its proper point of view. In this refpect the naturalift will here read with pleafure the article of general obfervations placed at the head of each part: he will diftinguifh the hiftory of infects, which Mr. D. has treated after a new plan: and he will notice the chapter on man, in which the author difplays the phyfical characters that diftinguifh man from brutes, and the confequences refpecting his manners that arife from his very ftructure. This chapter may be confidered as the connecting link between phyfics and metaphyfics.

Mr. D. has treated the infects on a new plan.

Elemens de l'Art de la Teinture, &c. *or Elements of the Art of Dying; with a Defcription of the Procefs of Bleaching by the oxigenated Muriatic Acid. Second Edition: by* C. L. *and* A. B. Berthollet. *Paris.*

This new edition of a work of the firft merit and celebrity is fpoken of in the Foreign Journals, as being confiderably improved by the former author and his fon. The great perfpicuity and order which are feen in all the productions of this eminent chemift, and his own unremitted labours in the fcience, are a fure guarantee to the fame effect.

Berthollet on dying.

Confiderations on Organized Beings. By J. C. Delametherie.

The fcience of natural hiftory is indebted to M. Delametherie for feveral interefting works which contain ideas of advantage to the progrefs of human knowledge. Befides the

Delametherie on organized beings.

Journal de Phyfique, &c. of which he is the editor, he has publifhed a Theory of the Earth ; a Treatife on Man ; different Phyfiological Views of the animal and vegetable Kingdoms, on Vital Air, &c. In this new work the author compares the ftructure of animals and plants, and applies to vegetables the different fyftems or apparatufes of organs and vital functions, which X. Bichat has propofed in his General Anatomy. M. Delametherie has benefitted by the experiments and labours of feveral philofophers of merit. All, perhaps, may not adopt fome of the opinions he offers, the conqueft of minds being ftill more difficult than that of hearts ; but they, at leaft, deferve examination, and may lead to unexpected refults.

Bibliotheque de Sonini.

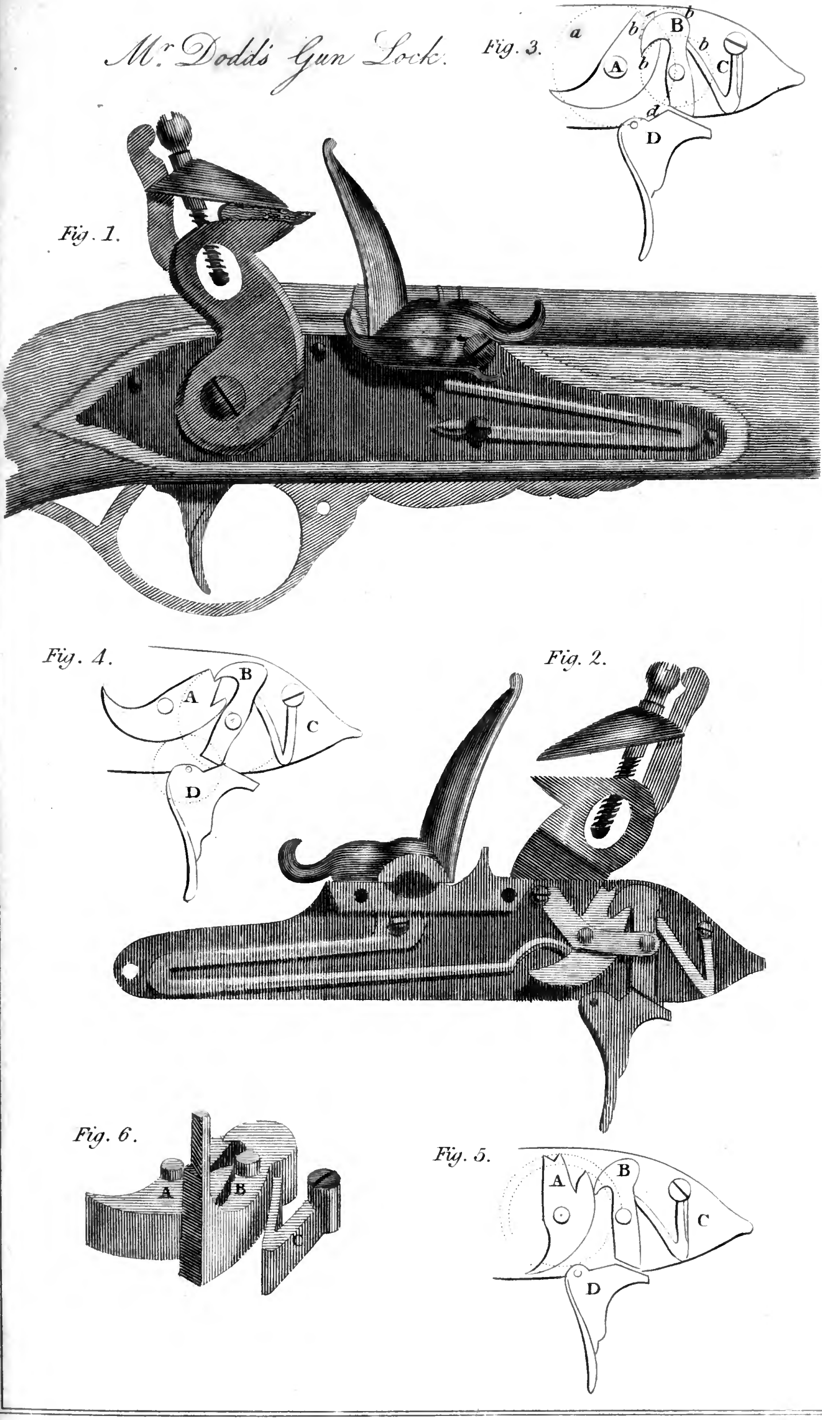
To CORRESPONDENTS.

My beft acknowledgements are due to Mr. D. *who has favoured this work with a precife ftatement of the elucidation of Mr.* BOSWELL'S *fecond propofition ; but he will perceive that the paper of Mr.* GOUGH, *which was already printed when his letter came to hand, has rendered it unneceffary.*

I believe the readers of this Journal will think with me, that the difpute between C. L. *and Mr.* E. WALKER *has proceeded at leaft as far as the interefts of fcience demand. In a letter from* C. L. *before me, I have fuppofed the following explanatory fentences to afford no new ground for difcuffion, and therefore extract them, and hope the bufinefs will end here.* C. L. *fays :*

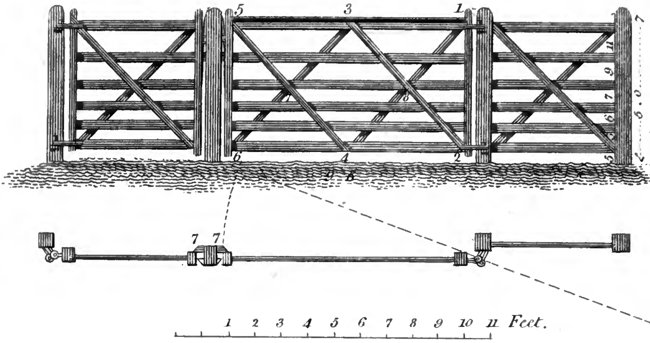
"*I cannot conceive how expreffions fo plain could be mifunderftood, and hope you will permit me to endeavour to make them plainer if poffible.* I have denied Mr. WALKER'S facts ; *that is, I have denied the* truth of *his narrative refpecting* certain fuppofed facts, *and I have pointed out an eafy way of convincing me and the world that he is not deceived ; namely, that you fhould repeat his experiments, and fee if what he afferts be true. But Mr.* WALKER declines examining the remainder of my letter, *which is an indiftinct, though fufficiently clear way of faying, that he does not choofe to rifk his fuppofed facts by putting them to fuch a trial.*"

Mr. Dodd's Gun Lock.

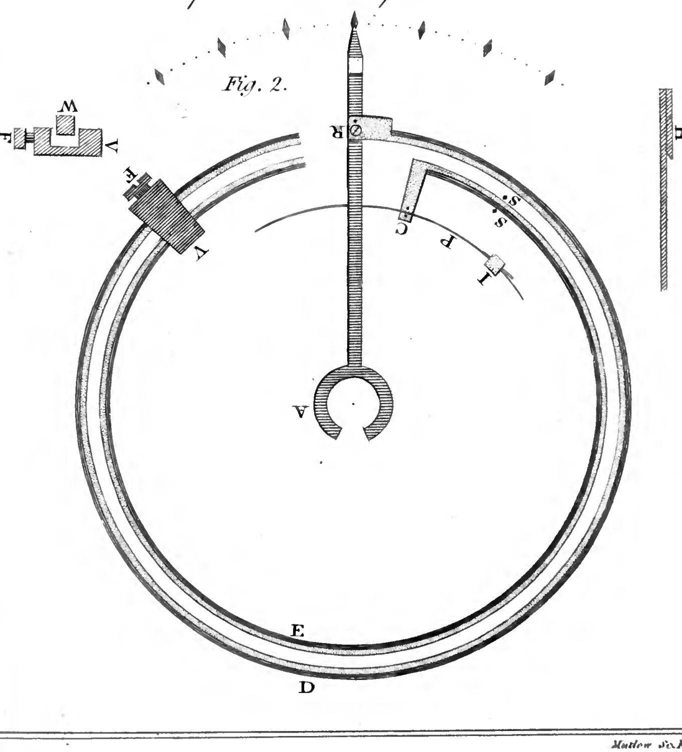


Mr. Waistell's improved Field Gate.

Fig. 1.



Mr. Scott's compensation Curb, for a Time piece.



Mutlow sc. Russell Ct.

Bavarian Method of evaporating Salt Waters.

Fig. 3.

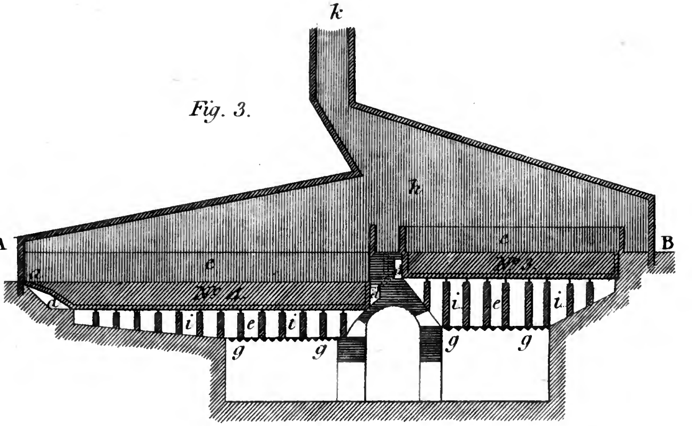


Fig. 4.

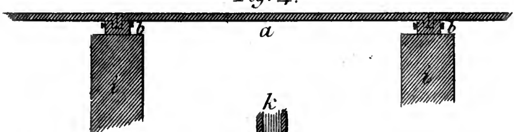
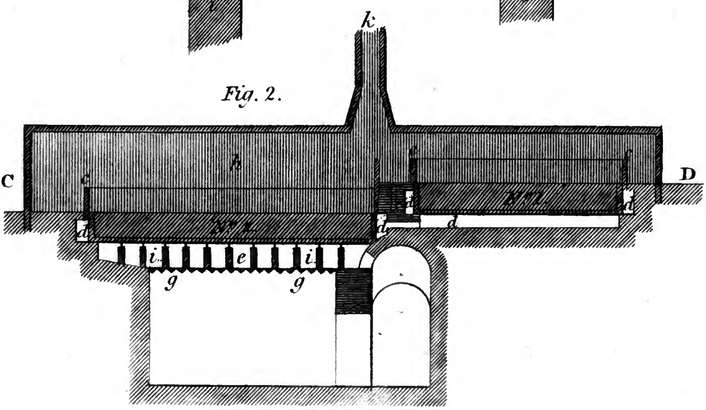
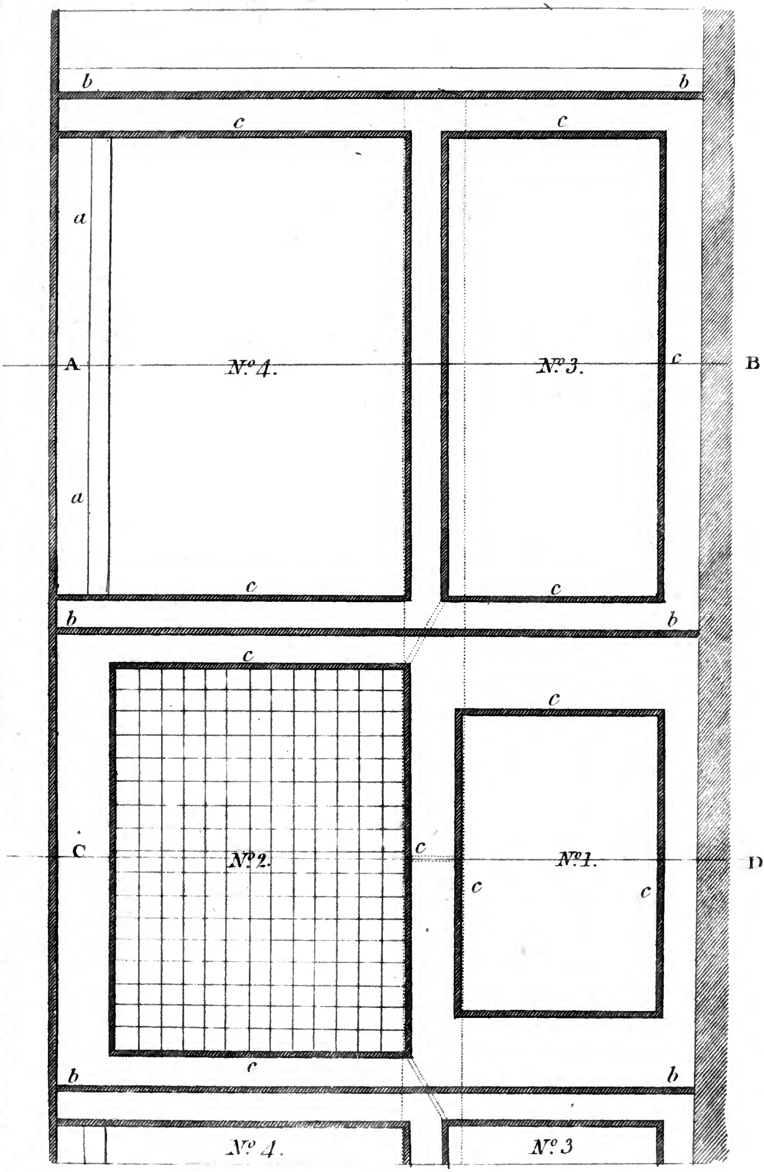


Fig. 2.

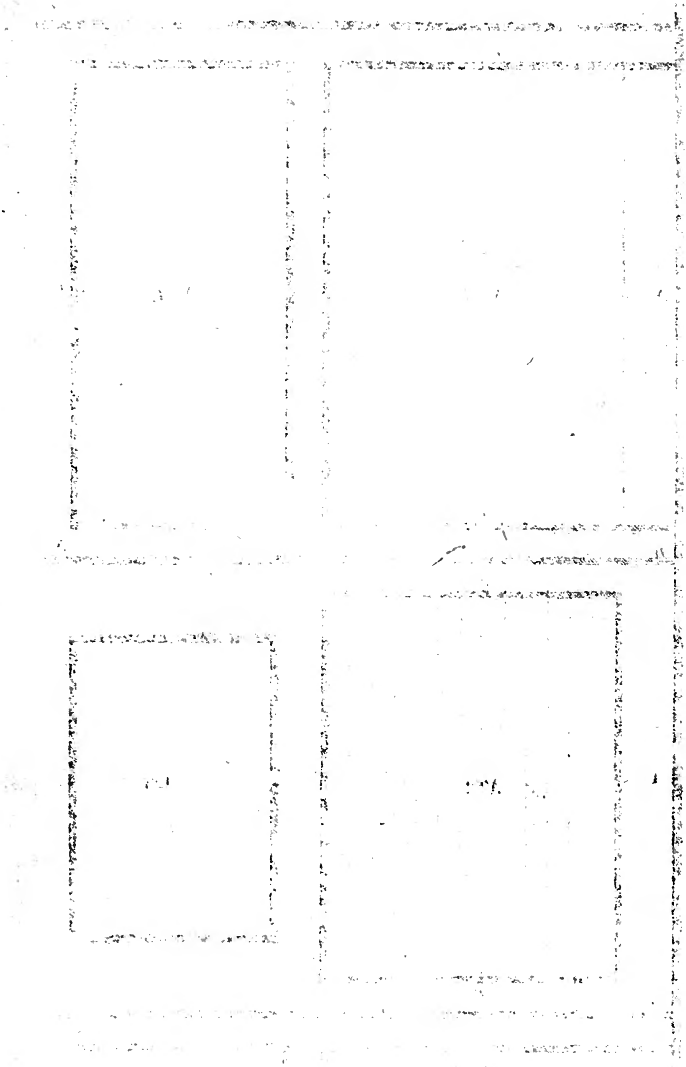


Mutlow Sc. Russell Co.t

Fig. 1.



Mutlow Sc. Russell Co.t



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE, 1805.

ARTICLE I.

Experiments on the Gafes obtained by the deftructive Diftillation of Wood, Peat, Pit-Coal, Oil, Wax, &c. with a View to the Theory of their Combuftion, when employed as Sources of artificial Light; aud including Obfervations on Hydro-Carburets in general, and the Carbonic Oxide: By Mr. WILLIAM HENRY. *(Communicated by the Author.)*

THE gas obtained by the deftructive diftillation of pit-coal, has become an object of confiderable intereft and importance, in confequence of its fuccefsful application (by Mr. Murdoch, of Soho, near Birmingham*) to the purpofe of affording light. Having conftructed an Argand's lamp laft winter, with the view of effecting the combuftion of the gas on Mr. Murdoch's plan, I made previous trials with pure hydrogen gas, with carburetted hydrogen obtained by paffing water over ignited charcoal, and with the carbonic oxide; but found that each of them burned with fo trifling a production of light, as to be altogether unfit for the purpofe of illumination; while the light evolved by the gas from coal was little, if at all, inferior to

Procefs of Mr. Murdoch to illuminate by gas from coal.

Neither hidrogen nor carbonic oxide give any notable light; but the gas from coal is nearly equal to oil.

* See the ftatement of Mr. M's claim to the difcovery in the poftfcript.

that from good ſpermaceti oil. So eſſential a difference in the combuſtion of theſe gaſes, induced me at firſt to believe, that the gas from coal owes its illuminating property to ſomething mechanically ſuſpended in it; but I was afterwards ſatisfied, that though it contains, when recently prepared, much that is ſubſequently depoſited, yet that its quality of burning with a bright and compact flame belongs to it, though certainly with conſiderable diminution, as a permanent gas, after the ſeparation of all condenſible matter. It appeared, therefore, worthy of inveſtigation to determine, on what the ſuperior fitneſs of the gas from coal, for evolving light, depends; and to connect the theory of its combuſtion with that of other ſubſtances, commonly employed as ſources of artificial light. With this view, numerous comparative experiments were made on the rapid combuſtion of this gas with oxigenous gas in cloſe veſſels, and alſo on that of other inflammable gaſes; and their compoſition may be inferred from the products of theſe experiments, the principal reſults of which are contained in the following table:

This laſt gas burns with much (though diminiſhed) light after it has depoſited its condenſible matter.

Products by burning the gas from coal with oxigen and alſo other gaſes.

Table of reſults.

Kind of Gas.	Oxigen Gas required to ſaturate 100 Meaſures.	Carbonic Acid produced.
Pure hydrogen, -	50 to 54	
Gas from moiſt charcoal,	60 - -	35
——— wood (oak).	54 - -	33
——— dried peat,	68 - -	43
——— coal, or cannel,	170 - -	100
——— lamp-oil,	190 - -	124
——— wax, -	220 - -	137
Pure olefiant gas,	284 - -	179

Now if it be aſſumed (which I believe is as nearly as poſſible the fact) that in the formation of each meaſure of carbonic acid, in the above experiments, an equal volume of oxigen gas is employed*, we ſhall learn, by deducting the numbers in the

If the meaſure of carbonic acid produced, be deducted from the whole oxigen employed, the remainder will expreſs the meaſure of oxigen which was employed in burning the hydrogen of the gas; and this laſt doubled will give the

* Mr. Cruickſhank takes it for granted, as the baſis of his calculations, that in forming ſix meaſures of carbonic acid, ſeven meaſures of oxigenous gas are employed. This proportion I believe to be over-eſtimated. Dr. Prieſtley obſerves, (on Air, 2d Edition, III. 377), " I heated $8\frac{1}{4}$ grains of perfect charcoal in 70 ounce meaſures of dephlogiſticated air, when it ſtill remained 70 oz.

m.;

the third column from the corresponding one in the second, what proportion of the consumed oxigen has been allotted to the saturation of the hydrogen of each hydro-carburet. Thus, for example, in the combustion of the gas from coal, 70 parts of oxigen have disappeared, besides that which has entered into the carbonic acid; and, since each measure of oxigen saturates two of hydrogen gas, the gas from coal must contain, in 100 measures, a quantity of hydrogen which, expanded to its usual elasticity, would occupy 140 measures. By a similar mode of estimation, the quantity of hydrogen in other species of inflammable gas may be ascertained; *viz.* by subtracting the number in the third from the corresponding one in the second column, in each instance, and doubling the remainder.

measure of hydrogen originally present.

The above experiments sufficiently explain why the gas from coal evolves so much more light, during combustion, than either hydrogen or the hydro-carburet from moist charcoal, because, in an equal volume, it includes, in its composition, above thrice the quantity of inflammable matter present in hydrogen gas, and nearly thrice as much as is contained in the gas from moist charcoal. The appreciation of the degree of combustibility of each gas, by the quantity of oxigen required for its saturation, entirely agrees, as might naturally be expected, with that founded on the phenomena of silent combustion in an Argand's lamp; for each gas seemed to me to evolve light, as nearly as could be judged, in proportion to the quantity of oxigen consumed by its detonation in a close vessel. Above all others, the olefiant gas * is decidedly entitled to rank, by the splendor and beauty of the light which it yields; and the violence of its detonation, when fired with a mixture of oxigen gas, also surpasses that of every other inflammable gas. By exploding only .03 of a cubic inch with .17 of oxigen gas, a strong glass tube was

These facts explain the greater illumination from the coal gas. The light is greater the more oxigen is required for the combustion.

m.; but, after washing in water, was reduced to 40 oz. m." In this experiment one grain and ¼ of charcoal was consumed, and 30 oz. m. of carbonic acid were generated, without any change in the volume of the oxigenous gas.

* A full abstract of the memoir of Messrs. Deiman, &c. on this interesting gas, may be seen in the 1st Vol. of the 4to Series of this Journal. Its characteristic property is that of being rapidly condensed into oil, by contact with oxigenized muriatic acid gas.

 shattered

ſhattered with violence; and a Volta's eudiometer, $\frac{1}{4}$ of an inch in thickneſs was burſt by leſs than a cubic inch of a mixture of the two gaſes. The ſpecific gravity of the inflammable gaſes, when perfectly freed from carbonic acid, is another competent teſt of their fitneſs as ſources of light. Thus the ſpecific gravity of the gas from moiſt charcoal, (common air being 1000) according to Cruickſhank is 480; of the hydro-carburet from alcohol 520, and of the olefiant gas, as determined by the Dutch chemiſts, 909.

Specific gravity of an inflammable gas (freed from carbonic acid) is a teſt of its fitneſs to afford light.

From the limitation to the proportions, in which bodies in general, having a ſuſceptibility of chemical union, are capable of combining, it ſeems to me reaſonable to infer, that carbon and hydrogen do not unite in all poſſible proportions, forming ſo many diſtinct compounds; but that the various inflammable gaſes are mixtures of a very few ſimple ones. Of thoſe at preſent known, pure hydrogen gas; the carburetted hydrogen, which by combuſtion affords an equal bulk of carbonic acid, and conſumes twice its bulk of oxigen; the carbonic oxide; and the olefiant gas, it will appear, may be traced in the mixed gaſes comprehended in the forgoing table. The gas from coal I apprehend to be principally hydro-carburet, with perhaps ſome portion of carbonic oxide, the preſence of which laſt is rendered probable, becauſe the gas from coal is ſaturated by leſs than twice its bulk of oxigen, though it gives an equal volume of carbonic acid. Now the gas from marſhes, which, with Mr. Cruickſhank, Mr. Dalton finds to be hydro-carburet, contaminated with about 20 *per cent.* azotic gas, conſumes, making allowance for this adulteration, double its volume of oxigen gas; and ſince the gas from coal requires a leſs proportion than this of oxigen, and yet gives an equal product of carbonic acid with that from marſhes, it is fair to preſume, that it muſt previouſly have contained ſome oxigen, which, after waſhing it with lime-water, can ſubſiſt in no other ſtate than that of the carbonic oxide.

It is probable that the inflammable gaſes are mixtures of few ſimple gaſes.

Gas from coal appears to be hydro-carburet with perhaps ſome carbonic oxide.

The gas obtained by decompoſing water over ignited charcoal, is moſt probably a mixture of carbonic oxide with hydrogen gas, and perhaps a little hydro-carburet. On no other preſumption can the reſults of its combuſtion be explained; ſince the quantity of oxigen required in ſaturating 100 meaſures is only ten more than are conſumed by 100 meaſures of pure hydrogen, though 35 m. of carbonic acid, containing at leaſt

Gas from ignited charcoal and water is probably carbonic oxide with hydrogen and a little hydro-carburet.

35 of

.35 of oxigen, are found after combuſtion. Now, according to Mr. Cruickſhank, theſe 35 m. of carbonic acid, if formed from carbonic oxide, would require only 15 additional meaſures of oxigen gas, which is not very remote from the truth. Reaſoning in the ſame mode, the gaſes from wood and from peat will appear to be mixtures in different proportions of two at leaſt of the above-mentioned, viz. hydrogen, and the carbonic oxide. The reſults of the combuſtion of the gaſes from wood and peat evince that they differ conſiderably from that obtained from coal; and contain much leſs uncombined inflammable matter. Another circumſtance of diſtinction alſo is, that, before being waſhed with lime-liquor, the gas recently prepared from wood or from peat, contains from $\frac{1}{4}$ to $\frac{1}{3}$ its bulk of carbonic acid; whereas the gas from coal loſes by this ablution only from $\frac{1}{27}$ to $\frac{1}{13}$. * In my firſt experiment, I found a large admixture of azote in all theſe gaſes; but this afterwards proved to be accidental and not eſſential; ſince by careful diſtillation in glaſs retorts, of the ſubſtances that afforded them, the gaſes were obtained entirely free from this contamination.

Gaſes from wood and from peat contain leſs of uncombined inflammable matter than the coal gas.

The gaſes obtained by the deſtructive diſtillation of oil and of wax, it may be obſerved in the table, conſume conſiderably more oxigen than the gas from coal. This circumſtance firſt led me to ſuſpect that they might poſſibly be mixtures of the olefiant gas with carburetted hydrogen; and on applying the oxygenized muriatic acid gas, this ſuſpicion was fully verified. One meaſure of the gas from oil with one of the oxygenized gas, were reduced ſpeedily to $1\frac{3}{4}$; a like diminution was produced in the gas from tallow; and that from wax had its bulk ſtill further contracted, only $1\frac{1}{2}$ m. being left by ſimilar proportions.

The gaſes from oil and from wax are pure hydro-carburets, except that the firſt contains one-eighth and the latter one-fourth of olefiant gas.

* The condenſible products alſo of coal probably differ from thoſe of wood and peat. If an intermediate veſſel be placed for their reception, it emits, after the diſtillation, a ſtrong ſmell of ammonia. This was long ago obſerved by Lord Dundonald, who enumerates, among other products of coal, the volatile alkali (ſee a pamphlet "on the Uſes and Qualities of Coal Tar," publiſhed by his Lordſhip in 1785.) This production of ammonia I have not obſerved from peat or wood; nor do I find it mentioned in a Hiſtory of Peat, including the reſults of its diſtillation, &c. publiſhed in the ſecond vol. of the Edinburgh "Eſſays Phyſical and Literary."

In order to aſcertain how much of the diminution was owing to the condenſation of oleſiant gas, the proportion of oxygenized acid, required for the ſaturation of that gas, was carefully aſcertained. After ſeveral trials, it was found that 3 m. of the oxygenized acid with $2\frac{1}{2}$ pure oleſiant gas, prepared according to the Dutch chemiſts, left only 0.15 m. of common air derived from the veſſels. It appears, therefore, that the gas from oil and from tallow contains about $\frac{1}{8}$, and that from wax $\frac{1}{4}$ oleſiant gas, the reſt being pure hydro-carburet.

Hence it is ſeen why waſhing diminiſhes the product of carbonic acid afforded by burning an hydrocarburet. For it takes away the oleſiant gas.

The reſults of theſe experiments, in connection with a fact communicated to me by Mr. Dalton, explain ſome circumſtances obſerved by Mr. Cruickſhank, for which that ingenious chemiſt was at a loſs to account, viz. the great variation in the products of carbonic acid, obtained by burning the ſame hydrocarburet when waſhed and when unwaſhed, or when long kept in contact with water; though the gas, when originally procured, was perfectly free from carbonic acid. The oleſiant gas, Mr. Dalton has aſcertained, is far more abſorbable by water than other ſpecies of hydrocarburet, viz. in the proportion nearly of $\frac{1}{8}$. Now the gas from camphor, I find to contain much oleſiant gas, and indeed this might have been inferred from Mr. Cruickſhank's own ſtatement, who obſerves that this gas, by admixture with oxygenized muriatic acid, undergoes a conſiderable diminution of bulk. The pure hydrocarburet, on the contrary, I have never ſeen at all condenſed by contact with this gas, in the rapid manner obſervable in oleſiant gas or mixtures containing it; though, by confinement together for ſome hours, they are mutually decompoſed into common muriatic acid, carbonic acid, and water. The hydrocarburets from ether and alcohol alſo contain oleſiant gas; and this, I apprehend, will be found to be the fact with all inflammable gaſes, which by combuſtion give more than their own bulk of carbonic acid. The variable products of carbonic acid, obtained in Mr. Cruikſhank's experiments, from equal quantities of different hydrocarburets, cannot, therefore, be conſidered as denoting ſo many diſtinct ſpecies of carburetted hydrogen; but as owing to the admixture with this of various proportions of oleſiant gas.

Hydrocarburets from ether and alcohol alſo contain ol. gas; which is the general cauſe of the difference in carb. acid products by Mr. Cruikſhank.

Error of that author reſpecting the conſtitution of carburetted hydrogen gaſes.

It will not, I am perſuaded, be regarded as indicating a wiſh to detract, in the ſmalleſt degree, from the credit due to Mr. Cruikſhank, whoſe memoirs on the hydrocarburets and carbonic

carbonic oxide I eſtimate among the moſt ingenious, and generally ſpeaking, the moſt ſatisfactory examples of chemical reſearch, if I obſerve that the part of his table (vol. V. p. 8 of the 4to ſeries of this Journal) which relates to the conſtitution of the carburetted hydrogen gaſes, I conſider as entirely erroneous. To excite ſtrong ſuſpicion of the accuracy of the proportions aſſigned to theſe gaſes, it is ſurely ſufficient that one of them (the gas from moiſt charcoal) is ſtated to contain in 100 cub. in. = 14½ grains, no leſs than 9 grains of water, a proportion abſolutely inconceivable; and the ſame objection applies, in a leſs degree, to the other caſes. Now in 100 cubic inches of the muriatic acid gas, I found the abſolute quantity of combined water to be only 1.4 gr. (Phil. Tranſ. 1800); and it is rendered highly probable, by the experiments of Clément and Deſorme (*Ann. de Chim.* XLII. 121) that all gaſes contain the ſame quantity of water. In the inſtance of the gas from charcoal, Mr. C. was moſt probably miſled, by not having ſuſpected the preſence of the carbonic oxide; and the correction is to be made as follows. One hundred cubic inches (=14½ grains) combined with the proper quantity of oxigen, gave 19 grains of carbonic acid, containing very nearly 4 grains of carbon; and ſuppoſing the carbon in the gas before combuſtion to have been in the ſtate of carbonic oxide, it would be combined with about 9 grains of oxygen, and would conſtitute 13 gr. or 43½ cub. in. of carbonic oxide.—There remains then only 1½ grain, of the 14½ ſubmitted to experiment to be accounted for, which is very exactly made up by the reſiduary 57½ cub. in. of hydrogen gas, taking the weight of 100 cub. in. of hidrogen to be 2.6 gr. The water contained in the gas may, I think, be ſet out of the queſtion; for it muſt be recollected that the product of the combuſtion is in part aeriform; and it may be conſidered as a tolerable approximation to the truth, that the gas from charcoal contains, in 100 inches, 43 of carbonic oxide, the remainder being principally hydrogen gas.

Carbonic oxide does not contain combined hidrogen, &c.

With reſpect to the preſence of hydrogen in the carbonic oxide, which has been a topic of controverſy, neither the fact nor the negative can, I think, be at preſent with certainty affirmed. If however any hydrogen be contained in it, I ſhould deem it an accidental and not an eſſential ingredient, and am of opinion that, if preſent at all, it exiſts in the ſtate of hydrogen

gen gas; for I find that the carbonic oxide is not expanded by electrical difcharges, which would affuredly happen if the carburetted hydrogen were one of its conftituents, or accidentally mixed with it.

Elucidation of the theory of lamps, &c.

To return to the theory of lamps, &c. it is proved by the preceding experiments, that the fubftances ordinarily employed as fources of artificial light, viz. oil, tallow, and wax, afford when fubmitted to an increafed temperature, much olefiant gas; and it has been juftly obferved by the editor of this Journal (4to feries I. 71) "that the wick of a lamp or candle furrounded by flame is exactly in the fituation of a body expofed to deftructive diftillation in a clofe veffel." In this cafe the feries of capillary tubes compofing the wick, ferve perhaps precifely the fame office as a tube horizontally difpofed in a heated furnace, through which an inflammable liquid is tranfmitted. The fuel previoufly melted, is drawn up into thefe ignited capillary tubes, and there refolved into olefiant and carburetted hydrogen gafes, from the combuftion of which gafes, and not merely of a condenfible vapour, it appears to me that the illumination chiefly proceeds. Hence it is not improbable, that the proportion of olefiant gas and hydrocarburet, obtained by the diftillation of any fubftance, will be a tolerable meafure of its fitnefs for affording light. In diftillations of this kind, however, the degree of heat is of confiderable moment, for I have found that the olefiant gas may be obtained or not, at pleafure, during the decompofition of ether, alcohol, oil, &c. by varying the temperature to which the containing veffels are expofed.

The oil, &c. is decompofed in the wick,

—into olefiant and carburetted hydrogen gafes, which are then burned. Whence the fitnefs of any fuel to give luminous flame may be known from its deftructive diftillation.

In the gafes from coal, peat, and wood, though thefe fubftances yield no olefiant gas, the defect is compenfated by an inflammable vapour diffufed through them when recent, and which is even not removed by paffing through a fmall quantity of water. Gas from coal, however, which had ftood over water upwards of a month, I have found burns with confiderably impaired brilliancy, though ftill with a far more denfe and bright flame than hydrogen gas, or the gas from charcoal.

The gafes from coal, &c. though they offered no olefiant gas, afford much light by an inflammable vapour when recent.

Manchefter, May 4, 1805.

POST-

POSTSCRIPT.

Since the preceding pages were written, I have examined a fresh portion of the gas from coal, obtained by very cautious distillation, with a view to ascertain whether any olefiant gas can be procured from that substance. Of this gas five measures mixed with five of oxygenized muriatic acid, were reduced to nine; from which it should appear that the gas from coal may possibly contain $\frac{1}{20}$ of olefiant gas. The production of oil, however, was not so manifest as in other instances; and I judged it to have happened chiefly because an iridescent film was visible on the surface of the water when held between the eye and the light.

The gas from coal appears by exp. with ox. m. acid gas to contain a small portion of ol. gas.

I am enabled also, by a letter received this morning from a friend who is well acquainted with the progress of Mr. Murdoch's experiments, in answer to some queries from me, to state specifically the grounds of that gentleman's claim to the important application of coal as a source of artificial light.— This I cannot do better than by an extract from the letter.

History of Mr. Murdoch's experiments for giving light by gas from pit-coal.

"In the year 1792, at which time Mr. Murdoch resided at Redruth in Cornwall, as Boulton and Watts principal agent and manager of engines in that county, he commenced a series of experiments upon the quantity and quality of the gases contained in different substances. In the course of these, he remarked, that the gas obtained by distillation from coal, peat, wood, and other inflammable substances, burnt with great brilliancy upon being set fire to; and it occurred to him, that by confining and conducting it through tubes, it might be employed as an economical substitute for lamps and candles. The distillation was performed in iron retorts, and the gas conducted through tinned iron and copper tubes, to the distance of 70 feet. At this termination, as well as at intermediate points, the gas was set fire to, as it passed through apertures of different diameters and forms, purposely varied with a view of ascertaining which would answer best. In some, the gas issued through a number of small holes, like the head of a watering pan; in others it was thrown out in thin long sheets, and again in others in circular ones, upon the principle of Argand's lamp. Bags of leather and of varnished silk, bladders, and vessels of tinned iron were filled with the gas, which was set fire to, and carried about from room to room, with a view of

ascer-

History of Mr. Murdoch's experiments for giving light by gas from pit coal.

ascertaining how far it could be made to answer the purpose of a moveable or transferable light. Trials were likewise made of the different quantities and qualities of gas produced by coals of various descriptions, such as the Swansea, Haverfordwest, Newcastle, Shropshire, Staffordshire, and some kinds of Scotch coals."

"Mr. Murdoch's constant occupations prevented his giving farther attention to the subject at that time; but he again availed himself of a moment of leisure to repeat his experiments upon coal and peat, at Old Cumnock in Ayrshire in 1797; and it may be proper to notice that both these, and the former ones, were exhibited to numerous spectators, who, if necessary, can attest them. In 1798, he constructed an apparatus at Soho Foundry, which was applied during many successive nights, to the lighting of the building; when the experiments upon different apertures were repeated and extended upon a large scale. Various methods were also practised of washing and purifying the air, to get rid of the smoke and smell. These experiments were continued, with occasional interruptions, until the epoch of the peace in the spring of 1802, when the illumination of the Soho manufactory afforded an opportunity of making a public display of the new lights; and they were made to constitute a principal feature in that exhibition. I do not know exactly at what time the first trials were made, or published in France. The first notice we received of them here, was in a letter from a friend at Paris, dated the 8th of Nov. 1801, in which he desires me to inform Mr. Murdoch, that a person had lighted up his house and gardens with the gas obtained from wood and coal, and had it in contemplation to light up the city of Paris."

"After mentioning the above, I think it is proper to state also, that in the ovens constructed upon Lord Dundonald's plan, at Calcutts in Shropshire, for the purpose of saving the tar, &c. which escapes during the coaking of coal, it has been usual for a number of years past to set fire to the large current of gas as it flies off, and thus procure a bright illumination. This however was not known to Mr. Murdoch, and was never seen by him."

Experi-

II.

Experiments on the Analysis of Goulard's Extract, or the Aqua Lithargyri acetati. By JOHN BOSTOCK, *M. D. Communicated by the Author.*

To Mr. NICHOLSON.

DEAR SIR.

I HAVE the pleasure to transmit to you some experiments on the analysis of Goulard's extract, which I hope you may think not unworthy of a place in your Journal.

I am, Sir,
Your obedient Servant,
JOHN BOSTOCK.

Liverpool,
May 5, 1805.

During the course of some experiments on the analysis of animal fluids, I was led to observe the effect of the *aqua lithargyri acetati*, or the extract of Goulard, as a coagulator of mucus, and particularly to notice the superiority of its power over that of the acetate of lead. From these circumstances I was induced to examine into the opinions that had been entertained respecting its composition, but was not able to obtain any satisfactory information. Although it is a compound so well known, and so frequently employed, it appears never to have been made the subject of chemical analysis. In Dr. Thomsom's System of Chemistry it is not distinguished from the common acetate of lead;* Dr. Murray informs us, that "it is merely a solution of acetate of lead in water, with an "excess of acid;†" and Dr. Duncan, Jun. conceives, that it does not differ from a solution of the acetate of lead of the same strength.‡ We meet with nothing specific respecting its constitution in M. Fourcroy's "*Systeme*", § nor is there any light thrown upon it by his acute commentator Proust.|| In this dearth of information, I proceeded to make the following experiments.

The extract of Goulard coagulates mucilage more than the acetate of lead.

It has not been examined.

*Chem. III. 52. † Mat. Med. II. 223. ‡ Edin. Disp. p. 506
§ VIII. 203. || Journ. Phys. LVI. 207.

To

Experiments on its compofition. The common acetate of lead was diffolved in water.

1. To 200 grains of diftilled water were added 60 grains of acetate of lead, in its ufual cryftalline ftate; the fluid was kept for about an hour at the boiling temperature, and was afterwards filtered. The refidue, when dried, did not amount to more than 2 grains; this was boiled in a frefh quantity of water, when about half of it was diffolved, but one grain ftill remained not acted upon by the water. It appeared, therefore, that a faturated folution of the acetate of lead was formed; it was tranfparent and colourlefs; it flightly reddened paper ftained with litmus.

This was precipitated by carbonate of potafh. It gave eleven parts carbonate of lead.

2. A folution of the carbonate of potafh was prepared, in the proportion of 11.25 grains of potafh to 100 grains of water. To 40 grains of the folution of the acetate of lead from No. 1, a quantity of this folution of potafh was gradually added; a copious precipitate of the carbonate of lead was produced; after the addition of 60 grains of the alcaline folution there was no farther precipitation, and the fluid flightly affected a paper foaked in the infufion of the mallow flower. The precipitate was carefully collected, and being dried by a gentle heat, afforded eight grains of carbonate of lead.

Goulard's extract gave eleven parts carbonate of lead.

3. Forty grains of *aqua lithargyri acetati* were treated in the fame manner with the carbonate of potafh; the precipitate formed appeared more copious than in the former experiment, and after the addition of 40 grains only of the alkali, no farther effect feemed to be produced; the fluid affected the mallow-paper in the fame degree as in the former experiment. The precipitate, being collected and dried, weighed 11 grains.

It did not redden litmus paper. Blue precipitate with litmus infufion.

4. The *aqua lithargyri acetati* did not in any degree redden litmus paper; a few drops of it were added to half an ounce of the infufion of litmus; a precipitate of a beautiful light blue was immediately formed, while the fluid was left tranfparent and nearly colourlefs.

The folution of acetate gave 4 gr. brittle clear refidue by evap.

5. Twenty grains of the folution of acetate of lead, No. 1, were flowly evaporated; the fluid became extremely vifcid, and at length, in fome degree, brittle and tranfparent, and affumed the appearance of dried gum. It weighed about four grains.

Aqua lith. acet. gave 5 gr. white opake refidue.

6. Twenty grains of the *aqua lithargyri acetati* were evaporated in the fame manner; it became white and opake, and when the procefs was completed, it exhibited the appearance of a number of fcales of a pearl colour. It weighed a little more than five grains.

7. A

7. A ſolution of gum Arabic was formed, in the proportion of one part of gum to 100 parts of water. One grain of this ſolution was added to 39 grains of water, ſo that the gum only conſtituted $\frac{1}{4000}$ part of the ſolution; a ſingle grain of Goulard dropped into it produced a perceptible opacity. Gum precipitated by Goulard's extract.

8. Twenty grains of the ſaturated ſolution of the acetate of lead, had one grain of the ſolution of gum added; the effect was barely viſible, certainly leſs than in the former experiment. —but ſcarcely by acetate of lead.

I am far from conſidering theſe experiments as ſufficient to afford a complete inveſtigation of the ſubject; but I think they may enable us to make ſome advances towards the truth.

The 40 grains of the ſolution No. 1. contain 11.6 grains of acetate of lead; by the addition of the alcali, this was converted into eight grains of the carbonate of lead. Theſe eight grains of carbonate conſiſt of 6.72 grains of the yellow oxide, and 1.28 grains of carbonic acid.* The 6.72 grains of yellow oxide conſiſt of 6.12 grains of pure lead, and .6 grains of oxigen, † ſo that the 40 grains of the ſaturated ſolution contain a little more than 6 grains of pure lead. Forty grains acetate contain ſix grains metallic lead.

By employing the ſame reaſoning to the analogous experiment with Goulard, we may conclude, that the 11 grains of carbonate produced in this caſe, conſiſt of 9.24 grains of the yellow oxide of lead, and 1.76 grains of carbonic acid; the 9.24 grains of oxide will be compoſed of 8.4 grains of pure lead, and .84 grains of oxigen, ſo that the 40 grains of the *aqua lithargyri acetati* contain nearly $8\frac{1}{2}$ grains of pure lead. Forty grains Goulard contain $8\frac{1}{2}$ of lead.

We ſhall not find it ſo eaſy to aſcertain preciſely the quantity of acetic acid which enters into the compoſition of the acetate of lead, and the *aqua lithargyri acetati* reſpectively; but if we truſt to the experiments of M. Thenard ‡, we muſt conclude that 11.6 grains of acetate of lead, contain about three grains of the acetic acid. The quantity of acid in the *aq. lith. acet.* is leſs than that in the acetate of lead, in the proportion of 40 to 60; therefore the 40 grains of *aq. lith. acet.* will only contain two grains of acid. Deduction of the reſpective quantities of acid.

Hence it follows, that 40 grains of the ſolution of acetate of lead conſiſt of, Component parts of each.

* Thomſon's Chem. III. 50. † Prouſt, Journ. de Phyſ. LVI. 206. ‡ Nich. Journ. VI. 223.

Lead

	Grs.			Grs.
Lead - -	6.12	The fame quantity of Goulard will confift of - -	Lead	8.4
Acetic Acid	3.		Acid	2.
Oxygene	.6		Oxygene	.84
Water	30.28		Water	28.76
	40.00			40.00

converting thefe proportions into quantities of 100 grains each they will be as follows,

	Sol. acet. lead.	Aq. lith. acet.
Lead - - -	15.3	21.
Acetic acid -	7.5	5.
Oxygene -	1.5	2.1
Water - -	75.7	71.9
	100.0	100.0

The experiments fhew that Goulard and the acetate are different falts.

From this ftatement it appears, that in the *aq. lith. acet.* the oxide of lead and the acid exift to each other in the proportion of 23 to 5, or of 100 to 21.74, and we find that M. Thenard has defcribed the perfect acetate of lead, as a falt in which the oxide and the acid exift in the proportion of 100 to 21.79. So near a coincidence between thefe two proportions can fcarcely be regarded as the mere effect of accident; but muft rather be confidered as a proof that the fubftances operated upon were nearly, if not altogether, identical. Admitting this to be the cafe, we muft conclude that the *aqua lithargyri acetati* is a faturated folution of the proper acetate of lead, that it is an effentially different falt from the fuper-acetate of lead, and that it is not, as has been imagined, an accidental compound, but an exactly neutralized falt, the conftituents of which exift in a conftant ratio to each other.

The former being at faturation,

—and the latter a fuper-acetate.

The neutral compound is moft eafily decompofed,

—and is therefore a better teft of mucus.

It happens in this, as in other inftances, that the ingredients compofing the completely faturated compound, poffefs a weaker affinity for each other than when they exift in a different proportion. To this circumftance muft be attributed the fuperior delicacy which Goulard poffeffes, as a teft of animal and vegetable mucus, over the fuper-acetate of lead, or the common *ceruffa acetata.* The *aqua lithargyri acetati* is fpeedily decompofed by the action of the atmofphere, in confequence of the oxide of lead which enters into its compofition having a ftronger affinity for carbonic than for acetic acid; this effect takes place in a lefs degree, in a faturated folution of fuper-acetate of lead.

From

From the firſt experiment we learn, that the ſuper-acetate of lead is more ſoluble in water than is generally imagined; Dr. Thomſon obſerves, that it is diſſolved only ſparingly *; yet we find that 100 parts of water retain in ſolution 27 parts of the ſalt. Super-acetate of lead more ſoluble than generally ſuppoſed.

III.

A conciſe View of the Theory of Reſpiration. By W. Brande', *Eſq.* *(From the Author.)*

THE term reſpiration implies the reception of atmoſpheric air into the lungs, and its ſubſequent emiſſion, after having produced changes in the blood neceſſary to the continuance of life †. Reſpiration,

No other gaſeous body being capable of producing theſe changes, it was natural to ſuppoſe, that until we became acquainted with the component parts of the atmoſphere, very little of the true nature of reſpiration could be underſtood. could not be explained till the atmoſphere was analyſed.

The firſt great ſtep towards the analyſis of the air was made by Dr. Prieſtley, who in the year 1774 diſcovered oxigen gas, called by him dephlogiſticated air. But we are indebted to Lavoiſier for the moſt accurate inveſtigation on this ſubject; who from many experiments, which it is not neceſſary here to relate, concluded that atmoſpheric air was compoſed of oxigen and azot, in the proportion of about 27 parts of the former to 73 of the latter. The air alſo contains a ſmall quantity of carbonic acid, and a conſiderable quantity of water (ſubject however to much variation) is always ſuſpended by it. Diſcoveries of Prieſtley, and Lavoiſier.

Some of the gaſes are totally unreſpirable, that is to ſay, incapable of being taken into the lungs; for whenever this is attempted, a ſpaſmodic affection of the epiglottis takes place, which by cloſing on the larynx, ſhuts up all communication with the organs of reſpiration. To this claſs belong all thoſe Gas which cannot be reſpired, or admitted into the lungs.

* Thomſon's Chemiſtry, III. 53.

† Reſpiration has been divided into, 1. Inſpiration, or the ingreſs of air into the cells of the lungs, cauſed by the enlargement of the cavity of the cheſt; 2. Into expiration, or the egreſs of air from the lungs, cauſed by the contraction of the cheſt.

gaſeous

Carbonic acid. Its effects; as described by Pilatre de Rozier.

gaseous bodies possessed of acid properties. The effects of carbonic acid are described as follows by Pilatre de Rozier:—He went into a brewer's tub which was full of carbonic acid gas; he at first felt a slight heat throughout his whole body, which produced a gentle perspiration; an itching sensation frequently obliged him to close his eyes, and on attempting to breathe, he was prevented by a very violent sense of suffocation. He wished to get out of the tub, but being unable to find the ladder, the necessity of breathing increased, he was seized with a violent giddiness, and felt a tingling sensation in his ears. He at length contrived to extricate himself, and although he then experienced no difficulty in breathing, he was unable to distinguish the objects around him; his hearing was also much impaired. On repeating the experiment he found, that as long as he remained without attempting to breathe, he could readily move or even speak, but whenever he tried inspiration, a violent sense of suffocation came on.

Gases which can be admitted into the lungs.

But there are certain gaseous bodies which may be drawn into the lungs, meeting with no opposition from the organs of respiration. Dr. Thomson has divided these into four classes. The first set, he observes, occasion immediate death, but produce no visible change in the blood; they occasion the animal's death, merely by depriving him of air, in the same manner as were he immersed in water: the only gases belonging to this class are hidrogen and azot. The second set occasion immediate death also, but at the same time produce certain alterations in the blood; and therefore kill, not only by depriving the animal of air, but by certain specific properties: the gases belonging to this class are, carburetted hydrogen, sulphuretted hydrogen, carbonic oxide, and perhaps also nitrous gas. The third set of gases may be breathed for some time without injury, but death ensues at last, provided their action be long enough continued: to this class belong the nitrous oxide and oxigen gas. The fourth set may be breathed any length of time without injury: the only gaseous body belonging to this class, is the air of the atmosphere, that compound of oxigen and azot every where surrounding the globe.

Hydrogen and azot kill by mere suffocation.

Gases which kill by a speedy positive action.

Nitrous oxide and oxigen kill by a slower action.

Atmospheric air maintains life.

After an animal has breathed a certain quantity of air for a given time, it becomes totally unfit for respiration; and if the air thus respired be chemically examined, we shall find that the oxigen is greatly diminished, and that a considerable quantity of carbonic acid gas has been produced.

This compound fluid loses its oxigen by that process, and carbonic acid gas is produced.

It

It appears from a number of experiments made by Dr. Hales, Dr. Menzies, and Mr. Davy, that the number of respirations made in a given time, as well as the quantity of air taken into the lungs, are liable to considerable variations in different people. Some have calculated the number of respirations at 14 only in a minute, others at 20; Mr. Davy informs us that he makes 26 or 27 in a minute; but having frequently endeavoured to count the respirations made by different people, in a given time, and without their knowledge, I have found them vary from 18 to 26 in a minute, most commonly, however, 20 or 21; and 21 in a minute make 30,240 in 24 hours.

The number of respirations made in a minute vary in different persons from 18 to 26; but are commonly about 20.

The quantity of air taken in at each respiration, must be in proportion to the size of the person and the capacity of his lungs. About 41 cubic inches of air are taken in at every natural inspiration.

Each respiration being about 41 cubic inches of air.

We now come to consider the changes which are produced, both in the air and blood, by respiration. 1. On the changes effected in the air. Dr. Priestley, M. Lavoisier, and Mr. Davy, have furnished us with many interesting and instructive experiments on this subject. The changes are, 1. That a portion of the air disappears; 2. That the air expired differs from that first taken into the lungs, in containing carbonic acid, and water in the state of vapour. Dr. Menzies has shewn, that $\frac{1}{20}$th of the air inspired disappears in the lungs, and the experiments of Lavoisier, which were made with much precision, differ but little from the above statement. I never knew the quantity of air which disappears to be less than $\frac{1}{19}$th part of the whole taken into the lungs; this may however be liable to variation in different people.

Phenomena of respiration.

Some air disappears; about one-nineteenth; and the expired air is altered.

It has hitherto been supposed that the portion of air which disappears, consists of the oxigen only: Mr. Davy has, however, given some very strong reasons for supposing that part of the azote likewise disappears during respiration. He supposes that the average quantity of air which is absorbed at every respiration, amounts to 1.4 cubic inches, of which 0.2 are azot and 1.2 oxigen.

The absorbed air was thought to be oxigen only: but appears to be in part azot.

Lime-water detects carbonic acid gas in the air emitted from the lungs, and the quantity of this gas may easily be estimated, by receiving the air expired into a graduated glass jar, standing over mercury; a little caustic soda being introduced, the absorption which takes place denotes the quantity of

The emitted air contains carbonic acid;

of carbonic acid. Lavoifier has eftimated the quantity of this gas thrown out from the lungs in 24 hours, at about 15.5 ounces troy. Mr. Davy makes the quantity thrown out in the fame time, amount to 37 ounces, which is about 1 cubic inch at every expiration *. The quantity however varies from 0.5 cubic inch to 1.5 at different times in the fame perfon, fo that this accounts for the great variation in the above-mentioned experiments. Moreover the proportion varies, in the fame individual, during the 24 hours; for I have found the quantity of carbonic acid gas emitted from my own lungs, to be rather lefs in the morning than towards the evening; but this alfo varies in different people.

about one cubic inch each expiration.

But this varies.

Aqueous vapour alfo emitted;

But watery vapour is alfo emitted in refpiration, the greateft part of which is probably given off by the exhalent arteries, which are fo copioufly difperfed on the furface of the lungs. A part is alfo emitted from the blood in the pulmonary veffels.

in quantity variable.

The eftimation of its quantity is attended with fome difficulty; acccording to Dr. Hales it amounts in a day to 20 ounces: this is however but of little confequence, for it is liable to much alteration.

The blood undergoes important changes during refpiration.

More important changes however than thofe juft mentioned are produced by refpiration, namely, the alterations produced in the blood; which fluid, returning from every part of the body by the veins, is poured into the heart; from whence, being propelled through the lungs, it is brought into contact with the air, undergoing certain changes which render it fit for the nourifhment and fupport of the body. Thefe changes, which are of a very complicated nature, have engaged the attention of feveral learned and ingenious philofophers. Dr. Thomfon has enumerated them as follows: 1. The blood abforbs air. 2. It acquires a florid red colour, and the chyle difappears. 3. It emits carbonic acid, and perhaps carbon. 4. It emits water, and perhaps hydrogen. Dr. Prieftley, M. Lavoifier, and Lagrange, have each adopted a different theory, by which they endeavour to explain and account for thefe changes produced by refpiration: they are all however liable to confiderable objections, and reft merely on the fuppofition that the oxigen is alone abforbed. Now Mr. Davy has fhewn, that at leaft a portion of the azot difappears in the lungs:

It abforbs air; becomes florid red; emits carbonic acid; and water; and perhaps hydrogen.

Varioustheories.

* Davy's Refearches, page 433.

lungs: he has even rendered it very probable that the air is not decompofed, but that it is abforbed unaltered by the blood; that it is decompofed during circulation; and that the ufelefs portion of azot is again given out. The following facts are in fupport of this opinion: "When the gafeous oxide of azot is refpired, its quantity is diminifhed, carbonic acid gas is evolved as ufual, and a quantity of azot makes its appearance. Now as this azot did not exift feparately, it muft have been produced by the decompofition of the gafeous oxide of azot; but its quantity being much lefs than the azot contained in the oxide of azot which had difappeared, it follows that a part of this laft gas had been abforbed unaltered; and if a part, why not the whole? In that cafe the azotic gas muft have been feparated from the blood by the fubfequent decompofition of the oxide of azot abforbed *." Atmofpheric air is compofed of exactly the fame ingredients as the oxide of azot, merely in different proportions, and in a ftate of lefs intimate chemical combination. It is moreover natural to afk, that if oxigen were alone abforbed by the blood, why fhould it not anfwer the fame purpofes as air? It is well known that this gas cannot be refpired for a length of time without producing fatal confequences; but even when it is refpired, the quantity (of oxigen) which difappears is much fmaller than when a like quantity of atmofpheric air is breathed for the fame time. Mr. Davy has given the following experiment in proof of this fact: He breathed 182 cubic inches of oxigen gas for half a minute, 11.4 c. inches difappeared; whereas when the experiment was repeated under the fame circumftances with atmofpheric air, the quantity abforbed amounted to 15.6 cubic inches.

Mr. Davy apprehends that azot is abforbed with the oxigen, &c.

Facts from the refpiration of oxide of azot.

Oxigen alone is not proper for refpiration:

Much lefs is abforbed than when a like quantity of atmofpheric air is breathed.

It was firft obferved by Lower, that the colour of venous blood, which is dark reddifh purple, was converted into the florid fcarlet colour of arterial blood, in its paffage through the lungs. The phenomena of refpiration, however, ftill remained unexplained, until Dr. Prieftley publifhed his experiments on the changes produced in venous and arterial blood when put in contact with certain gafeous bodies. "† He obferves, that having introduced pieces of the craffamentum of

Lower firft obferved the change of colour in venous blood by refpiration.

Prieftley fhewed the caufe; viz. the abforption of oxigen.

* Thomfon's Syftem of Chemiftry, Vol. IV. page 719.

† Prieftley on Air, Vol. III. page 71.

coagulated

coagulated ſheep's blood into dephlogiſticated air (oxigen gas); the blackeſt parts aſſumed a florid red colour, and that more readily than they would have done if common air only had been made uſe of: Whereas the brighteſt red blood became preſently black in any kind of air unfit for reſpiration, as in fixed air, *&c.*; and after having become black in phlogiſticated air (azot), it regained its red colour on being brought into contact with common air, the ſame blood becoming alternately black and ſcarlet, by being transferred from phlogiſticated into dephlogiſticated air, and *vice verſâ.*"

Theſe then may be regarded as the experiments which gave origin to all ſubſequent enquiries.

Deſcription of the proceſs of nutrition.

The food which is taken into the body is converted into chyle and excrement *; the former of which is abſorbed by a ſet of veſſels termed lacteals, which convey their fluid into the thoracic duct. The term lymph has been applied to that fluid which lubricates the ſurfaces of all the circumſcribed cavities of the body: This fluid is abſorbed by a ſet of veſſels termed lymphatics, which of courſe originate in every part of the body; they likewiſe terminate in the thoracic duct, which therefore is the great reſervoir of the abſorbent ſyſtem; it receives the chyle and lymph, and conveys them to the blood; they are here decompoſed, and converted into new ſubſtances neceſſary to the ſupport of the body. Now the coagulable lymph, or fibrina, appears to be the moſt eſſential part of the blood, for it is employed to ſupply the waſte of the muſcles, *&c.* and Dr. Thomſon has accounted for its formation in the following mannner †: " It follows," ſays he, " from the experiments of Fourcroy, that fibrina contains more azot and leſs hydrogen and carbon than any of the ingredients of the blood, and conſequently alſo than any of the ingredients of the chyle. In what manner the chyle, or a part of it, is converted into fibrina, it is impoſſible to ſay: We are not ſuffi-

Formation of fibrina.

* The food, on being received into the ſtomach, is converted into a pulpy ſubſtance termed chyme. This alteration is effected by a peculiar fluid called *gaſtric juice*, which is ſecreted by the internal coats of the ſtomach. The chyme thus formed is propelled into the duodenum, where it meets with the bile, which converts it into a fluid much reſembling milk, termed chyle, and into excrement.

† Thomſon's Chemiſtry, 2d Edit. Vol. IV. page 725.

ciently acquainted with the ſubject to be able to explain the proceſs. But we can ſee at leaſt, that carbon and hydrogen muſt be abſtracted from that part of the chyle which is to be converted into fibrina, and we know that theſe ſubſtances are actually thrown out in reſpiration. We may conclude then that *one* uſe of the air abſorbed is to abſtract a quantity of carbon and hydrogen from a part of the chyle by compound affinity, in ſuch a manner that the remainder becomes fibrina: Therefore one end of reſpiration is to form fibrina. Uſe of reſpiration in that connexion.

It appears then, from the above-mentioned facts, that the perfection of the blood is almoſt totally dependent on reſpiration; whenever therefore this function is ſuſpended but for a very ſhort time, death is the conſequence. Life cannot ſubſiſt without it.

It is well known that all the more perfect animals poſſeſs a temperature conſiderably higher than the ſurrounding atmoſphere; the cauſe however of this increaſed temperature, remained unexplained for a conſiderable time. At length Dr. Black's theory of latent heat became known, when ſeveral attempts were made to explain the cauſe of the increaſe of temperature, or ſtandard heat of the body, but none of them were ſatisfactory. Dr. Thomſon has however given us the following ingenious theory: As the air is abſorbed unaltered by the blood, it is evident that it will give out the greateſt portion of its caloric during circulation; that portion therefore which is emitted at the inſtant that the air combines with the blood, is united to the carbonic acid, converting it into the ſtate of gas, and the water into vapour. It appears moreover, that the heat of the blood is ſomewhat raiſed during circulation; for Mr. John Hunter found that the blood in the heart was a degree higher than in any other part of the body. The elevated temperature of the more perfect animals is cauſed by the combination and condenſation of air.

From the facts which have now been alluded to, it appears that the following changes are produced by reſpiration: The blood is propelled, by the contraction of the heart, into the pulmonary artery, which, by its numerous ramifications, conveys the blood into the ſmall branches of the air-cells of the lungs, which are of ſo fine a texture as to admit the abſorption of a portion of air. The blood having undergone this alteration, is returned into the heart by the pulmonary veins, from whence it is circulated over the whole body. During the circulation, the air which has been abſorbed undergoes a gradual decompoſition; carbonic acid and water are formed, which, Recapitulation.

together

together with a portion of azot, are returned by the veins, and thrown out as the blood paffes through the lungs. A frefh portion of air is at the fame time abforbed, and the above changes repeated.

Thefe then are the effects of refpiration, as far as we are at prefent acquainted with them; but this important branch of phyfiology ftill remains in confiderable obfcurity.

Arlington Street, WILLIAM BRANDE.
April 29, 1805.

IV.

Inftruction on the Proceffes difcovered by M. BRALLE, *of Amiens, for watering Hemp in Two Hours Time, in all Seafons, without injuring its Quality *. Publifhed by Order of the Minifter of the Interior of France.*

Experiments on hemp, by order of the French government.

IN the month of Fructidor in the year XI. (September 1803.) the government called to Paris M. Bralle, of Amiens, the inventor of new proceffes for watering hemp. This difcovery, which is interefting to agriculture, manufactures, commerce, and the marine, had engaged its attention; orders were given to make the experiments requifite to afcertain its value.

Every thing which could elucidate the principles and practice of M. Bralle's proceffes, which could prove and infure their fuccefs, was put in practice. Numerous and varied trials were made in the prefence of M. M. Monge and Berthollet, fenators, and Teiffier, member of the Inftitute. M. Molard, adminiftrator of the confervatory of arts and manufactures, directed thefe trials, and carefully purfued them for fix months. The refults were equal to the expectations that had been formed.

From the account rendered to his imperial Majefty, it was judged that the knowledge of a more expeditious method of watering hemp, than thofe employed at prefent, which is practicable at all feafons, and is in no refpect injurious to health, by means of which a greater produce can be obtained from an equal quantity of the materials; and, which muft extend

* From Bibliotheque Phyfico Economique, Brumaire, An. XIII.

and

and multiply the culture of an extremely valuable plant, could not be too extenſively publiſhed. In conformity to this deſire, we ſhall briefly deſcribe M. Bralle's proceſſes, relate the experiments which have been made, and offer ſome obſervations on the utility and advantages which are promiſed by this new diſcovery.

§ I.

The Proceſſes of M. Bralle.

The means used by M. Bralle for watering hemp, are

M. Bralle's proceſs. The hemp is ſteeped in hot water with ſoap.

1ſt. Water is heated in a veſſel to the temperature of from 72° to 75° of Reaumur's thermometer; (200° fahr.)

2nd. A quantity of green ſoap (*ſavon verd*) is added proportional to the quantity of the hemp to be ſteeped.

3rd. The hemp is then immerſed ſo that it ſhall be covered by the fluid, after which the veſſel is cloſed, and the fire put out.

4th. The hemp is left in this ſtate of maceration for two hours, and then taken out.

Proportions of the articles.

The weight of ſoap required for a complete ſteeping, is to that of hemp-ſtalks as 1 to 48; and the weight of the hemp to that of the water as 48 to 650.

The proceſs may be repeated with other hemp in the ſame water.

Several ſteepings may be made one after the other. It is ſufficient, before each new immerſion, to add a quantity of ſoap water to replace what was abſorbed by the preceding, and to raiſe the temperature of the bath to the above degree. The ſame water may be thus employed for fifteen ſucceſſive days.

When the bundles are taken out of the ſteeping veſſel, they are covered with a layer of ſtraw, that they may cool gradually, without loſing their humidity.

Breaking the ſtalk,

Next day they are ſpread on a floor, puſhing the bands towards the top of the ſtems, and a roller of ſtone or wood, loaded with a weight, is paſſed ſeveral times over them, to cruſh them, and diſpoſe the tow to be eaſily ſeparated from the reed, which is effected by beating. Whether the hemp be wet or dry, it peels completely in either ſtate.

beating.

After having tied the handfulls of the tow peeled off while wet, at the top, they are ſpread on the graſs, turned, and, after five or ſix days, carried to the warehouſe.

The

The handfulls of ſteeped and cruſhed hemp which are intended to be beaten and ſtripped dry muſt alſo be expoſed on the graſs; this operation being abſolutely neceſſary to whiten the tow, and facilitate the ſeparation of the reed.

§ II.

Recapitulation of the Experiments.

The experiments were varied to aſcertain the requiſite

By means of a portable ſteeping veſſel, different quantities of hemp were ſteeped, the temperature of the ſoapy liquor was varied at pleaſure, and the ſtate of the hemp was obſerved during the courſe of each operation, of which the duration was more or leſs prolonged, in order to aſcertain;

Temperature,

1ſt. The temperature which the ſoapy liquor ought to have before the immerſion of the hemp;

time, and

2nd. The time neceſſary for a complete ſteeping, at a determinate temperature;

proportion of ſoap.

3rd. The quantity of ſoap abſolutely neceſſary for a given weight of hemp-ſtalks, weighed before the immerſion, &c.

Reſults or general obſervations on the proceſs.

From a great number of experiments made in the months of January, February, and March laſt, it was found,

1ſt. That water containing the quantity of green ſoap directed by M. Bralle, for a given weight of hemp, effects the ſteeping completely;

2nd. That the ſteeping is ſo much the more ſpeedy as the temperature of the fluid is nearer to ebullition, at the time of the immerſion of the hemp;

3rd. That if the hemp be kept more than two hours in the ſteeping veſſel, the time preſcribed by M. Bralle for obtaining a complete watering, the tow ſeparates equally well from the reed, but it acquires a deeper colour, and loſes part of its ſtrength;

4th. That if the hemp be immerſed in a cold ſoapy liquor, and heat be then applied, the ſteeping is not accompliſhed ſo perfectly, whatever degree of temperature may be given to the liquor, and however long the immerſion may be continued *;

5th. That the bundles of hemp immerſed and kept vertically in the veſſel, are ſteeped more uniformly than if they were laid horizontally; and this poſition alſo facilitates the manipulation.

* Probably the fluid between the fibres is not heated, becauſe its conducting power is bad, and it is prevented from circulating. N.

§ III.

§ III.

Obfervations on the Utility and Advantages of the New Difcovery.

Two methods only of fteeping hemp are generally practifed. The firft confifts in fpreading the plant on the grafs, and turning it two or three times a week, until the air, the light, the dews, or the rains, have difpofed the tow to feparate eafily from the reed. The refult is obtained in a longer or fhorter time, according to the weather and the ftate of the air; and frequently, in certain countries, the operation is not finifhed in lefs than forty days.

Defcription of the methods of fteeping hemp, as heretofore practifed. Expofure on the grafs.

The fecond confifts in immerfing the bundles of hemp in rivers, brooks, ditches, or pools, and keeping them there for eight, fifteen, twenty, or even thirty days, according to the degree of the heat of the water, or of the atmofphere.

2. Steeping.

The maceration effected by both thefe proceffes is frequently incomplete, and always unequal. By following the firft, the cultivator is liable to have his crop difperfed by the winds, or injured by long rains: if he adopts the fecond, he runs the rifk of lofing a part of it by the overflowing of the rivers, or of its being covered with mud. The firft method in particular, is liable to the ferious inconvenience of depriving the national marine of part of the hemp produced by our territory: it is known that the tow produced from the hemp which has been expofed on the grafs is not ufed by the government.

Thefe proceffes very defective.

The fteeping of hemp according to M. Bralle's procefs, requires only a copper cylindrical veffel, placed on a fmall furnace of bricks.

Superiority of the new procefs.

A fteeping veffel of this kind, containing 240 litres of water, (52 ale gallons) is fufficient to fteep 18 kilogrammes of hemp-ftalks, (about 40lb.) and as the operation is completed in two hours, 100 kilogrammes (221lb.) may be eafily fteeped in a day.

This method appears to deferve the preference over the former ones, on many accounts.

1ft. The fteeping is practicable all the year, except during very hard frofts, when it is difficult to dry the hemp. But when it is to be peeled wet, the cold is no longer an obftacle; it is then only neceffary to take proper precautions for preventing the tow from freezing in its humid ftate.

It is practicable all the year.

2nd. The

faves time,

2nd. The time of fteeping being only two hours, affords a faving of time of great value to the cultivator, particularly during the feafon of harveft.

and is not injurious to health.

3rd. The workman has no caufe to fear any injury of his health: it is fufficient to keep up a current of air while the bundles are plunged into and taken out of the fteeping veffel; the handfulls of ftalks or tow, which are afterwards expofed on the grafs, do not emit any bad fmell, or vitiate the purity of the air, whatever may be the quantity of hemp dried at once in the fame place.

Every one knows, that when the bundles of hemp fteeped in water in the old method, are taken out and wafhed, they emit an infectious odour which becomes infupportable during the heats, and to which ferious diforders are afcribed. The valley of the department of the Somme, and many others in which hemp is fteeped, afford too convincing proofs. The waters are rendered unfit for the ufe of cattle, and the fifh contained in them are frequently deftroyed.

Apparatus on a larger fcale.

To accelerate the operation of fteeping by the new procefs, in countries where there is an extenfive culture of it, inftead of the portable fteeping veffel which was made ufe of in the experiments, the following apparatus may be adopted, confifting of a boiler and four wooden tubs, ferving for fteeping veffels.

After having heated the foapy matter to ebullition, it is fuffered to flow through a cock, into two of thefe tubs filled with bundles of hemp, and clofed by a cover: while the fteeping is going on in the two firft tubs, the neceffary quantity of liquor is heated, to be conveyed into the other two, which are alfo filled with bundles of hemp, and clofed with lids.

By means of this very fimple apparatus, a confiderable quantity of hemp may be fteeped in a day without interruption.

Comparative expence of the two proceffes.

4th. The expence of fteeping in water, compared with that required by the method of M. Bralle, is nearly the fame, when the fmall fteeping veffel is made ufe of; but if a cauldron, rather large, and the fteeping tubs which have been mentioned are employed, the coft will be diminifhed more than a half.

In fact, the expence of the firft includes the conveyance of the hemp to be fteeped, the time employed in forming the bundles of hemp into a fort of rafts, that they may be funk by by loading them with ftones, turf, clods of earth, and even mud; in fixing and fecuring thefe rafts by driving in ftakes; a tedious work, and the more troublefome, becaufe 10 kilogrammes of hemp-ftalks cannot be immerfed without a weight of 15 or 20 kilogrammes, and, after the fteeping, all this mafs muft be removed, to take the bundles out of the water, and wafh them.

The coft of the new procefs confifts principally in the price of the folvent made ufe of, which amounts to about eight centimes for a kilogramme of tow. To this fhould be added the price of combuftible neceffary for heating the liquor, if this combuftible was not afforded by the reeds of the bundles, whether they are peeled wet or dry.

At an equal expence, the new procefs is ftill preferable to the old, becaufe, from what has been faid, it renders the manipulation more expeditious and more eafy.

5th. Eight kilogrammes of hemp-ftalks fteeped by the new procefs, commonly produce two kilogrammes of pure tow, by peeling when wet; whereas hemp fteeped in water by the old procefs, and beaten, does not yield more from eight kilogrammes than one and a half. Comparative produce.

The dry peeling of hemp fteeped in the old way does not produce the fame quantity as that which is peeled when wet: the breaking of the reed in many places occafions a greater lofs of tow.

The hemp being wafhed, beaten and combed in the old method, a kilogramme of long tow is obtained from four kilogrammes of the rough tow; the remainder is fhort ftuff, hards and duft.

The fame quantity of hemp, manipulated in the new way, yields two kilogrammes of long tow, one kilogramme of fecond tow, and about a kilogramme of fhort ftuff and hards.

Thus from eight kilogrammes of hemp ftalks, two kilogrammes are obtained in rough tow by the new procefs, and from this quantity is obtained one killogramme of the firft tow, which does not exift in any known manipulation.

6th. The inhabitants of the banks of rivers and of the valleys, are almoft the only perfons who cultivate hemp: they owe this privilege to the vicinity of the waters, and the humidity of the foil. By the new procefs the culture of hemp will be extended to all places, and procure a new and very Extenfion of the culture of hemp.

very advantageous occupation to the inhabitants of the plains, the land of which is much more vegetative than that of the marshes.

It is an error to suppose that hemp cannot grow to a great height in the plains; it is a fact, that it rises to the height of two yards, in land which has been well ploughed and manured, when mild rains have promoted germinanation and growth.

It is equally a fact, that there is every where a sufficient quantity of spring or cistern water to steep the hemp by the new process: if droughts should supervene, which besides are only accidental, the steeping may be deferred.

It will, therefore, be possible to cultivate hemp in the plains, and in low lands, which are always rich and fertile, though frequently without springs of water, and to augment not only the mass of our products, but also our riches of this description, since one acre of good hemp yields as much profit as two acres of wheat.

Summary of the advantages arising from this process.

Such are the effects which may be expected from M. Bralle's new method of steeping hemp. It is, as was observed at the commencement of this instruction, more expeditious than those hitherto employed; it perfectly completes the steeping; it may be used at all seasons; it does not affect the purity of the air; from an equal quantity of materials, it procures a more abundant produce; and lastly, it is well calculated to extend the cultivation of the plant itself. The enlightened lovers of agriculture, and well-informed proprietors, who live upon and cultivate their own estates, without being slaves to the customary practices, will adopt it, and secure its advantages, by repeating the experiments which have ascertained its merit, and also by making trials on a more extensive scale than those which took place in the conservatory of arts and manufactures. Their example will be followed, the process of M. Bralle will be extended, and we shall see portable steeping vessels, similar to those used by M. Molard, multiplied; a cheap apparatus which requires very little repairs, and by means of which the hemp grown through the extent of one or of several communes may be steeped even in the field on which it grew.

V:

V.

Defcription of a Portable Steam Engine. By Mr. MATTHEW MURRAY.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of handing you the defcription of a portable fteam engine of my conftruction, which you will have the goodnefs to infert in your Philofophical Journal. I will juft obferve it is reduced to the feweft parts that practical utility will admit, which muft neceffarily render it of great advantage; as the fimplicity of its parts make it nearly impoffible to be out of order with a very moderate degree of management. The following defcription and reference to the plate will explain the nature of this engine.

I am, Sir,

Your much obliged humble fervant,

MATTHEW MURRAY.

Leeds, May 7th, 1805.

Defcription of a Portable Steam Engine.——Plate VII.

Defcription of a portable fteam engine.

A A Reprefents the ground or floor on which the engine ftands.

B Section of a recefs made in the ground for the beam O to work in.

C. Iron ciftern refting upon the ground or floor covering the recefs for the beam.

D An opening in the floor to admit a boy to oil the centers of the beam.

E A double fteam cylinder, having an upright pipe in the intermediate fpace, which effects a communication between the top and bottom and the valve box G.

F A fteam pipe that communicates with the boiler through which all the fteam paffes and furrounds the inner cylinder in its way to the valve box, prior to its application againft the pifton.

G The valve box fixed upon a projection from the cylinder bottom, having an opening or connection with the interval between the two cylinders. In this opening is a regulating valve

Defcription of a portable fteam engine.

valve for adjufting the quantity of fteam (that acts againft the pifton) in its paffage through the valve box. There are alfo three other openings in the bottom of this valve box, one of which connects with the top of the cylinder by the pipe in the intermediate fpace, the fecond with the bottom, and the third with the eduction pipe that leads to the condenfor. Two of thefe openings are alternately connected together by a flide valve,* while the third is left open for the admiffion of fteam to the pifton, this valve changes its pofition at the end of each ftroke of the pifton, and performs all the purpofes of the moft complicated machine.

H The air-pump connected with a condenfer at the bottom of the eduction pipe.

I. The fly wheel fixed upon an axis which receives its motion from a crank connected with the beam by the rod K.

L L Two rods for connecting the motion of the pifton to the beam, thefe rods move perpendicularly by a motion which could not be conveniently fhewn in this view without rendering it confufed.

M A fpherical triangle turn'd by the crank for moving the flide valve by the horizontal rod N that connects them together. This motion has the advantage of preventing the engine from ever turning the contrary way round from that which it is wanted to go, and prevents the noife that is ufually heard in engines.

O The beam attached to the bottom of the ciftern C by means of the hanging carriages P.

Q A reft or fixture in a wall for the end of the fly wheel fhaft; this will vary according to the fituation where the engine is to be fixed, or it may be fupported by a metal ftandard.

R. Index to the injection cock that admits water to the condenfer. *Note*, The ciftern is to be kept nearly full of water during the time the engine is at work.

The cylinder G and valve box E muft be furrounded on all fides by a cafe (not fhewn in this view) the fpace between filled with charcoal to prevent the tranfmiffion of heat, which if effectually done will work with the leaft poffible quantity of coals, as it combines the advantages of every other engine hitherto known. By detaching the air-pump and condenfer

* For thefe valves I took out a patent in 1802.

(which

(which may be done in half an hour) and where water cannot be had for condenfation, this engine may be worked by the preffure of ftrong fteam alone, as the internal cylinder is kept as hot as the fteam in the boiler.* This dangerous plan never ought to be reforted to but in cafes of neceffity, as it is no faving of coals, and as there can be no certain rule when to difcontinue the ufe of the boiler, the weaknefs of which is not prevented by putting the fire in a tube in the infide of it. This engine requires no framing nor mill-wright work in the fixing, but merely bolting down to the floor it ftands upon. It takes up very little room, and all its parts are within reach, without the neceffity of upper floors or ftages, which would be the cafe if the beam was above; but by being fixed below and alone, it has no tendency to move from its fituation.

Defcription of a portable fteam engine.

VI.

Letter from Mr. J. C. HORNBLOWER, *Engineer, on the Meafure of Force by Horfe Powers.*

To Mr. NICHOLSON.

DEAR SIR,

I AM induced to trouble you on account of the prefent unfettled ftate of things refpecting what is ufually called *the power of a horfe*. I do not know why a matter of this fort fhould remain fo difregarded, efpecially as it has fo long become one of our data, comprehending the unities of weight, fpace and time, by which we are to be underftood in our communications on the fubject, and by which we are to afcertain the pre-

Uncertainty of what is meant by the power of a horfe.

* Many engines are at prefent worked in London and elfewhere by the mere force of fteam, without condenfation, under Trevithick's patent. The force is from 45 to 60 lb. on the round inch; a preffure equal to about 25 fathoms of water at the moft. Various affertions and reports concerning the fafety, the economy, and the other effects of thefe engines have paffed under my notice; but the interefted fituation of fome of the narrators on both fides, and the fhort time of trial, have induced me to wait for more facts before I fhould give any account of the engine in this Journal. I hope to do this a few months hence.—W. N.

cife

ciſe value or effect of any mill or engine in and about London. Indeed I do not know why it was adopted for the purpoſes intended, it being ſo indefinite.

This unity probably aroſe from ſteam engines being ſubſtituted for horſes.

I can eaſily conceive it probable that ſomebody who has employed horſes for ſome time in mill-work, may have applied to an engineer, and ſaid "I have a mind to have my work done by a ſteam engine inſtead of horſes, for I am to a point that I ſhall ſave money by it, and pleaſe to give me an eſtimate of the coſt of an engine that will do the work of my horſes;" and then the engineer ſets about getting information as to what may be deemed the effect a horſe can produce, and calls it the *horſe power;* and perhaps having Deſagulier's Experimental Philoſophy at hand, applies to him, and there he finds that a horſe will raiſe a hogſhead of water 50 feet high in a minute; then what is the weight of a hogſhead of water, and he finds from ſome particulars related by him, 2nd vol. page 505, that a hogſhead of water is equal to 550 lb. but of what meaſure is uncertain, for the ale hogſhead, 51 gallons, is 540 lb. and the wine hogſhead, 63 gallons, is 504 lb. reckoning the cubic foot at 1000 ounces avoirdupois.

Deſaguliers conſiders it as 550 lb. raiſed 50 feet per minute.

Another eſtimate; nearly double that of Deſaguliers.

Some engineers who have very unceremoniouſly taken the lead in this affair, have adopted I do not know what for a *datum,* but the reſult is this: An engine by calculating 10 lb. on the ſquare inch, making the whole preſſure = 1000, moving through 200 feet per minute, is called a *four-horſe engine.*— Let us ſee then what will be the effect of one horſe according to this fact.

$1000^{\text{lb.}} \times 200^{\text{ft.}} = 200000 =$ the whole effect, then $\frac{200000}{4}$ = the effect of one horſe. Now compare this with the eſtimate of Dr. Deſaguliers, which is a hogſhead of water at 550 lb. 50 feet high in a minute; 550 lb. × 50 ft. = 27500 and; 50000 − 27500 = 22500 = the difference between one of Mr. Watt's horſes and one of the doctor's, on the former of which I make no comment.

Smeaton's eſtimate one-fifth leſs than Deſaguliers.

Mr. Smeaton, whom I hold as having ſuperior claim to precedency on ſubjects of this nature, has utterly diſapproved of Deſagulier's experiment by the moſt powerful conviction of its fallibility, formed by concluſions drawn from ſterling experience in the accompliſhment of works on a large ſcale; and he ſtates the greateſt effect to be 40 feet high in a minute; but

as

as this is ftill in the commonly received opinion as to the weight of the hogfhead, I would rather turn to thofe who have made their experiments on a weight of folid matter, expreffed in terms which cannot be miftaken.

I remember to have had fome converfation on this fubject many years ago, with the late Samuel More, at that time fecretary to the Society for the Encouragement of Arts, &c. when he fhewed an inftrument conftructed on purpofe to determine the refiftance againft horfes at plough. I do not recollect that I made any minutes on the refult of our converfation, fo can only fay that his relation of the fact furprifed me, until I came to compare it with the effect of horfes actually applied to overcome a load drawn up a fhaft in a mine; but I had not the fame means of determining the re-action that he had; however, the refult of his experiment may be feen in the Tranfactions of that Society; fome obfervations on it may be feen in the 3d vol. quarto, of the Philofophical Journal, page 136, only there feems to be a miftake in the deduction in the note: it fhould be 264 : 10 : : 1375 : 52 +

Mention of experiments on the reaction againft horfes, by the late Sam. More, Efq with an inftrument. *It was a fpring with graduation.*

I much wifh to have an experiment like Mr. More's made by a fledge drawn forwards and backwards on a level road, with Mr. More's inftrument placed between it and the horfes; fuch an experiment would be very practicable, and the fmall deviation from the true level of a road would be compenfated by alternately going firft one way and then the other.

Propofed experiment.

It is true that we are become pretty well acquainted with what may be done by horfes in grinding malt, pumping liquor and worts in breweries; but there are fo many fortuitous circumftances to be regarded, even here, that nothing decided can refult from the clofeft inveftigation. For inftance, fome brewers chufe to have their malt ground much lower than others; the pump-work is executed in fome breweries under very different advantages, and from local circumftances may be retarded by the inertia neceffary to communicate motion from the wheel to the work, adding the different condition of valves, buckets, &c. All thefe confiderations demand fome *invariable* refiftance to be overcome by the exertion of the horfe, and I know of nothing fo appropriate as that I have juft mentioned.

Uncertainty of the meafure of a horfe power from grinding or pumping, &c.

The power of a horfe (by which I mean the mechanic power) is not eafily afcertained. It has fcarce any analogy with

Difficulty of this fubject, not only from the work, but the claſs of horſes employed.

with a weight deſcending through a given ſpace or a quantity of water falling a given height, and therefore is better expreſſed by the terms effect, reſiſtance, re-action, &c. and even then, to be any thing like preciſe, we ought to diſcriminate whether brewers' horſes, or higlers' horſes, waggon horſes or coach horſes, heavy horſes or light, and if you will go into the country among the coal-mines, you will have another claſs of theſe animals, which I know not what to call unleſs it be *poor horſes*, full worked and half ſtarved; in ſhort, I mean that neither one or the other ought to be taken into the account as the *meaſure of a mechanic power*.

Mr. More's eſtimate of 80lb. 3 miles an hour. This is nearly three fourths of Deſagulier's rate.

Nevertheleſs it ſeems deſirable to have ſome popular expreſſion for the application of whatever may be ſubſtituted in the place of horſes, whether ſteam, water or wind; nor can there be any objection to ſaying, "equal to the work of ſo many horſes," provided we can attain to a clear, unequivocal and ſomewhat exact value, attributable to that power, and if I may give my own opinion, I think Mr. More has ſtated the utmoſt effect to be 80lb. three miles per hour, in ſuch horſes as are proper for giving motion to mill-work, and at ſuch ſpells as will not exhauſt the breath or ſtrength of the animal.

Remarks.

I am ſurpriſed to find this mode of calculation has obtained ſo far as to determine the power engines employed purely as pumping engines, as lately at the Tunnel, the New Docks, &c. but I am glad it reaches no further than the bills of mortality, and I hear that the Dutch method of hoiſting goods to warehouſes has lately been adopted at ſome of our new docks. *O tempora, O mores!* While other countries are availing themſelves of the application of the ſteam engine in place of animal labour, we are taking up the expedients of thoſe who have ſcarce heard there is any ſuch thing as a ſteam engine, or who cannot appreciate its value on that degree of evidence we have in our own country.

Whether the horſe power or unity be true or not, it ought ſurely to be free from ambiguity.

It may be objected to by ſome to alter the preſent data, however erroneous, as we ſhall be obliged to require 20 horſe engines inſtead of 10 (for it appears the eſtimate is nearly, if not quite *cent. per cent.* more than it ſhould be); but even that can make no difference in any reſpect than as making a rent in an egregious error; the coſt of an engine cannot be altered by it, nor the conſumption of fuel, but a material convenience would be the reſult of ſuch a regulation, conſidering the ad-

 vantage

vantage of a coincidence in this point throughout the kingdom, and as partaking of the nature of a unity of weights and meafures, it ought to be paramount to every fubordinate confideration.*

Your much obliged obedient fervant,

J. C. HORNBLOWER.

VII.

Letter from Mr. A. F. THOELDEN, *communicating three manufcript Tables from Mr.* BODE, *of Berlin, of the geocentric Places of the new Planets Ceres, Pallas, and Juno, for twelve Months to come.*

To Mr. NICHOLSON.

SIR,

THE three planets, or afteroids (according to Dr. Herfchel), lately difcovered, being fo very fmall, are not eafily found, unlefs the obferver is acquainted with the place where he is to look for them. This uncertainty induced fome aftronomical gentlemen to defire me to inquire, if there were not any ephemeris of their motions publifhed abroad. I complied with their requeft, and Mr. Bode has very obligingly communicated to me the following written account of their refpective fituations, calculated for the Obfervatory at Berlin, (that of Juno, according to a table of his own calculation). If you think this communication may be interefting to the aftronomical readers of your Philofophical Journal, I beg you will make ufe of it.

Tables obtained from Mr. Bode of the places of the new planets.

I am, Sir,

Your moft obedient humble fervant,

A. F. THOELDEN.

10, *St. Alban's Street,*
May 13, 1805.

P. S. A new edition of Mr. Bode's fmall Celeftial Atlas has juft been publifhed; with a Catalogue of 5500 Stars, after Piazzi's obfervations. This laft work can be had feparate.

Any gentleman who may be defirous of one or both thefe works, will be fupplied in a reafonable time after fending an order to me.

* For a very clear and rational report of a fteam-engine in horfe-powers, fee our Journal, IX. p. 215.—W. N.

TABLE I.

GEOCENTRIC MOTION OF CERES.

1805.	A. R.		Decl.		1806.	A. R.		Decl.	
Oct. 2.	104°	34′	22°	51′N.	Jan. 9.	103°	26′	29°	46′N.
11.	106	53	23	3	18.	101	11	30	25
20.	108	49	23	18	27.	99	18	30	53
29.	110	21	23	39	Feb. 5.	97	56	31	11
Nov. 7.	111	24	24	5	14.	97	13	31	22
16.	111	54	24	40	23.	97	10	31	26
25.	111	49	25	22	Mar. 4.	97	46	31	25
Dec. 4.	111	7	26	12	13.	98	58	31	20
13.	109	48	27	7	22.	100	43	31	11
22.	107	59	28	3	31.	102	57	30	58
31.	105	46	28	58	Apr. 9.	105	32	30	40
					18.	108	28	30	18
					27.	111	40	29	51
					May 6.	115	6	29	19
					15.	118	42	28	40
					24.	122	27	27	56

TABLE

TABLE II.

GEOCENTRIC MOTION OF PALLAS.

1805.	A. R.	Decl.	1806.	A. R.	Decl.
Aug. 3.	59° 29′	3° 4′ S.	Jan. 3.	68° 34′	31° 16′ S.
12.	62 47	4 22	12.	67 51	29 27
21.	65 54	5 56	21.	67 48	27 12
30.	68 52	7 47	30.	68 24	24 39
Sep. 8.	71 34	9 53	Feb. 8.	69 37	21 54
17.	73 58	12 15	17.	71 26	19 3
26.	75 59	14 50	26.	73 45	16 10
Oct. 5.	77 35	17 37	Mar. 7.	76 33	13 20
14.	78 40	20 30	16.	79 44	10 35
23.	79 12	23 23	25.	83 16	7 59
Nov. 1.	79 8	26 8	Apr. 3.	87 6	5 34
10.	78 26	28 38	12.	91 11	3 21
19.	77 11	30 43	21.	95 28	1 22
28.	75 29	32 13	30.	99 57	0 19 N.
Dec. 7.	73 33	33 3			
16.	71 35	33 9			
25.	69 51	32 32			

TABLE III.

GEOCENTRIC MOTION OF JUNO.

1805.	Longitude.	Latitude.	1806.	Longitude.	Latitude.
Oct. 1.	4s 28° 22′	6° 21′ S.	Jan. 1.	5s 26° 39′	4° 18′ S.
11.	5 2 28	6 12	11.	5 27 25	3 56
21.	5 6 18	6 1	21.	5 27 29	3 31
Nov. 1.	5 10 23	5 47	Feb. 1.	5 26 45	3 0
11.	5 13 54	5 33	11.	5 25 19	2 22
21.	5 16 44	5 21	21.	5 23 20	1 44
Dec. 1.	5 19 23	5 8	Mar. 1.	5 21 33	1 15
11.	5 21 34	4 54	11.	5 19 0	0 33
21.	5 23 27	4 38	21.	5 16 41	0 10N.
			Apr. 1.	5 14 27	0 50
			11.	5 12 56	1 26
			21.	5 12 5	1 57
			May 1.	5 11 46	2 19

VIII.

Letter from Professor Pini, *Inspector of Mines to the Italian Republic, to* J. C. Delametherie, *on Corindon found in Italy.**

THE interest you take in publishing discoveries in natural history in your excellent Journal, induces me to communicate to you a mineralogical rarity lately found on a mountain of the Italian republic: it is a very fine corindon, or adamantine spar, of a deep ruby colour. I saw it for the first time among the minerals which the learned Brochi, professor of natural history at Brescia, had made a short time before in the department of Serio. At the first view he considered it as a feldspar, of which it has all the appearance; and, in fact, the corindon being a substance which hitherto has only been furnished by countries far distant from us, it would have been imprudent to have judged otherwise at first: but the colour of stone, exactly resembling that of a red corindon which I brought from Paris, given to me as coming from Madras, led me to suppose that it did not differ from it.

Beautiful red adamantine spar found in Italy.

But as Professor Brochi purposed meeting me in a short time at Milan, to which place I was going, we postponed the verification of this suspicion. When he saw the red corindon from Madras, in my possession, he no longer doubted the identity of its species with that of our sample. I afterwards discovered the same identity in the trials to which I submitted it: the following are the results:

1st. The corindon of Italy scratches the hardest rock-crystal. 2d. It does not melt before the blow-pipe, either alone or with the addition of borax. 3. Its texture is in laminæ, which follow different directions. 4th. Its fissure is triple, and when it is cut in the three directions, it offers a rhomboid, the acute angle of which is 64⅓°. 5th. Its cross fracture shews the splendor of the diamond, and reflects the light, the flashes of which are almost the colour of silver, 6th. Its specific gravity is 3.87, which is the mean of that of the true corindon.

Examination. 1. Hardness. 2. Infusibility. 3. Laminar texture. 4. Fissure and angle. 5. Reflection of light. 6. Sp. gravity.

Hitherto it has been met with in a mountain of micaceous schistus, in pieces of several inches in length, which are amor-

Is found on a mountain of micaceous schistus.

* From Journal de Physique, Vendemiaire, An XIII.

phous

phous and opake, but ſemi-tranſparent on the thin edges. Profeſſor Brochi and I purpoſe making new reſearches there, which may lead to ſome more intereſting diſcovery.

Countries where corindon is found.

In the mean time it will no longer be doubted, that the corindon is a product of Europe. M. de Bournon, with whoſe memoir on corindon, inſerted in the Journal des Mines, Vol. XIV. you are well acquainted, has noticed the different countries which furniſh this ſubſtance; they are the iſland of Ceylon, the peninſula of India, and in particular Madras, the Carnatic, and China. He concludes his intereſting details by enquiring whether this ſubſtance exiſts in other countries, except thoſe acknowledged to be the chief, if not the excluſive ſituations of this ſpecies. This queſtion ariſes from ſeveral ſtones found in Europe having been given as corindons. In fact, thoſe collected in Germany were found to be ſometimes feldſpars, and ſometimes the *ſchorlartiger-beryll* of Werner, your leucolite. That mentioned in the Muſeum Brittanicum as coming from Tyrie, on the eaſtern coaſt of Scotland, was far from having the hardneſs belonging to this ſpecies; that from Cheſnut-hill near Philadelphia, announced by Mr. Smith, was diſcovered by Mr. Richard Phillips to be a fragment of badly cryſtallized quartz. It only remained to decide on the feldſpar found by Bournon in France, in the province of Forez, the deſcription of which he ſent to you in a letter inſerted in the Journal de Phyſique for June 1789, and which he ſtill conſiders as a true corindon.

The ſpecimens of European ſtones called corindon do not belong to that ſpecies.

This ſubſtance appears to be the ſame as that you have called andalouſite, and which ſome dealers in natural hiſtory have circulated in commerce by the name of adamantine ſpar, from the kingdom of Caſtile: it has been placed by Abbé Hauy in the appendix, which contains thoſe ſubſtances, the nature of which did not appear to be ſufficiently known to permit him to aſſign them a place in his method. He calls it apyrous feldſpar. Thus we may be ſatisfied that hitherto there is no certainty of corindon having been found in Europe. I flatter myſelf that now there will be no doubt on the corindon of Italy which I have the honour to announce; it is not even deficient in the ſpecific gravity belonging to this ſubſtance; a defect which induced Profeſſor Hauy not to acknowledge the feldſpar of Forez to be a corindon.

If

If rubies and ſapphires be claſſed with corindon, as many mineralogiſts ſeem diſpoſed to do, Europe will probably have mines of theſe precious ſtones as it already has mines of emeralds, ſuch as thoſe of Limoges, diſcovered by the learned Lelievre, counſellor of the mines. But theſe ſtones will be rubies, ſapphires, and emeralds of mineralogiſts, but not of the jewellers, until they ſhall be found very tranſparent, which I hope will be the fruit of new reſearches, followed with perſeverance. Probability that Europe may afford the precious gems.

HERMENEGILDE PINI.

IX.

On diſcloſing the Proceſs of Manufactories. In a Letter from Mr. JOHN CLENNELL.

To Mr. NICHOLSON.

MY DEAR SIR, *Newcaſtle*, *Feb.* 17, 1805.

PERMIT me to intreat the attention of ſome of your numerous correſpondents towards a queſtion which muſt certainly be intereſting to every manufacturer, but of which no regular diſcuſſion has yet been offered—Is it proper or improper to lay before the public in reſpectable periodical works, a full and impartial ſtatement of the various proceſſes of our manufactories? I ſhall ſtate ſuch reaſons as have offered themſelves to me why they ſhould be diſplayed, but I am principally anxious to receive further information on a ſubject that appears to me peculiarly intereſting. Intereſting queſtion reſpecting the diſcloſure of manufacturing proceſſes.

The firſt argument I ſhall adduce is that of Mr. Boyle, as quoted by Dr. Johnſon in the 201ſt number of the Rambler. "The excellency of manufactures, and the facility of labour, would be much promoted, if the various expedients and contrivances which lie concealed in private hands, were, by reciprocal communications, made generally known; for there are few operations that are not performed by one or another with ſome peculiar advantages, which, though *ſingly* of little importance, would, by *conjunction and concurrence,* open new inlets to knowledge, and give new powers to diligence." Argument of *Boyle,*

The

confirmed by experience.

The fecond is the very confiderable improvements that have taken place in thofe few manufactories which have yet been under the influence of chemical enquiry; thus realizing, but on a very extenfive fcale, the fuggeftions of Mr. Boyle: fo far therefore as we are to be guided on the one hand by experience, and on the other by the influence of fcientific enquiry on liberal difplay, will the argument be in our favour.

Accidental difcoveries improved by difclofure.

In the third place I would obferve, that as many very valuable difcoveries are owing to accident, thofe with whom they happen are frequently perhaps incapable of improving them to the extent they would admit of in the hands of men of fcience, and thus, by a fpirit of monopoly, preclude even themfelves from the advantageous cultivation of fuch difcoveries, merely left others might enjoy it alfo. If, again, we confider the rapid progrefs that has been made of late years in every department of ufeful and practical knowledge, we muft attribute it entirely to thofe liberal communications that have been made by men whofe attention has been immediately directed to the promotion and improvement of every thing valuable to the public. Again, the profits of every bufinefs depend on the regularity and knowledge with which it is conducted; but how is the laft to be enjoyed without refources to apply to, and how much more eafily would it be obtained if fcience could regulate and fimplify the combinations of the manufacturer? If to accomplifh by every thing employed (and even in many cafes the refufe) in each procefs its utmoft poffible ufe, is a favourite principle with manufacturers; to take the moft accurate and beft adapted means to effect it, ought certainly to be as powerful with them. Is it not alfo obvious, that, to difcard all myftery and quackery, and fairly to difclofe each procefs, is to invite the attention of men of fcience and refearch to extend any advantages gained by chance or otherwife, and to difcover greater utility in the various fubftances employed. The origin, progrefs, prefent ftate, and hints for the improvement of the "arts of life," would certainly be worthy the contemplation of our firft chemifts, and are fubjects that have appeared of fuch importance to a neighbouring nation, that many of their moft eminent men have been employed in them; and fome volumes of the *Encyclopedie Methodique* are dedicated to fuch information, with plates too, in feveral inftances difplaying even the moft minute work-tools employed in each.

Science would be thus introduced into workfhops, &c.

The

The hiſtory and detail of manufactories conducted in each place ought, I preſume, to form a principal object with the writers of local hiſtories; yet very few of thoſe gentlemen are enabled to obtain ſuch accounts as they can depend on, from the ſelfiſh and monopolizing jealouſy of manufacturers in general. To theſe various advantages an objection may be offered, "That diſplay is placing objects of taxation in the hands of miniſters: be it ſo; diſplay will make it eaſier to collect the tax, will make it more certain, and it may be, *leſs oppreſſive:* if to theſe be added the above advantages, it may fairly be preſumed, that diſcovery and conſequent improvement is the moſt advantageous track to be purſued; but, my dear Sir, I beg your pardon, on this ſubject I did not mean to offer my own opinion ſo much as to ſolicit information from that of others. Some objections anſwered.

I am truly your's,

JOHN CLENNELL.

How far literary purſuits are compatible with the duties of the commercial man, or the manufacturer, ſeems a queſtion ſo completely decided in the affirmative, in the firſt volume of the Mancheſter Memoirs, by Mr. Henry; in the ſecond volume of the ſame work, by Dr. Barnes; and in the hundredth number of the Lounger,—that the above paper aſſumes the principle as being fully eſtabliſhed. J. C. Literary purſuits are compatible with the duties of merchants and manufacturers.

X.

Queſtion reſpecting the Purification of Copper. By J. P. *With a conciſe Reply.* W. N.

As copper in its pureſt ſtate (eſpecially out of London) for manufacturing different articles, cannot be obtained without a very tedious proceſs; as it forms the principal ingredient in mirrors for reflecting teleſcopes, and likewiſe is much uſed as an alloy for gold; if it is impure, it never fails to render the gold ſo alloyed brittle, and not to be reſtored to its ductility until the impure alloy be waſted by ſubſequent meltings, to the loſs and diſappointment of the workman. Required, therefore, Queſtion reſpecting the purification of copper.

therefore, a method of purifying the copper, particularly for the latter purpofe?

Your's refpectfully,

Morley Street, J. P. *Jun.*
Newcaftle-upon-Tyne.

Reply.

As the proceffes for refining copper in the large way are grounded upon its property of refifting oxidation more than the other metals which are ufually combined with it, it may be adviseable to adopt the procefs of Pelletier, with a due attention to the manipulation and the proportion of manganefe to be made ufe of. The very interefting letter of Mr. Thomfon in the prefent Number, will indicate the principles of operation. Mr. Hatchett's excellent papers in the Philofophical Tranfactions, of which a correct abridgement is given in our V. and VI. Volumes, fhew the mifchievous confequences of impurity in the copper for alloying the precious metals; and it is but too well known, that it is difficult to be procured, or even to be made pure, upon a fcale of extenfive magnitude.

W. N.

XI.

Defcription of a Method of connecting Iron Bars, and coating them with Lead, fo as to form folid Pillars for Light-houfes on Rocks covered at High-water, and to defend them from Corrofion. By Capt. JOSEPH BRODIE, *of the Royal Navy.**

Defcription of the means of coating iron bars: by reference to the drawing.

FIG. 1, *Plate V.* A fhews four rods of caft iron, compofed of a number of pieces two feet long, rivetted together, in a manner explained by the plate, fo as to produce the effect of one bar of the thicknefs of the whole. B. A tube of caft iron, formed from a number of feparate pieces, each about ten inches long, and which, when placed round the iron rods above-mentioned, and then fcrewed together, form a mould, into which the melted lead is to be poured, to coat the iron rods.

* Communicated to the Society of Arts (Memoirs, MDCCCIV. 258.) who voted him the gold medal.

rods. C. A portion of the rods covered with the melted lead, ſo as to form a cylindrical pillar apparently of lead, the iron being perfectly coated therewith.

Fig. 2. D ſhews the manner in which the hollow cylinder is formed to any length required, by the junction of a number of ſemi-cylinders rivetted together and fitting each other. E, the ſide flanges ſcrewed cloſe together. F, the end flanges alſo ſcrewed together, as prepared for the melted lead.

After a certain portion of the iron rods are coated with lead, the lower parts of the tube are taken off and placed higher up; by which repeated changes, a few tubes will anſwer the purpoſe to coat any length of the iron rods.

XII.

Reply to Mr. Accum's *laſt Letter on the Production of Nitrous Acid. By* W. F. S.*

To Mr. NICHOLSON.

SIR,

I THINK Mr. Accum by no means throws off the charge of cenſure with which he is accuſed, upon ſo material a part of the modern theory. He ought to have been more explicit, and in ſo nice and ſo diſputed an experiment, to have given us a minute detail. Remarks on Mr. Accum's letter.

I have performed the experiment which he ſpeaks of, but I could detect no nitrous acid after the proceſs. The air gradually diminiſhes by the electric ſpark, but this diminution is owing to the oxigen gas producing a calcination of the metals employed, for the pureſt oxigen gas anſwers better than when mixed with nitrogen. Therefore I hope, if Mr. Accum poſſeſſes a more accurate experiment, he will give it through your Journal: certainly the preſent ſtate of modern chemiſtry requires a very minute inveſtigation. I hope, Mr. Nicholſon, you will not refuſe inſerting this in your Journal. Unſucceſsful attempts to repeat the experiment of forming nitrous acid.

London, May 2.

* See our Vol. X. p. 109 and 214.

Experiments

XIII.

Experiments on the Electricity of Metallic Filings sifted through Metal; with Remarks in answer to a Letter of Mr. Cuthbertson. By Mr. Wm. Wilson. *(From the Author.)*

To Mr. NICHOLSON.

SIR.

An error pointed out by Mr. Cuthbertson admitted.

IN the last number of your Philosophical Journal, I find a letter from Mr. Cuthbertson, containing some remarks on my letter on the electricity of metals, in which he notices an error in the table of results of experiments, that had escaped my notice till I read his letter. In that table I have by mistake put P against copper filings sifted through zinc instead of N.

Copper filings when sifted through zinc are electrified strongly with negative electricity.

Mr. C.'s objection to the production of electricity by mere separation of metals, considered. The metallic filings *touch* the sieve, are *separated*, and *touch* the receiving metal. As the el. varies with the sieve it does not depend on the *latter contact*,

Mr. C. in his other remarks does not seem to admit that the separating the metals from contact is the cause of the electrical fluid being excited and not touching, because both touching and separating are employed. In the way the experiments were made touching takes place two ways, viz. the filings come in contact with the metal plate they are sifted into, and they are in contact with the sieve before they are sifted through it; but I cannot conceive how either of these contacts could cause the excitation of the electricity in the experiments. If the contact of the filings with the plate they were sifted into was the cause, we should have had the same effect with the same filings, whatever metal the sieve was made of they were sifted through; because as they were always sifted into the same plate, the same metal filings always came in contact with the same metal plate, and consequently we should have had the same effects in all cases with the same filings; whereas out of the ten sorts of filings that were used there was only one (steel) that produced the same effect with the different sieves; and if the contact of the filings with the sieve excited any electric fluid, it would be dissipated as fast as excited, because neither the sieve nor the filings were insulated; consequently it could have no effect on the results of the experiments.—Therefore since neither of these contacts excited the fluid, it must have been excited by the separation.

—and in the *first contact* there is no insulation,

—consequently it depends on the *separation*.

I think

I think the following experiments will put this beyond a doubt: 1. I faſtened a piece of card into a ſtick of glaſs, and then rubbed it over with ſtrong gum water and covered it with filings of zinc; ſo that when it was dry, it had a ſurface of filings of zinc. From a heap of the ſame filings I took up as much as I could on this little ſhovel without touching it with any thing elſe, and let them fall very ſlowly upon a piece of bright ſheet copper faſtened in an inclined poſition upon the cap of an electrometer, and formed into a receptacle at its lower part to contain the filings. In this operation almoſt every particle of filings neceſſarily came in contact with the face of the copper. After letting fall upon the copper about an ounce and a half of filings there was not the leaſt ſenſible effect on the electrometer. 2. I then took a piece of the ſame ſheet copper, which was pierced full of ſmall holes, and ſifted through it the ſame filings I had uſed in the above experiment, upon the ſame copper on the electrometer, and the gold leaves diverged with poſitive electricity and diſcharged themſelves againſt the ſlips of tin foil on the inſide of the glaſs ten times before the whole of the ounce and half of filings were ſifted into it.

An inſulated ſhovel of card, faced with zinc filings was uſed to pour zinc filings on a bright plate of copper.

The copper plate was not electrified.

A copper ſieve was then uſed inſtead of the ſhovel.

The copper plate was ſtrongly poſitive.

Now, ſince the ſeparating the two metals from contact in the ſecond of the above experiments is the only difference between them, and as the electric fluid was excited only in the ſecond, I think we may ſafely conclude that that ſeparation was the cauſe of the excitation, and not touching.

Separation was alone the cauſe of this electricity.

I am very much inclined to believe that the excitation that takes place in friction is cauſed by the ſame circumſtance, and that the friction does nothing more towards the excitation than bring the different parts of the ſubſtance that are rubbed together into contact, and ſeparate them from it.

Electric excitation is probably a fact of this kind.

I am your obedient humble ſervant,

WILLIAM WILSON.

Reply

XIV.

Reply to Mr. Boſwell. By AN OLD CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

Explanatory remarks on Mr. Boſwell's letter.

WHEN I began to read your correſpondent, Mr. Boſwell's anſwer to my obſervations on his geometrical propoſitions, and found myſelf accuſed of having "thrown ſome very undeſerved reflections" on his communication, I could not help feeling a degree of apprehenſion leſt I ſhould inadvertently have made ſome miſtake or other, for which I muſt have been obliged to apologize to him and to the public; but on peruſing the letter through, I was not a little ſurpriſed to have found but *one* reflection pointed out, and that applying not to the *matter* but *manner* of my obſervations; it ſeems I have accuſed him of being *too confident* in one of his aſſertions; a literary crime, of which he exculpates himſelf by proving from a quotation, certainly very much to the purpoſe if we make no diſtinction between *doubt* and *diffidence*, that he is, on the contrary, a very *diffident writer*, which quotation, to be ſure, contraſted with an expreſſion that fell from my pen, might be concluſive, if the two paſſages were applicable to the *ſame thing;* but unfortunately it turns out, on cloſer examination, that the accuſation applies *excluſively* to Mr. B's *ſecond* propoſition, and his exculpatory quotation *excluſively* to his *firſt.*

Had I committed myſelf ſo far as to ſay that he announced his firſt propoſition with confidence, I muſt have ſtood clearly convicted of having done him injuſtice; but as it was his ſecond propoſition, or, in other words, his *other fact* in geometry, to which my objectionable obſervations *ſolely* applied, he ought to have quoted what he has ſaid about *it*, and about *it only*, as evidence againſt me; he has, however, directed his arrow at a wrong mark, on which account I claim the reader's indulgence to repeat the paſſage alluded to, which is the *only* paſſage in Mr. B's paper that relates to his ſecond propoſition: "The diſcovery of a fact in geometry often leads to *another;* one of this kind *I have here to add, which is,* that a right line (BE) drawn from the extremity B of the line IB, at right angles through the oppoſite diameter (IF) to the circumference,

ence, *will be equal* to a fourth of the circumference." In this annunciation, which is an appendage to the main ſubject or firſt propoſition, and evidently not included in the prefatory apology, I ſee nothing like a ſuſpicion of inaccuracy expreſſed, or even hinted at, and conſequently can trace no mark of *diffidence*, now that I come to examine it again; on the contrary, I repeat, that a fact is confidently aſſerted to exiſt, which has been proved *not to be a fact*, both by myſelf and by Mr. Gough, to whoſe teſtimony probably the reader will pay ſome deference, particularly as this gentleman has ingeniouſly ſhown that *another line* in the circle poſſeſſes the identical property erroneouſly attributed to the line BE, and that previouſly to Mr. B.'s complaint being made public: to the reader, therefore, the falſeneſs or juſtice of my obſervation or "reflection" muſt be referred; and it will anſwer the purpoſe of both Mr. B. and myſelf, if he will let the affair drop here; for *he* will then ſtand a good chance of being reputed what is his principal aim, a *diffident writer*, and I ſhall ceaſe to be, what ſome of your readers, beſides Mr. B. may poſſibly be diſpoſed to think me, a *caviller*.

Explanatory remarks on Mr. Boſwell's letter.

I am, Sir, once more,

AN OLD CORRESPONDENT.

May 17, 1805.

P. S. The mathematical portion of your readers need not be informed, that the *forgetfulneſs* imputed to me, reſpecting the aſſumed *approximation* ſubſtituted for the *exact ratio* of the diameter to the circumference of a circle, is a charge applying with equal propriety to every mathematician who has deduced calculations depending on the area of a circle: even Mr. Gough, whoſe mathematical ſkill is juſtly the admiration of thouſands, whom he has never ſeen, and is doomed never to ſee, has ſomehow been *obliged* to be guilty of the ſame want of recollection, though his calculations may be conſidered as the reſult of *demonſtrative truth*.

It remains now for Mr. Boſwell to ſhow the ſcientific world, by an example or two, how he applies his diſcovery to practical meaſurements, which it is preſumed, will prove a communication of general intereſt.

XV.

Defcription of an accurate Method of banking the Balance of a Time-keeper. By Mr. WILLIAM HARDY. *Extracted from his Letter to* CHAS. TAYLOR, *Efq. Sec. to the Society of Arts.**

SIR,

Importance of the ftop or banking piece ufed to prevent extreme vibrations in time-pieces.

THIS letter is accompanied with a drawing, a defcription, and a model, of a more perfect mode of banking the balance of a time-keeper, than any that has yet appeared; and its application to a time-keeper is a matter of fuch real importance, that the moft accurate, without this moft neceffary appendage, is liable to fuch derangement, that from the moft trivial caufe it is in one moment rendered ufelefs.

The author's invention is confirmed by long trial,

To preferve the good qualities of the time-keeper, on which often the ftrength, the wealth, the grandeur, and fafety of this great empire depend, I deem it neceffary that my invention fhould be laid before the Society of Arts, as the means of its being more generally known; and I hope that I fhew proper refpect to the Society, when I affure you that I do not offer any crude idea, neither could I think of giving you any trouble, until I had fully verified the utility of my contrivance by feveral years trial. As I can produce the teftimony of fome of the moft eminent watchmakers in favour of my invention, I look forward with fome degree of confidence, in expectation of obtaining the approbation of the Society.

and teftimony.

The banking is required in watches which have a vibration through very great arcs.

It was at firft imagined, that a banking to a watch with a free efcapement was quite unneceffary, as the limits of banking were fo great as to admit of almoft twice 360, or 720 degrees; but, on trial, the balance was frequently found to exceed this quantity, and that a very flight motion given to the time-keeper (particularly when the axis of the balance became the axis of that motion), was fufficient to alter the ftrength and figure of the pendulum-fpring, and pofition of the pieces in refpect of the balance-wheel, fo as to change the rate of the time-keeper; and, what was worfe, require a new

* In their Memoirs for 1804. A premium of 30 guineas was awarded for this invention.

new adjuſtment of the balance, to accommodate itſelf to the changes made in the ſpring, and other parts connected with it. Hence it became neceſſary, that ſome means ſhould be uſed to ſtop the balance at certain limits beyond its natural arch of vibration; and various attempts have been made to effect it. One way is, by a moveable piece on the axis of the balance, which banks againſt a pin, yet ſo as to ſuffer the balance to vibrate more than 360 degrees. Another method is to have a piece moveable on a centre in one of the arms of the balance, and applying itſelf as a tangent to the pendulum-ſpring, which paſſes through a hole in the piece. It has alſo a knee, which almoſt touches the plate, and juſt paſſes free of a pin placed in it. But when the balance vibrates ſo as to approach its utmoſt limits, the action of the ſpring, while in a ſtate of unwinding, throws the piece outward, ſo as to fall in the way of the pin, and ſtop the balance from proceeding farther. Another mode is by a ſtraight ſpring, ſcrewed upon the plate, having a hook at the end of it, into which a pin placed in the balance ſtrikes, when, as before, the pendulum-ſpring, in unwinding, touches the ſtraight ſpring, and moves it a little outwards. There is alſo a way of banking by means of a bolt, which is thrown back by the pendulum-ſpring, and made to fall in the way of a pin placed in the rim of the balance. Theſe are the principal modes of banking now in uſe, and they do not differ materially from one another in principle. But the weight and friction of ſo many pieces, on ſo delicate an organ as that of a pendulum-ſpring, are perhaps nearly as hurtful to the time-keeper as the injury it may ſuſtain when it is left without any banking whatever.

Former methods of banking.

They are objected to from weight and friction.

In *Figures* 1 and 2, *Plate VI.* the ſame letters are placed, to ſignify the ſame things. A A is the balance to which the pendulum-ſpring is faſtened in the uſual way. In one of the croſſes of the balance is placed a pin P, which ſtands a little way above its ſurface; and when the balance is cauſed to vibrate a complete circle, the pin in its motion will deſcribe the dotted circle P O Q, and juſt paſs clear of the inſide of a projection formed on a cock B, which is faſtened on the plate by means of a ſcrew. At about one-fourth of a turn of the pendulum-ſpring, reckoned from its ſtud E, is placed a very delicate tapering piece of ſteel S, having a ſmall hole in it,

Deſcription, with reference to the engraving.

Defcription, with reference to the engraving.

it, through which the pendulum-fpring paffes; and it is faftened to it by means of a pin, and ftands perpendicular to the curve of the fpring. Let the balance be at reft, as reprefented in *Fig.* 1, the banking-pin at P, and the banking-piece at *s*. Suppofe the balance is made to vibrate from P towards O, when P arrives at the banking-piece *s*, it will pafs it without touching, becaufe its extremity *s* lies wholly within the circle traced out by the banking-pin. But when the banking-pin P has arrived at Q, the banking-piece *s* will have advanced to *t*, by the pendulum-fpring winding itfelf up into the figure reprefented by the dotted curve; and when the banking-pin P (now at Q) returns back to P, and paffes on from P towards Q, to approach B, and fo complete the other half-arch of its vibration, before P can arrive at the banking-cock B, the pendulum-fpring will have unwound itfelf into the figure defcribed by the dotted curve, and the banking-piece *s* will have advanced into the pofition at *r* juft touching the banking-cock. Its extremity *r*, however, being thrown beyond the dotted circle, muft neceffarily fall in the way of the banking-pin, which arrives there almoft at the fame moment, and is oppofed by it, without the flighteft fhock to the pendulum-fpring. The model* renders any farther explanation unneceffary.

WILLIAM HARDY.

No. 61, *Chapel-Street, near White-Conduit-Houfe,*
Jan. 18, 1804.

XVI.

Defcription of a new Apparatus for making the gafiform Oxide of Carbon. Communicated by Mr. DEYEUX.†

The progrefs of chemiftry owing chiefly to improved apparatus.

IT is generally admitted, that chemiftry is indebted to the invention of various kinds of apparatus, and the perfection to which they have been brought, for much of the progrefs it has made within thefe laft thirty years.

Inftance in that of Woulfe.

For inftance, before Woulfe made known his apparatus for obtaining the aeriform fluids evolved from various fubftances,

* Which is preferved in the collection of the Society.

† Annales de Chimie, Vol. LIII. p. 76.

either

either when expofed to the action of fire, or when brought into contact with matters capable of combining with them, the operator was obliged to employ bulky veffels, difficult to manage, and fo inconvenient, that he did not wifh to apply them; and when he did, it was impoffible for him to collect the aeriform fluids with any accuracy; for when they were rarefied to a confiderable degree, they efcaped through apertures left purpofely to preferve the veffels from burfting.

Inconvenience of the old inftruments.

Thefe inconveniences are now removed by Woulfe's apparatus, fo that the operations in which gafes are evolved may eafily be performed in veffels of fmall bulk; the gafes may be fubjected to calculation; their quantity as well as quality afcertained with the utmoft precifion; and operations, which were formerly confidered as very hazardous to the operator, may now be continued for hours together without the leaft fear of injury.

Contrafted advantages of the modern arrangement of veffels.

With thefe advantages many others are connected; and it is known to every one, that they are owing to the degree of perfection to which chemifts have brought Woulfe's apparatus, and particularly to their happy application of it on various occafions.

Yet, notwithftanding thefe difcoveries have been carried a great way, it is more than probable that many remain to be made: too much praife therefore cannot be beftowed on thofe who turn their attention to this important object, fince the apparatufes they invent are fo many new means afforded chemifts of collecting an infinite number of products which frequently efcape them, and the knowledge of which may have great influence on the improvement of chemical fcience.

Improvements yet remain to be made.

From thefe motives I have thought it may be of ufe to make known an apparatus just invented by Mr. Baruel, operator to the chemical lectures of the Medical School at Paris.

Invention of Mr. Baruel;

This young chemift, who had often noticed the difficulties and even dangers incurred when it is neceffary to decompofe gafes, or to combine them with different fubftances, attempted to make fome alteration in the proceffes commonly employed in the laboratory for operations of this fort; and after feveral trials he invented an apparatus, which fucceeded beyond his hopes.

for decompofing gafes, or combining them with other fubftances.

I have feen this apparatus employed with the greateft fuccefs for the fabrication of the gafiform oxide of carbon. This gas,

Gafiform oxide of carbon formerly procured with difficulty, is now eafily made.

gas, which formerly was to be procured only with difficulty and in fmall quantity, may now be obtained eafily, readily, and without much expenfe, which affords the advantage of fubjecting it to many more experiments than has hitherto been done.

Other ufes of the apparatus.

The apparatus in queftion may be of ufe likewife for the preparation of fulphurated hydrogen gas, carbonated hydrogen gas, and phofphorated hydrogen gas: it may likewife be employed for faturating a gas with any fubftance whatever, particularly when a high temperature is requifite for this faturation.

The better to make known the apparatus of Mr. Baruel, I will give the defcription of it communicated to me by the author, and add a fketch of it, which will give a more complete idea of all its parts. *Plate VIII.*

Defcription of the apparatus, and method of procuring gafiform oxide of carbon.

Suppofe it is required to make gafiform oxide of carbon: fome charcoal is to be taken very dry and carefully chofen, broken into fmall pieces, and introduced into the three gun-barrels, B, C, D. The charcoal is to be preffed lightly together with an iron rod, fo as to occupy only the part of each barrel that is to be heated, but it muft not be rammed hard. The three gun-barrels are then to be placed horizontally fide by fide in a reverberatory furnace, A, leaving about two inches diftance between them; fecured in their places with moiftened brick earth, and covered with the dome of the furnace.

This done, fix in the mouth of the barrel B the glafs tube E, which is curved fo as to admit its other extremity to be inferted into the neck of the bottle F; the neck of this bottle being large enough to hold likewife the pipe of the curved funnel G. Into the oppofite end of the barrel B one of the ends of the curved tube H is to be introduced, the other end of the tube being inferted into the opening of the barrel D, fo as to form a communication between B and D. A fimilar tube I is fitted to the other extremity of D and the adjacent end of C, forming a communication between thefe two barrels. Laftly, From the oppofite extremity of the barrel C iffues the tube K, which is bent at right angles, fo that its fecond curvature paffes under the receiver M, placed on the fhelf of the pneumatic tub with water L.

Every

Deſcription of the apparatus, and method of procuring gaſiform oxide of carbon.

Every thing being thus arranged, carbonate of lime diluted with a ſmall quantity of water is to be poured into the bottle F, and after all the joints of the tubes have been luted with great care, a fire is to be kindled in the furnace. As ſoon as this fire is ſufficiently ſtrong to make the gun-barrels red-hot, ſulphuric acid is to be poured into the funnel G, and, when it comes into contact with the carbonate of lime in the bottle F, it will expel a large quantity of carbonic acid. This acid preſently paſſes through the tube E into the barrel B, is conveyed from B to D through the tube H, from D to C through the tube I, and thence iſſuing by the tube K, comes out beneath the receiver M, placed on the ſhelf of the pneumatic tub.

The object propoſed by Mr. Baruel in this arrangement of his apparatus, was to oblige the carbonic acid gas evolved from the carbonate of lime, to traverſe the charcoal contained in the three gun-barrels, and thus ſaturate itſelf with all the carbon it could take up.

This method more certain and expeditious than the old;

In fact it is eaſy to conceive, that this method muſt be more certain and expeditious than that formerly employed, when the operator was ſatisfied with paſſing the gas through a ſingle barrel. It is true it was collected, and ſubjected to a ſecond operation, or even to a third; but this mode was tedious, and much of the gas was always loſt. In this new method on the contrary nothing is loſt, and a product is ſeparated at once, which poſſeſſes all the properties that characteriſe the gaſiform oxide of carbon, and which may be uſed unſparingly, ſince it is always obtainable in large quantity.

for nothing is loſt, and the product is obtained at once.

XVII.

Deſcription of an improved Mill for levigating Painters Colours. By Mr. James Rawlinson, *of Derby.**

Great inconveniences and unwholeſome effects of grinding colours on the ſtone.

THE hitherto very unmechanical, inconvenient, and highly injurious method of grinding poiſonous and noxious colours, led me firſt to imagine a better might eaſily be contrived for that purpoſe. It muſt be obvious to every perſon, that the

* From the Memoirs of the Society of Arts for 1804, who awarded him the ſilver medal.

method

method hitherto adopted of grinding colours on an horizontal marble ſlab, with a ſmall pebble muller, requires the body of the perſon who grinds to bend over that ſlab, and conſequently his head; which cauſes him conſtantly to inhale the noxious and poiſonous volatile parts of the paint, which is not unfrequently ground with oil ſaturated with litharge of lead; and if we may judge from the very unhealthy appearance of theſe men, accuſtomed to much colour-grinding, it ſhould ſeem the bad effects of this employment require a ſpeedy remedy.

Machine by which the work is much better performed, and without inconvenience.

The machine, of which I now ſend the Society a model, has not only the advantage of being an effectual remedy of this extenſive and ſevere evil to recommend it, but it grinds the colour much eaſier, much finer, and much quicker, than any method hitherto adopted. Having occaſion for a conſiderable quantity of colour-grinding in the profeſſion in which I am engaged, and that in the fineſt ſtate poſſible, and having made uſe of this machine for ſeveral years, and being more and more convinced of its utility, I thought it my duty to preſent it to the Society of Arts, hoping that it might not be altogether unworthy of their attention. The roller of the machine that I uſe is ſixteen inches and a half in diameter, and four inches and a half in breadth. The concave muller that it works againſt covers one-third of that roller: it is therefore evident, that with this machine I have ſeventy-two ſquare inches of the concave marble muller in conſtant work on the paint, and that I can bring the paint much oftener under this muller in a given ſpace of time, than I could by the uſual method with the pebble muller, which is ſeldom more than four inches in diameter, and conſequently has ſcarcely ſixteen ſquare inches at work on the paint, when my concave muller has ſeventy-two. I do not mean to ſay that a roller, the ſize of that which I now uſe, is the largeſt which might be employed; for truly I believe that a roller two feet in diameter, with a concave muller in proportion, would not be hard work for a man; and then the advantage to the public would be ſtill farther increaſed.

It works with five times the ſurface of the muller,

and is applicable to water-colours as well as thoſe in oil.

This machine will be found equally uſeful for the colours ground in water, as for thoſe ground in oils; and I doubt not but the great importance of this ſimple machine will be very ſoon generally experienced in all manufactories where colours are uſed. The labour neceſſary with this machine, in grinding

ing colours exceedingly fine, is very eafy. It is ufelefs to enter into any minute defcription in this place, as a bare infpection of the machine muft fufficiently explain itfelf.

To the colourman it would evidently be an effential faving of labour, and confequently of expenfe, which will probably have fome weight as a recommendation; and the advantages to the colour-grinder have been already ftated.

Defcription, by reference to the drawing.

Plate V. Fig. 8. A is a roller or cylinder made of any kind of marble; black marble is efteemed the beft, becaufe it is the hardeft, and takes the beft polifh. B is a concave muller covering one-third of the roller, of the fame kind of marble, and fixed in a wooden frame *b*, which is hung to the frame E at *i i*. C is a ftrong piece of iron, about an inch broad, to keep the muller fteady, and is fixed to the frame with a joint at *f*. The fmall binding-fcrew, with the fly nut, that paffes through the centre of the iron-plate at *c*, is for the purpofe of laying more preffure on the muller, if required, as well as to keep it fteady. D is a taker-off, made of a clock-fpring about half an inch broad, and fixed in the manner of a frame-faw in an iron frame *k*, in an inclined pofition to the roller, and turning on pivots at *d d*. G is a flide-board to draw out occafionally, to clean, &c. if any particles of paint fhould fall from the roller, and which alfo forms itfelf for the plate H, to catch the colour on as it falls from the taker-off. F is a drawer, for the purpofe of containing curriers fhavings, which are the beft things for cleaning paint-mills.—E is the frame.

Pulverization, or dry grinding, previous to levigation.

Previous to the colour being applied to the mill, I fhould recommend it to be finely pulverized in a mortar, covered in the manner of the chemifts when they levigate poifonous drugs *. This procefs of dry-grinding is equally neceffary for the marble flab now in ufe; after which it fhould be mixed with oil or

* Or rather in an improved mill, ufed at Manchefter by Mr. Charles Taylor, for grinding indigo in a dry ftate, of which I have annexed a drawing, and reference, to render the whole bufinefs of colour-grinding complete.—*Note of the Author.*

This is the fame apparatus as was ufed under the name of *a philofophical mill*, in the laboratory at Gottorp, about the beginning of the laft century. See the memoir of Dr. Joel Langelot, with an engraving, in Lowthorp's Abridgment of the Philofophical Tranfactions, III. 318.—W. N.

water,

water, and with a ſpatula or pallet-knife put on the roller, near to the top of the concave muller, and the roller turned round, which takes the colour under the muller without any difficulty, and very few turns of the roller ſpread it equally over its ſurface. When it is perceived ſufficiently fine for the purpoſe required, it is very eaſily taken off by means of the taker-off deſcribed, which muſt be held againſt the roller, and the roller turned the reverſe way, which cleans it very quick and very completely; and the muller will only require to be cleaned when you deſiſt or change the colour. It is then turned back, being hung on pivots to the frame at *i i*, and cleaned with a pallet-knife or ſpatula very conveniently. Afterwards, a handful of curriers ſhavings held on the roller, with two or three revolutions cleans it effectually; and there is leſs waſte with this machine than with any marble ſlab.

Method of working.

As to the quantity ground at once on this mill, it muſt be regulated by the ſtate of fineneſs to which it is required to be ground. If it is wanted to be very fine, a ſmaller quantity muſt be put on the roller at a time; and as to time requiſite for grinding a given quantity of colour, this will alſo depend on the ſtate of fineneſs to which it is ground. I have obſerved that my colour-grinder has ground the quantity of colour which uſed to ſerve him per day, with this machine, in three hours, and, as he ſaid, with eaſe. The colour alſo was much more to my ſatisfaction than in the former way, and attended with leſs waſte.

Quantity and fineneſs of grinding.

I have mentioned the pulverizing the colours in a covered mortar, which would prevent waſte, and prevent the duſt and fineſt parts of noxious colours from being injurious to the grinder. In ſome manufactories, where large quantities of colours, prepared from lead, copper, and arſenic, are uſed, this precaution is particularly neceſſary. I do not mean to ſay that my machine is intended to ſuperſede the paint-mill now in uſe for coarſe common colours. It is intended for no ſuch purpoſe; but to ſuperſede the uſe of the very awkward and unmechanical marble ſlab now in uſe, and on which all the colours for china manufactories, coach-painters, japanners, and colour-manufacturers for artiſts, &c. &c. are now ground.

Several of the colour-manufacturers have expreſſed to me their great want of ſuch a machine; and that I had no deſire of troubling the public with a machine that would not anſwer, is

This mill is not a crude project, but has been uſed ſeveral years.

is evident, from my having uſed it ſeveral years before I preſumed to recommend it to their attention. Being therefore now completely convinced of its utility, and hoping that it might relieve a number of my fellow-creatures from a dangerous employment, I have ventured to commit it to the protection of the Society of Arts; hoping, through their means, to ſee its ultimate ſucceſs. And, farther to give the Society the moſt complete aſſurance in my power, I have annexed the opinion of a very ingenious and mechanical friend of mine who has frequently ſeen it work. If any other queſtions ſhould occur to the Committee, that may be in my power to explain, I ſhall gladly do ſo.

I am, Sir,

Your moſt obedient ſervant

JAMES RAWLINSON.

Derby, Feb. 6, 1804.

Charles Taylor, Eſq.

Inſtructions for tying up the bladders of colour.

P. S. When the colour is ground, I recommend the following mode of tying it up in bladders, in preference to the uſual method. Inſtead of drawing the neck of the bladder cloſe, in the act of tying it, inſert a ſlender cylindrical ſtick, and bind the bladder cloſe around it. This, when dry, will form a tube or pipe, through which, when the ſtick is withdrawn, the colour may be ſqueezed as wanted, and the neck again cloſed by replacing the ſtick. This is not only a neater and much more cleanly mode than the uſual one of perforating the bladder; and ſtopping the hole with a nail, or more commonly leaving it open, to the prejudice of the colour; but the bladder, being uninjured, may be uſed repeatedly for freſh quantities of colour.

N. B. The barrel of a quill may be tied, in place of the ſtick, into the neck of the bladder, with its cloſed end outwards, which will keep the colour ſecure in travelling, and when uſed, the end of the quill being cut off, it may afterwards be cloſed by a ſtick.*

* A certificate from Mr. Thomas Swanwick, of Derby, and alſo from Mr. John Middleton, of St. Martin's Lane, confirming the above ſtatement, accompanied theſe papers.

Reference

Reference to the improved Mill for grinding Indigo, or other dry Colours.

Defcription of the mill for dry grinding.

Plate V. Fig. 4. L reprefents a mortar made of marble or hard ftone; one made in the common way will anfwer. M, A muller or grinder, nearly in the form of a pear, in the upper part of which an iron axis is firmly fixed, which axis, at the parts N N, turns in grooves or flits, cut in two pieces of oak projecting horizontally from a wall, and when the axis is at work, are fecured in the grooves by iron pins, O O. P, the handle, which forms a part of the axis, and by which the grinder is worked. Q, the wall in which the oak pieces N N are fixed. R, a weight which may occafionally be added, if more power is wanted.

Fig. 5. fhews the muller or grinder, with its axis feparate from the other machinery; its bottom fhould be made to fit the mortar. S is a groove cut through the ftone.

On grinding indigo, or fuch fubftance, in a dry ftate, in this mill, the muller being placed in the mortar, and fecured in the oak pieces by the pins, the indigo to be ground is thrown above the muller into the mortar; on turning the handle of the axis, the indigo in lumps falls into the groove cut through the muller, and is from thence drawn under the action of the muller, and propelled to its outer edge within the mortar, from whence the coarfer particles again fall into the groove of the muller, and are again ground under it; which operation is continued, till the whole of it is ground to an impalpable powder; the muller is then eafily removed, and the colour taken out.

A wood cover, in two halves, with a hole for the axis, is ufually placed upon the mortar, during the operation, to prevent any lofs to the colour, or bad effect to the operator.

A new

XVIII.

A new and cheap Method of purifying Gold and Silver. By ANDREW THOMSON, *Esq. of Banchory, near Aberdeen. In a Letter from the Discoverer.*

To Mr. NICHOLSON.

SIR,

I INTENDED to have deferred the present communication till such time as I should have it in my power to lay before the public the complete series of experiments in which I have been engaged with regard to the purification of gold and silver. But unluckily I mentioned a few particular circumstances with regard to them, to a man who took it upon him, without my knowledge, to send an account of them for publication to a periodical work. As I understand that work will not appear so soon as your next number, I beg, if you think it worthy of a place, that you will insert the following account of some attempts I have been making to purify the precious metals.

This publication is prematurely made, because the inventor might else be anticipated.

Being much at a loss for want of a crucible of pure silver for the analysis of some minerals, and as all the usual methods practiced for purifying that metal are very troublesome, I set myself to consider the various operations on metals, in hopes of falling on a more simple way of accomplishing my purpose. At length, I found a process of Pelletier's, which promised to succeed, and mine is merely extending his idea a little further than he did himself.

Process of Pelletier for extracting copper from bell-metal.

He was, I believe, employed by the French government to discover an easy way of separating the tin from copper on bell-metal, and the process he gave, is this. Upon the melted bell-metal project black oxide of manganese in powder, frequently stirring the metal till all the tin becomes oxidated by the manganese. He adds a caution, not to add too much manganese, otherwise part of the copper also will be destroyed.

He oxides the tin by manganese; in such proportion as not to affect the copper.

It immediately struck me, that in this way I might be able to oxidate the copper which alloys our silver, and upon making the trial I succeeded completely; I had some impure silver rolled out to about the thickness of a shilling, this I coiled

The author's improved process. Coarse silver was rolled out; then coiled up; bedded in manganese; and ſtrongly ignited for a quarter of an hour.

All the metal was oxided.

coiled up ſpirally, and put into a crucible, the bottom of which was covered with black oxide of mangneſe. I then added more oxide till the ſilver was covered, and all the ſpace between the coils completely filled. A cover was then luted to the crucible, and a ſmall hole left for the eſcape of oxigen gaſs. When this had been expoſed for a quarter of an hour to a heat ſufficient to melt ſilver, I found the ſurface of the manganeſe brown from the loſs of oxigen; but, where the ſilver had been, the whole was one uniform black powder, without the leaſt appearance of metallic luſtre, ſo that I had no doubt, that even the ſilver was become an oxide.

The whole contents were then put into a larger crucible with thrice its bulk of green glaſs.

I then put the whole contents of the firſt crucible into a ſecond of a larger ſize, into the bottom of which I put a quantity of pounded green glaſs, about three times the bulk of the contents of the firſt crucible, and luted on a cover as before, to prevent the acceſs of any inflammable ſubſtance.

Strong heat fuſed the glaſs, and reduced the ſilver pure, and alone in a button.

The crucible was then expoſed to a heat ſufficiently ſtrong to melt the glaſs very fluid. Upon cooling and breaking the crucible, I found the ſilver at the bottom perfectly pure, as its oxide alone could part from its oxigen without the acceſs of ſome inflammable ſubſtance. I find this proceſs anſwers equally well for purifying gold, and to me it ſeems to poſſeſs ſome advantages over all the former methods. The materials uſed are cheap, and a large quantity can be refined as ſoon, and as eaſily as a ſmall quantity, by merely altering the capacity of the crucible you uſe.

This proceſs anſwers equally with gold.

The metal muſt be in thin or ſmall maſſes.

I tried the ſame operation on gold and ſilver in round maſſes, but found it went on very ſlowly, and what I ſcarcely expected, in the firſt part of the proceſs of oxidating the metals, the remaining metal continued uniformly impure or nearly ſo, until the whole was oxidated.

The proportions, &c. are not here given, becauſe the author has haſtened to communicate his proceſs.

I regret that I have been forced to make this matter public, before I could do it in a manner ſatisfactory to myſelf. I wiſhed to have given the exact proportions of alloy, manganeſe, and glaſs to be generally uſed, and to have aſcertained if there is any truth in the old opinion, that ſaltpetre melted with gold deſtroys a part of it. I ſuppoſe that idea may have arizen from the oxigen given out by the nitre in a high heat, oxigenating the copper contained in the impure gold, which has been the ſubject of the experiment.

5 Since

Since the above was written, I have been informed that this matter has actually been publiſhed, but know not in what work. I hope you will ſtill have the goodneſs to inſert this as an *original communication*, as I do not think the perſon who has publiſhed it will have the impudence to call it his own, and as Mr. Kirwan, and other celebrated chemiſts long ago adviſed me to publiſh it, I have already ſtated my reaſons for not following ſuch good advice.

He vindicates his claim.

As I have now been forced to appear before the public, I have hopes I ſhall be able to prevail on ſome of my friends to commit themſelves in the ſame way, in the confidence that their labours will be found uſeful to the public.

I am, Sir,

Your's truly,

ANDREW THOMSON.

Banchory, by Aberdeen,
May 5th, 1805.

XIX.

Memoir on the Propagation of Sound. By M. HASSENFRATZ.*

THE production of ſound is aſcribed by all natural philoſophers to the vibration of the molecules of bodies.

Sound produced by vibrations,

The vibration of theſe molecules admits of two kinds of modification; 1ſt, in velocity; 2ly, in magnitude. The firſt of theſe determines the nature of tones; the ſecond, their force or intenſity.

which differ in velocity and in magnitude.

Sound is tranſmitted to the ear by the molecules that fill the medium or interval between the ſonorous body and the organ of the hearing. The movement of the ſonorous body impreſſes on the molecules of the medium an impulſe, which they tranſmit from one to another, till it reaches the ear, with a greater or leſs velocity. In this tranſmiſſion the vibration may undergo two kinds of alteration: 1ſt, in its velocity; 2dly, in its intenſity. In this memoir I ſhall only tranſcribe ſome experiments relative to the velocity of ſound.

The ſonorous body impreſſes its vibrations on ſome medium, the vibrations of which are continued to the ear.

Philoſophers have long been engaged in determining the velocity of ſound, but conſidering the air as the chief medium by

The velocity of ſound hitherto examined only in air.

* Annales de Chimie, Vol. LIII. p. 64.

by which it is tranſmitted to the ear, they have attended only to its velocity in air, and have employed two different methods to determine this, theory and experiment.

This velocity uniform in a given medium,

and the denſity of the medium is one of its elements.

Theſe two methods have led to the following remarkable reſults: 1ſt, that the velocity of ſound in a given medium, is uniform, whatever its diſtance from the phonic centre, and whatever its intenſity: 2dly, that the denſity of the medium at equal preſſures is one of the elements of the velocity; for it has been found by theory, that the velocity of ſound is the ſame as that of a body falling from half the height of an atmoſphere ſuppoſed to be of equal denſity with the air in the place where the ſound is tranſmitted; and by experiment, that, all other circumſtances being equal, the velocity of ſound is the ſame at different preſſures of the barometer; ſo that it is equal on the ſummit of a mountain and on the ſea-ſhore. In fact, the denſity of the air being proportional to the compreſſing weight, the height of the column of mercury in the barometer, divided by the denſity occaſioned by this preſſure, is a conſtant quantity; and the height of the atmoſphere of a uniform denſity being equal to the total weight of the air divided by its denſity, it follows that the height of the barometer ought to make no difference in the velocity of ſound.

Not affected by the height of the barometer.

Blanconi aſſerts it to be leſs in winter than in ſummer.

Blanconi aſſerts (Comment. Bonon, vol. II. p. 365), that the velocity of ſound is leſs in winter than in ſummer, ſince, according to his experiments, it takes four ſeconds more in winter to traverſe a ſpace of ſixteen Italian miles.

Derham denies this, but is probably wrong.

Derham affirms, that the velocity of ſound is the ſame whether the air be extremely hot or extremely cold, though his tables of experiments will be found on examination favourable to the opinion of Blanconi; for the greateſt velocity of ſound in them was on the 5th of April, at one o'clock in the afternoon, being three miles in 111 half-ſeconds, and the leaſt velocity on the 12th of February, at ſix o'clock in the evening, being three miles in 122 half-ſeconds. As the experiments on the velocity of ſound undertaken by the Academy of Sciences in 1737 were made at temperatures that exhibit only two or three degrees difference, perhaps it would be well, as Mr. Laplace thinks, if they were repeated at a time of the year when the temperature is very different; for experience has taught us, that this velocity is equal in rainy and in fine weather, ſo that nothing but change of temperature can produce any variation in this reſpect.

Not affected by rain or fine weather.

Wha

Whatever theſe reſults may prove, as the experiments on the velocity and propagation of ſound have been hitherto made in the air alone, it was an intereſting enquiry to determine the velocity of ſound, when tranſmitted by other bodies, and particularly of different denſities with reſpect to air. Mr. Laplace, to whom branches of phyſical ſcience are indebted for improvement, invited me about eight months ago to make experiments on this ſubject, and particularly on the propagation of ſound through ſolid bodies; and the experiments of which I ſhall give an account in this paper were principally made in conſequence of that gentleman's ſuggeſtion.

Experiments on the tranſmiſſion of ſound through the ſtone in quarries.

Making experiments in the quarries beneath Paris on the tranſmiſſion of ſound through long galleries, I cauſed a perſon to ſtrike with a hammer againſt a maſs of ſtone, retiring at the ſame time by degrees from the place of ſtriking, in order to diſtinguiſh if poſſible the ſound tranſmitted by the ſtone from that tranſmitted through the air. Placing my ear againſt the maſs of calcareous ſtone through which theſe galleries are carried, at a ſhort diſtance I diſtinguiſhed two ſounds perfectly ſeparate, one tranſmitted by the air, the other by the ſtone. Both ſounds grew weaker in proportion as I retired from the ſtriking point; but that tranſmitted by the ſtone was weakened much more rapidly than that tranſmitted through the air. In a gallery excavated beneath Rue de la Harpe the ſound tranſmitted by the ſtone ceaſed to be audible at 134 paces diſtance; and in a gallery beneath Rue de St. Jaques at 140 paces.—— Through the air the ſound was tranſmitted to 400 or 440 paces diſtance. The ſound tranſmitted by the ſtone always reached the ear much ſooner than that tranſmitted by the air.

It was conveyed through the ſtone only 140 paces; —but through the air 440. It paſſes quickeſt through the ſtone.

Mr. Berthollet, to whom Mr. Laplace imparted theſe reſults, deſirous of being aſſured whether the ſound of a hammer could be tranſmitted through a maſs of ſtone 140 paces thick, requeſted Mr. Gay, by my deſire, to be preſent at my experiments. With this young chemiſt I repeated the experiment of the tranſmiſſion of ſound through ſtone on ſeveral ſeparate maſſes, and he convinced himſelf, that ſound was capable of being tranſmitted through a maſs 150 paces in length.

Experiment repeated, and the ſound conveyed 150 paces.

It was long ago obſerved in working mines, that the noiſe was propagated to a very great diſtance through maſſes of rock; and the line of the ſound heard through the ſtone ſerves on many occaſions to determine the direction in which the gal-

The propagation of ſound through ſtone long ago obſerved by mechanics.

leries are carried on; but no one had attempted to obferve, whether the velocity of the found tranfmitted by the ftone differed from that of the found tranfmitted by the air. My experiments in the quarries underneath Paris have taught me, that the difference is confiderable; and when the gallery is fufficiently ftraight, to be able to difcern the motion of the hammer with the eye, no calculable difference can be perceived between the conveyance of the motion to the eye and that of the found to the ear.

Its velocity apparently equal to that of light.

The differences of the diftances to which found is tranfmitted through folid maffes remarkable.

The diftance which the found of the hammer can be conveyed to the ear varies confiderably with the nature of the ftone and the feparations or fiffures in the mafs. Having caufed a man to ftrike with reiterated blows againft an ifolated wall, built of common ftone of the fame kind as that in the quarries, and cemented with mortar, the found was tranfmitted only thirty paces. Striking in the fame manner on a parapet of hewn ftone raifed on the borders of the Seine, the found was tranfmitted 46 paces. Thefe experiments were made in the open air by day, confequently under circumftances lefs favourable to the propagation of found than when on the calcareous maffes in the quarries; but the difference between 30 and 46 paces, under the fame circumftances, on maffes differing only in the dimenfions of the ftones of which they were formed, is very remarkable.

Experiments repeated,

Encouraged by the fuccefs of my experiments in the quarries, and by the invitation of Mr. Laplace, I attempted to repeat the fame experiments on different fubftances.

on timber.

By the fide of the high road that leads from the place *de la Concorde* to *Chaillot* along the bank of the Seine, on the ftone wharf of St. Leir, oppofite the fteam-engine of Gros-Caillou, is placed a railing 210 paces in length, formed of 31 pieces of timber, feparated by four large pofts. The blow of a hammer at one extremity of this railing was heard diftinctly at the other, though through the air it was audible only 120 paces. At the diftance at which both the founds were audible, that through the wood was heard long before the other; and when, ftanding at the greateft diftance from the place of the blow, I heard only the found tranfmitted through the timber, the velocity of its tranfmiffion was fo great, that it was difficult to diftinguifh any interval between the perception of the found by the ear, and of the motion of the hammer by the eye.

Conveyed farther than in the open air.

Its velocity apparently equal to that of light.

Having

Having convinced myſelf, that the propagation of ſound through ſtone and through wood was effected with much greater velocity than through air, and that the time of its tranſmiſſion to ſuch ſhort diſtances as thoſe on which I was able to make experiments was too little for calculation, I was deſirous of knowing whether the velocity of its tranſmiſſion through metallic ſubſtances were the ſame.

Similar experiments and reſults with long bars of iron.

Several experiments on bars of iron fixed on ſolid maſſes, as the bars that hold together the ſtones of parapets, having given me uncertain reſults, I ſought for iſolated bars of ſufficient length to afford ſome certainty. My firſt experiment was made on the upper bars of an iron railing, 34 paces long, erected on one of the walls of the garden of the Legiſlative Body, adjoining to the Place des Invalides. On ſtriking one extremity of this aſſemblage of bars, two diſtinct ſounds were heard at the other end; that tranſmitted by the bars, and that by the air; the former being always heard firſt. The ſame experiment afterwards repeated on bars of different lengths, gave me the ſame reſult; and this reſult is ſuch, that it is impoſſible to diſtinguiſh, at the ſmall diſtances at which theſe experiments were made, any difference between the tranſmiſſion of the motion by light, and that of the ſound by the ſolid medium.

Mr. Gay ſuppoſed he heard two ſounds through the air, firſt a grave, then an acute.

Repeating my experiments on the velocity and propagation of ſound through the maſſes of ſtone in the quarries beneath Paris, in company with Mr. Gay, this young chemiſt imagined that he diſtinguiſhed two ſounds tranſmitted through the air, one grave and the other acute, which reached his ear in ſucceſſion, the graver ſound appearing to have the greater velocity.

Mairan and others had conjectured the ſame.

This reſult, though contrary to the theory of the propagation of ſound, according to which grave and acute ſounds ought to have the ſame velocity, had already been conjectured by ſeveral philoſophers, particularly by Mairan, and was therefore worth confirmation. For this purpoſe I ſtretched two ſtrings, one of braſs, the other of catgut, ſo as to make them emit two different tones, the firſt one more acute, the ſecond one more grave; then ſtriking both theſe ſtrings at once with the wood of a black lead pencil, the two ſounds, which were confounded together at firſt, appeared to ſeparate at the diſtance of 400 paces in a large gallery of the quarries, and we both imagined we could diſtinguiſh the graver ſound firſt.

Experiment to determine this.

Probable cauſe of fallacy in this experiment.

While obſerving the propagation of ſound in the galleries, I had ſeveral times occaſion to remark, that a ſound at a great diſtance was frequently repeated, either by reflection or by the vibration of the walls, ſo as to cauſe two differerent ſounds to be heard and diſtinguiſhed, which reached the ear in ſucceſſion. As it was poſſible that the difference which appeared to Mr. Gay and me might have been produced by the cauſe here mentioned, I determined to repeat the experiment in the open air.

The experiment repeated in the open air.

For this purpoſe I took two glaſs bells, the tones of which were as an octave and a fifth, that is, the ratio of their vibration was as 1 to 3. A hammer was ſo adjuſted as to ſtrike both the bells at once, and make them ſound at the ſame inſtant. Carrying this inſtrument into the fields, I endeavoured to aſcertain whether the two ſounds reached the ear in unequal times. Several experiments repeated in various places, made me believe a long time, that their velocity was unequal; but having obſerved, that on ſome occaſions the ſounds reached the ear at the ſame time, I was led to remark, that whenever I imagined I diſtinguiſhed a difference of velocity, this difference had been occaſioned by a repetition of the ſound, and that frequently very trifling obſtacles, as trees or hedges, were ſufficient to produce this repetition. I repeated my experiments therefore anew in the midſt of plains of greater or leſs extent, as thoſe of Montronge, Grenelle, St. Denis, &c. and whenever I was remote from any obſtacle capable of producing a repetition, both ſounds were heard at the ſame time. With my inſtrument I could diſtinguiſh the ſound of the two bells at the diſtance of 700 paces or 631 yards; whence it follows, that both experiment and theory concur to demonſtrate, that grave and acute ſounds have the ſame velocity.

The double ſound produced even here by an echo.

Grave and acute ſounds have the ſame velocity.

Concluſions from the experiments.

From the experiments related in this paper it follows: 1ſt, that the velocity of ſound differs according to the medium by which it is propagated: 2dly, that this velocity is much more conſiderable, when it is propagated by ſolid and very denſe bodies, than when by aeriform bodies, and of little denſity: 3dly, that both grave and acute ſounds have the ſame velocity; a reſult to which theory led us.

A very

XX.

A very advantageous Mode of preparing Muriate of Barytes by the Mutual Decompofition of fulphurated Barytes and Muriate of Lime. By Mr. GOETTLING.*

MR. DRIESSEN of Groningen firft obferved, that muriate of lime and fulphate of barytes decompofe each other at a high degree of heat. Trommfdorff verified the experiment of the learned Dutch profeffor, and applied the principle to the preparation of muriate of barytes. He perceived, however, that the whole of the fulphate of barytes was not decompofed in this way, and he thought, that equal parts of the two falts were the fuitable proportion for decompofing them as completely as poffible. Mr. Goettling has deemed the fubject worth farther inquiry, the refult of which was the procefs I fhall now defcribe.

Drieffen firft decompofed fulphate of barytes by muriate of lime; and next Trommfdorff.

One part of native fulphate of barytes in fine powder is to be mixed with half a part of muriate of lime.† This mixture is to be introduced into a Heffian crucible, which is to be clofely covered, and brought gradually to a red heat. The matter muft be kept in a ftate of incandefcence a full half-hour, and frequently ftirred. It is then to be poured out into an iron cone, and after being coarfely powdered, is to be thrown into three parts of boiling water. The veffel being immediately taken from the fire, the mixture is to be ftirred occafionally with a glafs fpatula, and the undiffolved matter is then to be left to fubfide. The clear liquor being decanted off, the refiduum is to be poured into a filter of linen of a clofe texture, and the fluid lightly preffed out. The refiduum being again lixiviated with one part of water, is to be ftrained as before. The liquors are then to be mixed together, and evaporated to a pellicle, to obtain the falt by cryftallization. In this way we fhall have five-eighths of a part of muriate of barytes. The

Mr. Goetling's procefs for preparing muriate of barytes in this way.

* Van Mons's *Journal de Chimie*, Vol. VI. p. 92. Abridged from the *Tafchen-Buch fuer Scheidekuenftler.*

† This muriate is obtained in abundance in our laboratories, as an adventitious product: it may be procured likewife at a very trifling expence by adding lime to the mother-water left after refining common falt.

Muriate of lime, how obtained.

mother-

mother-water of the firſt cryſtallization, which is almoſt wholly muriate of lime, is to be ſet aſide for a freſh operation, or for any other purpoſe.

As the inſoluble maſs of ſulphate of lime ſtill contains a large quantity of ſulphate of barytes, indeed about half the original quantity, it is to be treated afreſh as above, with one fourth part of muriate of lime; and the ſame proceſs is to be repeated with an eighth part of the ſame ſalt. Thus we obtain an addition from one to two eighth parts of muriate of barytes. The ſalts of the various cryſtallizations require to be diſſolved and re-cryſtallized anew, in order to free them from a little muriate of lime, which adheres to them in the firſt cryſtallization.

The ſalt to be purified by re-cryſtallization.

XXI.

Obſervations on the Rectification of Nitric Acid, by Mr. STEINACHER, *Druggiſt at Paris.**

Nitric acid contaminated in its firſt portions with muriatic. Why, according to Berthollet.

IT has long been known, that the firſt portions of nitric acid diſtilled from litharge contain muriatic acid. Berthollet explains this phenomenon by ſaying, that the oxide of lead dividing its action between the two acids, both are ſubjected to the action of expanſibility produced by the heat. Meſſrs. Welter and Bonjour aſſert, that, if muriate of ſilver be employed, oxigenated muriatic acid is formed, which riſes with the firſt portions. If I may be allowed to give the reſults of my labours after thoſe of ſo many able chemiſts, I would ſay, that, when the nitric acid has been ſufficiently concentrated before being ſubjected to rectification on ſilver, or on oxide of lead, the firſt part of the rectified acid is found on trial to contain no muriatic acid, notwithſtanding the nitric acid contained much of it after its concentration; and hence I infer, that an exceſs of water is the true cauſe, that diminiſhes the attraction of the muriatic acid for the oxide of lead or of ſilver.

True reaſon is the preſence of an exceſs of water.

The operator however would in vain expect to ſucceed by merely concentrating his acid before rectifying it, if he uſed a determinate proportion of litharge, or diſtilled to dryneſs, as ſeveral

Concentrating the acid inſufficient, if the proportion of litharge be improper, and the diſtillation carried to dryneſs.

* Van Mons's *Journal de Chimie*, Vol. VI. p. 88.

ſeveral authors direct. The quantity of litharge muſt vary from one to eight ſixteenths of the weight of the acid, according to its degree of impurity. On the other hand, if diſtilled to dryneſs, the latter portions of the nitric acid will carry over with them in ſolution muriate of lead, or of ſilver.

Mr. Steinacher's proceſs.

Four kilogrammes (10lb. 8oz. 16dwt. troy) of nitric acid of the ſhops, at 35°, containing muriatic acid, and a very little ſulphuric, are firſt diſtilled in a reverberatory furnace in a retort placed on an earthen veſſel filled with ſand. The fire muſt be ſo regulated that the drops ſucceed each other ſlowly, and half the acid is to be thus drawn off. It will then give 15° of Baumé's areometer. What remains in the retort is to be poured into a bottle. Its ſpecific gravity will be expreſſed by 4° of the areometer. Litharge being thrown into it in fine powder, and ſtirred with a glaſs rod, will be converted into a white powder in a few hours. More litharge is then to be added in the ſame manner; and this is to be continued, till the litharge retains its colour after ſeveral hours ſtanding. The muriate and ſulphate of lead are then to be left to ſubſide entirely, and the acid is to be decanted off into a tubulated glaſs retort, placed on a ſmall earthen plate filled with ſand, in the midſt of a reverberatory furnace, all the parts of which are retained except the dome. A receiver is to be adapted, which fits cloſely without luting; for, as the vapour of the acid eaſily deſtroys every kind of lute, the product would otherwiſe be liable to become impure; and the diſtillation is to be ſo conducted, as to admit a ſhort interval between the fall of each drop. Great care muſt be taken not to ſuffer the acid to boil, for thus it would be diſſipated in incompreſſible vapour. The firſt half that comes over marks 35°, the ſecond 40°. Both portions are colourleſs, and have all the properties of a very pure nitric acid, if $\frac{1}{32}$ of the liquor be left in the retort.

Beautiful cryſtals of muriate of lead may be obtained when half the acid is diſtilled off.

If a ſtop be put to the diſtillation after the firſt portion is ſeparated, and the retort left to grow cold, you will obtain a beautiful cryſtallization of muriate of lead in large and very brilliant ſtriated hexaedral laminæ. This ſalt is a true muriate, for ſulphuric acid expels from it vapours eaſily diſtinguiſhable to be thoſe of the muriatic acid. On continuing the diſtillation, theſe cryſtals gradually loſe their regular figure, and at length fall to the bottom in a powdery precipitate.

Eaſy

XXII.

Easy Method of making the very combustible Oxide of Phosphorus. By AMICUS.

To Mr. NICHOLSON.

DEAR SIR,

Oxide of phosphorus.

YOU know very well that phosphorus united to a much smaller proportion of oxigen than is requisite to render it into the acid state, brings it into the condition of an oxide which sets on fire sulphur, on just rubbing it against a common match. But the common method of oxidizing phosphorus for phosphoric matches by fixing it in the bottles with a hot iron, is troublesome and wasteful. This oxidation, however, may be effected with great facility and economy by exposing a large proportion, viz. a hundred grains of phosphorus in a jar containing half a pint measure of oxy-muriatic acid, in which circumstance the phosphorus will be melted and fume, but scarcely take fire. After cooling, it must be kept excluded from the air, to prevent the inflammation from mere exposure.

DEAR SIR, Yours,

AMICUS.

May 28*th*, 1805.

XXIII.

Description of an extremely sensible Micro-electrometer. By Mr. MARECHAUX.†

A piece of leaf silver is suspended so as to be moveable in a glass cylinder.

IN a glass cylinder, about an inch and half in diameter and five or six inches high; a piece of leaf silver is suspended from a small pair of nippers, capable of being lowered or elevated as the length of the leaf may require. The piece that carries the nippers may likewise be moved horizontally, so that the leaf may be moved at pleasure nearer to or further from a sphere of copper, which is one of the poles of the instrument.

* Translated from Von Mons's *Journal de Chimie*, Vol. VI. p. 38. Abridged by Van Mons from Gilbert's *Annalen der Physik*.

The

The glafs cylinder has, about 1″ (centimetre, near 4 lines English meafure?) from the plate on which it is fixed, a fmall hole, through which paffes the extremity of a micrometer-fcrew, about the fize of a large goofe-quill, and very carefully cut. This fcrew has fifty threads in a Rhynland inch, cut very deep, though very fine. It is made of two pieces, and fhould be at leaft three quarters of an inch long, to avoid any fhake. The extremity of this fcrew carries a little ball, which is put on after the fcrew is paffed through the opening in the cylinder. To avoid all friction againft the glafs, care is taken that the fcrew, when turned, does not touch the edges of the cylinder. The fcrew carries a plate three inches and a half (3.8 Englifh) in diameter, which has 360 divifions, and confequently divides each thread of the fcrew into as many parts.

Through a hole in the cylinder a micrometer-fcrew paffes,

In this manner we are enabled to determine the fphere of activity of the two electricities in 18000ths of a Rhynland inch. The mounting which contains the female fcrew has a fmall pillar, which advances on the plate, and carries an index, by which the degrees are marked with precifion.

by which the diftance of $\frac{1}{18000}$ of an inch may be meafured.

To ufe this inftrument, which is perfectly infulated by the glafs plate on which it refts, the firft thing is to place the plate in fuch a pofition that the 0 fhall be exactly under the index. The adjufting fcrew which carries the nippers is then to be moved till the leaf filver is fo near the ball, that no light paffes between them. Thus we have the point of contact, and of 0 for the fphere of activity of the two electricities. To be certain that the leaf is brought as clofe as poffible to the ball without being forced out of the vertical direction, the micrometer fcrew fhould be moved a turn firft backward then forward feveral times, and the pofition of the leaf obferved every time the ball is brought into contact with it. The inftrument being thus adjufted, the micrometer-fcrew muft be moved backward one turn, and then we have between the leaf and the ball a diftance of one-fiftieth of an inch, which may be fubdivided at pleafure by means of the plate; for with a plate near four inches in diameter, and by means of the fine needle on which it turns, we may diftinguifh half or even a quarter of a degree if neceffary.

Mode of preparing the inftrument for ufe.

An adjuftment fixed to the plate on which the cylinder refts ferves to afcertain whether the leaf of filver be in fact drawn out

Apparatus for afcertaining the perpendicularity of the leaf.

out of the vertical line by any attractive power. This consists of a fine silk thread, stretched in the same plane as the silver leaf. By means of the screw this thread may be moved both horizontally and vertically, so as to follow the movements of the silver leaf.

Mode of using the instrument.

Every thing being thus arranged, make a communication by means of conducting wires between a single couple of metallic disks, or one constituent part of a pile, placed on a plate of glass, and the instrument, so that one of the metals shall communicate with the top of the instrument, and the other with the bottom. Then by means of a glass handle fitted to it move the plate slowly from one degree to another, and you will find the leaf touch the ball with ordinary electricity, when it is 60° or 80° of the micrometer screw from the vertical plane, in which the leaf silver rested before its communication with the metallic disks. This distance increases for every pair of disks added; and as the ball remains fixed at the point to which the screw has carried it, the motion of the instrument may be observed with great accuracy.

Its extreme sensibility.

This instrument is so sensible, that, if a slender glass tube be rubbed but twice, and brought near the apparatus, though several inches from its summit, it passes through the whole extent of its scale. It is for this reason the inventor calls it a *micro-electrometer*, because we can measure only very weak degrees of electricity with it.

XXIV.

Action of Phosphorus on the Solutions of Metals. By Mr. SCHNAUBERT *.

Phosphorus observed to precipitate metals, by Sage, Ilsemann, and Mrs. Fulhame.

SAGE † had already observed, that phosphorus precipitates the sulphates of copper and of manganese. After him Ilsemann ‡ obtained a crystallization of silver in the humid way by means of phophorus. Still more recently Mrs. Ful-

* Van Mons's *Journal de Chimie,* Vol. VI. p. 95. Abridged from Goettling's *Chemisches Taschenbuch.*

† *Analyse chimique et Concordance des trois Régnes.*

‡ Crell's *Chemische Annalen,* 1789, Tom. II. p. 323.

hame* publiſhed experiments on the reduction of ſome metals by phoſphorus diſſolved in ether. Laſtly, Mr. Schnaubert has undertaken a new inveſtigation of the ſubject, the principal reſults of which are as follows: Schnaubert's experiments.

Gold.

Two little bits of phoſphorus were put into a nitro-muriatic ſolution of gold diluted with a ſmall portion of water. At the expiration of twenty-four hours the ſolution was completely colourleſs, and pellicles of the colour of metallic gold ſwam on the ſurface of the liquid. The phoſphorus itſelf was covered with a deep brown coating, and in this was obſervable in ſeveral places thin layers of reduced gold. At the place where the phoſphorus was a black circle was perceived. The ſolution thus treated by phoſphorus, had not a ſingle atom of gold left in it. Gold completely precipitated in a metallic form by phoſphorus.

Silver.

Some phoſphorus, which was left for twenty-four hours in a nitric ſolution of ſilver diluted by water, was completely covered with metallic ſilver in the form of dendrites, the ramifications of which were directed upwards. During ebullition this remarkable cryſtallization of ſilver, which made the phoſphorus appear as if garniſhed with points, aſſumed firſt a white colour, and afterward formed a light black maſs, which at length became of a light brown colour. Silver precipitated in dendritic cryſtals, which boiling converted into a phoſphure.

Quickſilver.

Mercury diſſolved in nitric acid is precipitated on the phoſphorus in the form of little metallic globules, which cover it entirely. By heating to ebullition the mercurial globules diſappear, and a black maſs without any metallic luſtre is formed. Mercury precipitated in globules, which heat converts to a phoſphure.

Lead.

At the ordinary temperature phoſphorus did not act on the nitric ſolution of lead, though the digeſtion was continued for ſeveral days: at a boiling heat however a change was obſerved in the phoſphorus, which was covered with a grey colour ſlightly metallic. Lead not reduced without a boiling heat.

* Eſſay on Combuſtion, &c.

Copper,

Copper.

Copper powerfully acted on.

No metallic ſolution was more ſtrongly attacked by phoſphorus than that of copper in nitric acid. The phoſphorus was no ſooner introduced into the ſolution of copper, than it aſſumed a black colour. In twenty-four hours it was covered with a ſtratum of metallic copper in very thin layers *; and the ſolution had become much paler. The application of heat cauſed drops of phoſphorus to flow out upon the reduced copper, where they immediately aſſumed a black colour. Theſe drops after a time were in their turn covered with a metallic coat of copper. After this the ſolution was perfectly colourleſs, and ammonia did not detect in it the leaſt particle of metal.

Completely precipitated from the ſolution.

Tin.

Tin partly reduced, partly converted into phoſphure.

Several bits of phoſphorus were put into a ſolution of tin in nitro muriatic acid. The next day the phoſphorus was coloured of a deep brown, only in ſome parts a metallic colour was obſervable. Theſe metallic ſpots diſappeared on boiling, and the phoſphorus became ſtill deeper coloured.

Sulphate of copper forms a beautiful experiment.

* In making this experiment with a ſolution of ſulphate of copper, and ſlightly heating the mixture, at firſt a vapour ariſes, conſiſting of phoſphorus gas, that carries off with it ſome ſmall particles of phoſphorus, which take fire on the ſurface of the ſolution: but the extrication of this vapour ſoon ceaſes, and the phoſphorus becomes hermetically encloſed in a box of copper, in which it is defended againſt any farther action of the ſulphate, and even of the air, to whatever temperature ſhort of fuſing the copper it be afterwards expoſed. The plate of copper that forms this covering is two or three lines thick: it poſſeſſes more tenacity than common copper, for it may be flattened with a hammer in different directions without cracking, which at the ſame time proves the great compreſſibility of the phoſphorus; and it ſhines with a very pure metallic luſtre. On opening the box carefully with a cutting inſtrument, the phoſphorus is found in it retaining perfectly its form, filling its copper caſe completely, and not appearing even to have acted upon the ſulphate.

The phoſphorus enveloped in a caſe of particularly malleable copper impervious to air.

Other metals did not produce this effect.

I did not obtain the ſame effect with ſeveral other metals which I tried, no doubt on account of their containing more oxigen.

VAN MONS.

Manganeſe.

Manganese.

Some bits of phosphorus were put into a sulphuric solution of manganese. The next day the phosphorus was of a deep brown colour, and on its surface was perceivable a pleasing mixture of colours, owing to the reduced metal. The mixture was then heated to ebullition, and after it had grown cold, the manganese was found reduced in the form of little radiating lines on the fused phosphorus, intermixed with a few small globules of the white colour of tin. Manganese reduced by it.

In these experiments we may observe, that the phosphorus, beside deoxidating the metals, united with the metals when reduced to form phosphures, as was evident in the solutions of silver, mercury, and tin. General conclusions.

XXV.

Account of a new Pyrometer, which is capable of indicating Degrees of Heat of a Furnace. By Mr. J. G. SCHMIDT, *of Yassay, in Moldavia. From the Author.*

WITHOUT entering into any detail concerning the substances best calculated for pyrometrical enquiries, I flatter myself that it will be admitted that those must receive the preference which are capable of regularly contracting or expanding, without altering their chemical properties, when subjected to elevated temperatures. Pyrometrical substances.

The permanently elastic aeriform fluids appear to me to be superior in those respects to any other bodies. Gases are the best for strong heat.

Let atmospheric air be freed from moisture by caustic alkalis, or other bodies, and included in a vessel of platina. This vessel A *(Fig. 3, Pl. VI.)*, which may be made of any convenient size, is connected with the tube B B, of as fine a bore as possible. This tube is also made of platina, and reaches into a vessel C, which is filled with water up to *e e*, and into this the tube is fixed air-tight. Out of the vessel C rises a glass cylinder G hermetically sealed, including a thermometer, and a graduated tube F F is secured into the vessel C in a similar manner. Atmospheric air in a vessel of platina.

This

Use and application.

This is the whole construction of my pyrometer. To make use of it nothing more is necessary than to introduce the platina vessel A into the furnace the temperature of which is to be learned. The moment the included air is acted upon by the heat it expands, and expels the water up into the graduated tube F F. This rise will take place accordingly as heat increases. If care be taken that the air be cooled in the vessel C as much as possible (which will be the case from the large surface of water to which it is exposed), it is obvious that a volume of water equal to the volume of air in the vessels of platina, can never pass up into the tube. The refrigeration may be facilitated by the application of vaporizable fluids, such as ether, alcohol, &c.

If the degree of temperature be obtained which the air had before it was subjected to the experiment, and a proper allowance be made for the pressure of the water in F F, the true expansion of the air may thus be found, and compared with the respective temperatures.

XXVI.

New, easy, and economical Method of separating Copper from Silver. By Mr. GOETTLING *.

Sulphuric acid used instead of nitric on account of cheapness,

THERE are four methods of separating copper from silver, all of which require the alloy to be dissolved in nitric acid. As this acid is very dear, Mr. Geottling thought of using the sulphuric in its stead, which is comparatively very cheap. His success perfectly equalled his expectation, and the following is his method:

and with perfect success.

Description of the process.

Having ascertained by the touchstone, or in any other way, the proportion of silver contained in the alloy, take one part of sulphuric acid for every part of silver, and for every part of copper three parts and three-fifths of a part of the same acid. Dilute the acid with half its weight of water, and pour it into a matrass on the alloy reduced to very small pieces. In order

* Translated from Van Mons's *Journal de Chimie*, Vol. VI. p. 77. Originally published in the *Taschen-Buch fuer Scheidekuenstler*, and abridged by Van Mons.

to

to promote the action of the acid, it is of ufe to put one part more to every fixteen parts of the alloy. The matrafs is then to be placed in a fand-heat, and the acid brought to a ftate of ebullition. In two or three hours time the alloy is commonly diffunited and converted into fulphate, particularly if care be taken to ftir the mafs from time to time with a glafs fpatula. This mafs is thick, and frequently hard. While it is ftill hot, fix or eight times its weight of boiling water is to be added to it, and it is to be left fome time longer on the fire. The fulphate of copper will be diffolved, and great part of the fulphate of filver will be precipitated. The operator will now examine whether the whole be completely diffolved; and if it be, a plate of copper, or fome pieces of copper or halfpence tied up loofely in a piece of coarfe linen, muft be fufpended in the mixture, and the whole kept boiling for fome hours. The fulphate of filver will thus be decompofed, and the filver feparated in the metallic ftate.

Mode of afcertaining whether the feparation be complete.

To afcertain whether the feparation be complete, a few drops of folution of muriate of foda are to be dropped into a little of the liquor. If a cheefelike precipitate be formed, it is a proof, that all the filver is not feparated, and in this cafe the ebullition with the copper muft be continued longer. After the whole of the filver is feparated, the liquor is to be poured off, the precipitated filver is to be well wafhed, and the entire feparation of the cuprous falt is to be afcertained by the addition of a few drops of liquid ammonia to the water with which the precipitate has been wafhed, which, if it contain any copper, will be rendered blue by the ammonia. After the filver is thoroughly freed from the fulphate of copper, it may be kept in the ftate of powder as it is, or it may be fufed with a fourth or at moft half its weight of fulphate of potafh.

May be kept in powder, or fufed with fulphate of potafh.

The water poured off is then to be mixed with what was ufed for wafhing the precipitate, and evaporated in a copper pan, fo as to obtain the fulphate of copper by cryftallization. The blue vitriol thus produced will be at leaft equal in value to the fulphuric acid employed.

Blue vitriol obtained equal to the coft of the acid.

If any parts of the alloy remained undiffolved, it fhould be feparated by decantation, and referved for a future operation.

Undiffolved alloy to be fet by.

SCIENTIFIC NEWS.

Extract of a Letter from BRUGNATELLI, *concerning the non-existence of the charged Pile* *.

VOLTA has made many experiments on piles composed of a single metal, and a single wet stratum, which, from being inactive by themselves, become more or less active after affording a passage for a longer or shorter space of time to an electric current set in motion by active pile, &c.

Ritter asserted that a pile composed of one fluid, and one metal, was capable of being charged by another pile, but the fact is, that the fluid is converted into two different fluids by the electric current.

Ritter, the most judicious of the galvanic philosophers of Germany, has asserted, as Volta says, that the active pile, or common electrometer, transmits a real charge to the pile that is itself inactive, which it therefore calls the *charged pile*. Volta however has convinced himself, that no charge is transmitted, but, by virtue of the ordinary chemical action, the electric current being continued, changes the single wet stratum interposed between two pieces of gold, for example, into two different fluids, one acid, by which the electric current issues out of the metal, and the other alcaline, by which it enters; which constitutes a pile of the second order, namely, of one metal and two fluids of different natures, the action of which however does not continue long, because the fluids soon mix.

Mechanical work by Mr. Gregory.

Mr. GREGORY of the Royal Military Academy, Woolwich, has now in the press a Treatise on Mechanics, which is intended to be published in two volumes octavo. The first will be devoted chiefly to the theory, and will be divided into five books under the several heads of statics, dynamics, hydrostatics, hydrodynamics, and pneumatics. The second volume will be chiefly appropriated to practical and descriptive subjects, and will commence with general remarks, rules, and tables, relative to the nature, construction, and simplification of machinery; the effects of friction, and the rigidity of cords; and estimates of the varied energy of different first movers, &c. These will be followed by descriptions arranged alphabetically, of about 100 of the most curious, useful, and important machines. In this latter part, Mr. Gregory has been promised communications from some celebrated civil engineers, so that he hopes, on the whole, to render the work in some measure deserving the attention of those who are engaged in the cultivation and improvement either of the theory or the practice of mechanics.

* Van Mons *Journal de Chimie*, Vol. VI. p. 132.

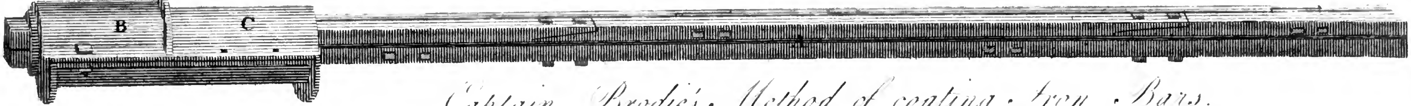
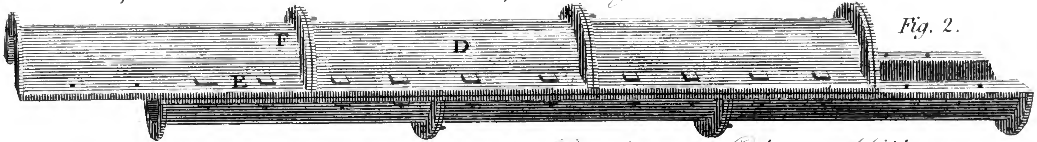


Fig. 1.

Captain Brodie's Method of coating Iron Bars.

Fig. 2.



Mr. Rawlinson's Colour Mill.

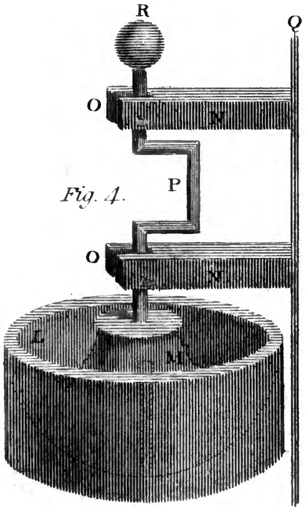


Fig. 4.

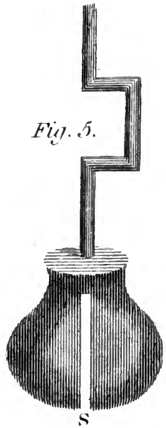
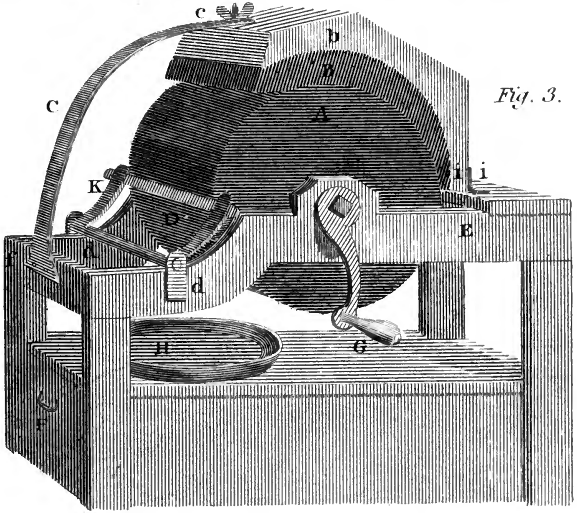


Fig. 5.

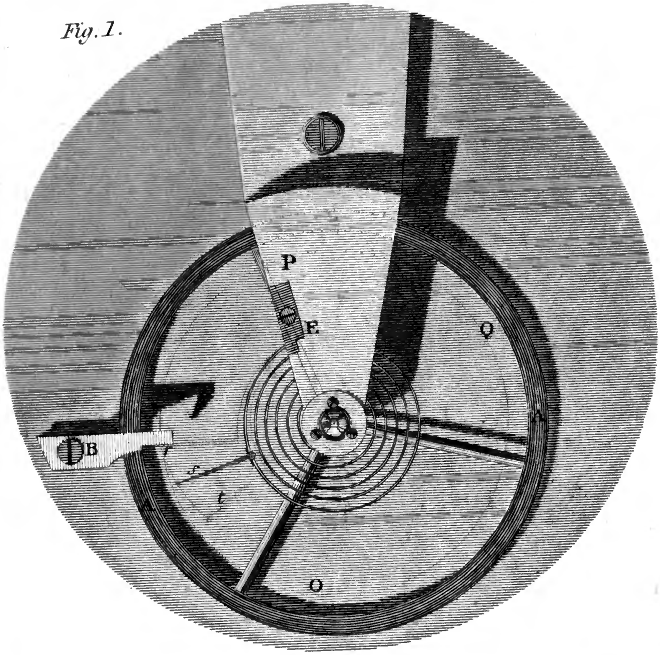
Improved Indigo Mill.

Fig. 3.



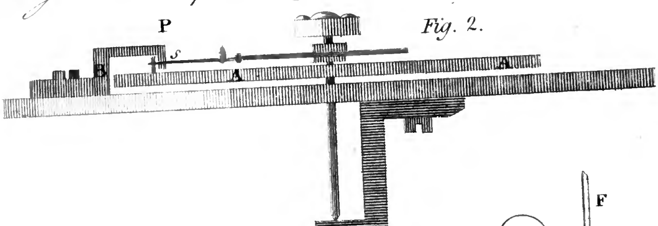
Mutlow Sc. Russell Ct.

Fig. 1.



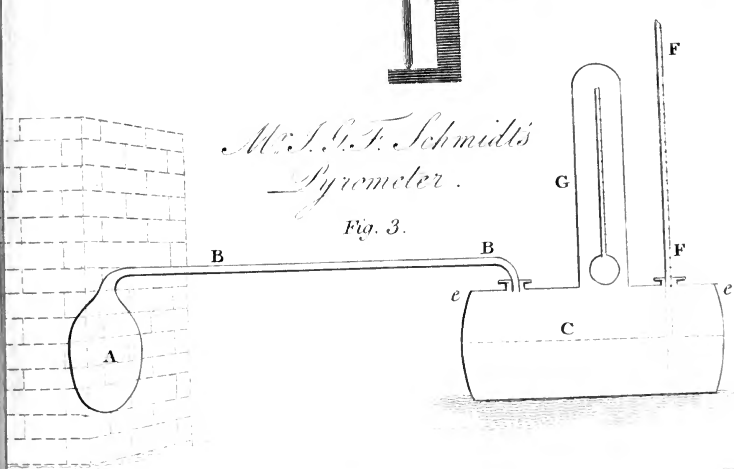
Mr Hardy's method of banking the balance of a Time keeper.

Fig. 2.



Mr J. G. F. Schmidt's Pyrometer.

Fig. 3.



A
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THE ARTS.

JULY, 1805.

ARTICLE I.

Remarks on the Estimation of the Strength of Horses. In a Letter from Mr. O. GREGORY, *of the Royal Military Academy, Woolwich.*

To Mr. NICHOLSON.

SIR,

THE remarks of your ingenious correspondent, Mr. Hornblower, on the various estimates of the *Power of a Horse*, and the absurdity of adopting a quantity so fluctuating and so difficult to ascertain, as a common measure by which the powers and effects of steam engines and other machines are to be estimated and compared, have induced me to throw together a few observations on the same subjects; the theoretic part of which, though familiar to most men of science, seems not to be always known, or at least recollected, by some persons who are employed in the practice; and which are altogether much at your service for insertion in the Journal, if you think them likely to be of any utility.

Reference to Mr. Hornblower's paper on horse powers.

Dr. Desaguliers has given another estimate of the labour of a horse, beside that mentioned by Mr. Hornblower, and which indeed does not seem very consistent with it; for in vol. II. p. 251. of his Experimental Philosophy, he affirms that a horse in an

Estimate of horse power by Desaguliers. 200lb. eight hours per day, at $2\frac{1}{2}$ miles per hour; or $3\frac{2}{3}$ feet per second, an advantageous ſituation, is able to draw 200lbs. eight hours a day, walking at the rate of $2\frac{1}{2}$ miles in an hour, or $3\frac{2}{3}$ feet in a ſecond. This ſtatement of the power of a horſe, though it is not ſo great as that which is arbitrarily aſſumed by Meſſrs. Boulton and Watt, exceeds the determination of M. Sauveur, who eſtimates the mean effort of a horſe at 175 French, or 189 avoirdupois lbs. with a velocity of rather more than three feet per ſecond; and it probably exceeds Mr. Smeaton's ſtatement of 550lbs. moved 40 feet in a minute; though, as will ſoon be ſeen, we are not furniſhed with proper data to inſtitute a comparifon between theſe various reſults. It is probable, however, as obſerved by the ingenious contributor of the article at page 216, vol. IX. of your Journal, that "the loweſt of theſe "performances is more than equal to the average power of a "horſe employed in huſbandry for eight hours per day." So far as my own obſervations on this point extend, I am inclined to conclude that the average work of a ſtout London cart horſe, for eight hours in a day, is little if any more than 130lbs. moved at the rate of three feet in a ſecond, or $2\frac{1}{22}$ miles per hour. But this it would be ridiculous to aſſume poſitively as a univerſal unit of meaſure, in a caſe where the cauſes of variety are ſo numerous, and my opportunities of experiment comparatively few. The eſtimate juſt given, it ſhould be obſerved, is not intended to expreſs what a horſe can draw upon a wheel carriage, where friction alone is to be overcome, after the load is once put into motion, and where a horſe will often draw much more than 1000lbs. but the weight which a horſe would raiſe out of a well, &c. the animal acting by a horizontal line of traction turned into the vertical direction by a ſimple pully or roller, whoſe friction is reduced as much as poſſible.*

—by Sauveur 189lb. avoird. three feet per ſecond,

—by the author 130lb. three feet per ſecond.

Miſapplication of the general principle that gain in power is loſs in time.

Before we can inſtitute any compariſon between the reſults of different experiments, it will always be neceſſary to enquire what machine was interpoſed between the weight moved and the animal, in each caſe, that we may thence deduce the real velocity with which the animal moved, from the velocity of the weight or load given by the obſervations. This is too fre-

* The late Mr. More, Sec. to the Society of Arts, found by the interpoſition of a graduated ſpring inſtrument between the horſe and his work, that the re-action was between 70 and 80lb. when the velocity was three miles in an hour. I think the work was ploughing. See Philoſ. Journal, quarto, Vol. III. 136. N.

quently

quently omitted in consequence of an implicit reliance upon a maxim, which, though highly useful under proper restrictions, is far from universal in its application. In the case before us, if we admit the maxim now alluded to, namely, that *what is gained in power is lost in time,* with regard to the machine through whose intervention the velocity of the weight is rendered different from that of the horse, it would be unsafe to adopt it in the appreciation of the varied energy of the animal when moving with different velocities. The reason of this is obvious. The energy of the horse is obliged to be employed not only in overcoming the weight or resistance which opposes his progress, but in part in moving *himself;* for the particles which constitute his frame possess weight and inertia, and therefore cannot be put into motion without effort. Hence it follows that there is a certain velocity, which may be denoted by U, with which, when the animal moves, his whole power will be employed in producing his own motion solely, without being able to move any other body. If a body whose mass is M be attached to the horse, so that he cannot move without giving an equal velocity to the extraneous body, the same effort being employed both in moving the animal and the mass attached; the velocity V, with which they move must necessarily be less. And if M be farther increased while the weight and energy of the horse continue the same, the velocity V will be still farther diminished; and thus as M increases V will diminish, until when M arrives at a certain magnitude, W, the animal is unable to make any progressive motion, and exerts his force at what is called a dead pull. If M exceeds W, then will V become negative, and instead of the animal advancing with the load, the load will compel him to move backwards, and no useful work can be accomplished.

An horse, having himself to carry, does not exert his force against his work only.

Now these circumstances may be expressed algebraically, by the general formula $M \propto (U-V)^n$, in which the exponent n can only be determined by means of judicious and numerous experiments, where the magnitude of M should be ascertained for many variable values of V between the terms $V=U$, and $V=0$. From this theorem, following the common rules for the maxima and minima of quantities, it may readily be found that in order to have the *useful* work done the greatest possible, we must increase or decrease the weight till V becomes $=\frac{U}{n+1}$, when

Method of computing the effect of horses.

when the performance will be denoted by $\frac{n^n}{(n+1)^{n+1}} \times \text{W U}$

And if the value of V thus exhibited be once ascertained experimentally, we need never be apprehensive of a material loss by a small variation from it; for by a well known property of those quantities which admit of a proper maximum and minimum, a value assumed at a moderate distance from either of these extremes will produce no sensible change in the effect.

Example.

In some of the actions of men, such as dragging a boat along a canal, &c. the value of n in the preceding theorems has been found to be nearly $= 2$. And the draught of horses is conformable to a law not widely different. The best experiments which have yet been made on this point with regard to horses drawing in nearly rectilinear paths, lead us to conclude that n is then very nearly $= \frac{9}{4}$, in the expression $\text{M} \propto (\text{U}-\text{V})^{n}$. Assuming therefore for the utmost walking velocity of a horse, the value $\text{U} = 9$ feet per second, a value which is quite high enough, any proposed estimates of the strength of this animal may be compared with facility. Thus, for example, let us enquire which is greater, the estimate of Mr. More (mentioned by Mr. Hornblower) of 80lbs. three miles per hour, or $4\frac{2}{5}$ feet per second; or that of 130lbs. moved at the rate of three feet per second? Here we shall have

$$(9-3)^{\frac{9}{4}} : (9-4{\cdot}4)^{\frac{9}{4}} :: 130 : 71\tfrac{1}{2}\text{lbs. nearly.}$$

The operation may easily be performed by means of a table of logarithms, and shews that the mean estimate I have laid down, when reduced to the same velocity as that by Mr. Moore, furnishes a result less than his by $8\frac{1}{2}$lbs. Which of these is the most accurate can only be determined by future experiments.

On the power of horses walking in circular paths;

If, however, either of these estimates should be adopted, it may be proper to remark that they would not hold with regard to the power of horses working in circular paths; yet, if it be at all proper to use *horse powers* in estimating the energy of machines, it seems most natural to take these powers as exerted by the animal in a round walk; so that it is still necessary to have a series of experiments to determine the values of n, and the relation of M and V when horses draw in circular walks of different radii. I say, of *different* radii, because it is certain that *cæteris paribus*, the greater the radius of the circle in

—of different radii.

in whose circumference the animal moves, the less fatigue will be occasioned by that kind of motion. Indeed it is obvious, that since a rectilinear motion is the most easy and natural for the horse, the less the line in which he moves is curved, with the greater facility he will walk over it, and the less he need recline from the vertical position. Besides this, with equal velocity at the circumference, the centrifugal force will be less in the greater circle, which will proportionally diminish the friction of the cylindrical part of the trunnions, and the labour of moving the machine. And farther, the greater the radius of the horse-walk, the nearer the chord of the circle in which the horse draws is to coincidence with the tangent, which is the most advantageous position of the line of traction. Hence it follows, that although a horse *may* draw in a walk of 18 feet diameter, yet he will work with far greater ease in one whose diameter is 35 or 40 feet; and it is very desireable that an experimental enquiry should be made to ascertain the proportion and absolute quantity of work in different circles.

The larger walks are most advantageous.

I am of opinion that it would not be difficult to make some useful experiments, while work was actually carrying on at any horse mill, or machine where horses are constrained to move in a circular walk. The simple drawing which accompanies this letter will assist in conveying a clear idea of the method which I fancy might be advantageously adopted. Let AB *Fig.* 1. *Plate* X. be the vertical shaft to which the horizontal horse poles AC, AD, are attached. Let one horse work the machine by drawing at the ear E; but, instead of the transverse bar to which the harness is fixed being simply hung upon the hook *h*, let a good spring steelyard be interposed between that cross-bar and the hook, the graduations of which shall, when the machinery is put into motion, indicate the resistance (in lbs.) overcome by the animal, including the weight of the mass moved, the friction, &c. Near the extremity of the opposite horse-pole AD, let there be fixed a strong and correct common steelyard, whose divisions shall shew the various weights from 40 or 50 to 200lbs. and whose centre of motion shall be at the point *f* on the fixed stand. Let the cord *c* which is fastened to the shorter arm of this steelyard, pass (with as little friction as possible) over the pulley *p*, and thus, being turned into the horizontal direction, or rather, inclining a little upwards, let it be fixed to the cross

Description of a mechanical apparatus for measuring the re-action in mills.

bar

Description of a mechanical apparatus for measuring the re-action in mills.

bar of the harness of a second horse, equal in point of strength to the former. Then if the two horses, thus attached to the ears E and F, be made to pass over the walk in the same direction, following each other constantly at the distance of a semi-circumference; while that which draws at the ear E overcomes the whole pressure and resistance opposed by the work, the other which draws at F by the cord over the pulley *p*, will raise the weight *w* of the steelyard; which therefore, by being moved to and fro upon the arm *f i*, may be brought to exhibit an exact counterpoise, or measure of the exertion and power of the horse. And in order to ensure the greatest degree of accuracy in this respect, the motion of the two animals, and the position of the weight *w*, should be so adjusted, that the same weight should be shewn by the graduations both of the spring and of the lever steelyard. The shaking of the machinery will in some measure disturb the effect; but an ingenious manager of the experiments will find means of checking this: and as to the centrifugal force to which the weight *w* is exposed, it will never be of any material consequence in any of the slow motions which will be produced by this kind of work.

Each experiment should occupy the space of a fair day's work for the horses: for the conclusions deduced from shorter and irregular efforts are always erroneous in excess, and should be guarded against. The rate at which the animals move may readily be ascertained from the known circumference of the walk, and the number of rounds they are observed to make in 10 or 15 minutes. Thus, by continuing the experiments day after day, varying the velocity of the motion in some cases, and the radius of walk in others, such a series of results might at length be obtained, as would in a great measure remove the obscurity and doubt in which this business is at present enveloped. It is scarcely necessary to suggest the propriety of making a few experiments with a view of determining how far a load upon the back of each draught horse, would assist him in his labour. Nor can it be requisite to point out in what way, by means of such steelyards properly applied to waggons, &c. upon tolerably smooth roads, and two horses marching abreast, (one drawing the load, the other raising the weight,) experiments might be instituted to ascertain the magnitude of the efforts of horses when drawing in in rectilinear paths. Judicious experiments having these purposes in view, would certainly be beneficial, as they would enable us to tell what advantages might be expected from the labours of this useful quadruped in different circumstances. But with respect to the adoption of "*horse power*" as a unit of force in estimating the power of steam engines, &c. I confess that if it were as well known, and as unvariable as the length of the day of the equator, I should feel an aversion to applying it to any such purpose. It is a common measure arbitrarily adopted, which has no necessary connection with the subject that is referred to it, which does not in any respect facilitate the computation of the powers of an engine, and which may, without proper caution, lead to considerable errors in the conclusions deduced from it.

The adoption of an arbitrary unit from the power of an horse.

Before I close this letter, already perhaps too long, I beg permission to say a few words respecting the measure which is generally employed to determine the mechanical effect produced. This is the measure of the deservedly celebrated Mr. John Smeaton, who says that "the weight of a body multiplied by the height through which it descends, while driving a machine, is the only proper measure of the power expended; and that the weight multiplied by the height through which it is uniformly raised is the only proper measure of the effect produced." Mr. Smeaton was led to the use of this measure by his professional habits; and many who in this respect pay too great a deference to his authority, have adopted this measure as universal and preferable to any other. Taking this as a popular measure easy to recollect, and simple in its application, it undoubtedly has its uses; but in many instances it is inadequate to the purpose for which it is proposed.

Smeaton's measure of mechanical power and effect. He says it is as the product of the weight into the height passed through in either case;

The late Professor Robison has some excellent observations on this subject, in the article *Machinery*, *Sup. Encyclopædia Britan.* where he lays down the just measure to which the scientific investigator will generally have recourse. "We take, says he, for the measure, (as it is the effect) of exerted mechanical power, the quantity of motion which it produces (or whose accumulation it prevents) by its uniform exertion during some given time. We say *uniform exertion*, not because this uniformity is necessary, but only because, if any variation of the exertion has taken place, it must be known in order to judge of the power."

rectified by Professor Robison.

A single

Smeaton's measure is not applicable to an horse sustaining (but not raising) a weight.

A single instance may be adduced, to which the measure of Mr. Smeaton is inapplicable, and in which we must have recourse to some such measure as that mentioned by Professor Robison. Suppose that a horse while standing still sustains by means of a rope and simple fixed pulley, a mass of a hundred-weight, and thus keeps it suspended at the top of a well, for the space of a minute. Neither the animal nor the weight moves, but shall we therefore say, in conformity, as it would seem, with Mr. Smeaton's measure, that there is no power expended, and no effect produced? On the contrary we know there *is* a power expended, and that the effort if sufficiently long continued would completely tire the horse. The effect which is produced is the annihilation of the simultaneous action of gravity upon the suspended mass; consequently, the effect produced is equal, and contrary to the momentum that would be generated by gravity in the space of a minute. So that $50 \times 32\frac{1}{6} \times 112 = 216160$, is the proper representative of the power expended, as well as of the work done. Were the rope to be cut and the weight suffered to fall for a minute, the same number would likewise denote the labour of the horse in restoring it to its original place, provided that could be accomplished in an equal space of time, without the horse changing his situation.

General statement.

It may not, perhaps, be entirely useless to state this matter rather more universally. To this end, let M represent any mass or body, $g = 32\frac{1}{6}$ feet, the velocity communicated to a body falling freely in the first second of time, and $\dot{t}$ an indefinitely small portion of any time whatever t. Then will $g\,\dot{t}$ be the velocity generated in the instant $\dot{t}$, and $M\,g\,\dot{t}$ the corresponding quantity of motion; this, therefore, measures the effort which must be exerted at each instant to sustain the weight, whether that effort be applied immediately, or through the intervention of a single fixed pulley. Hence it follows, that during the whole time t, the force will have consumed a quantity of motion equal to $\int M\,g\,\dot{t} = M\,g\,t$: that is to say, if t denote the time at the end of which the agent is no longer able to sustain the mass M, we may regard $M\,g\,t$ as being an adequate measure of the force φ of that agent. If the agent not only prevent the mass from falling, but actually raise it with a given uniform velocity V during the whole time t, then we must add the quantity of motion MV to the former, which

which gives $\phi = MV \times Mgt = M(V \times gt.)$ And laſtly, if the agent poſſeſs inertia, its maſs muſt alſo be conſidered. Thus, in the caſe of a horſe whoſe maſs is H, moving along with the velocity V during the time *t*, and raiſing the maſs M, we ſhall have $\phi = (M \times H) V \times Mgt$. And from ſimilar principles formulæ may be inveſtigated to repreſent the power of a firſt mover in more complicated caſes.

It will after all, be proper to diſtinguiſh carefully between the quantity of power expended, and that portion of it which is *uſefully* employed: but a due conſideration of this would too widely extend the limits of the preſent communication. Indeed I ought to apologize to yourſelf, and the ſcientific part of your readers, for dwelling ſo long as I have done upon topics which are well known to all who are converſant in the theory of mechanics: but if thoſe, for whoſe uſe this letter is chiefly intended, ſhall derive ſome preciſe information, or add to the ſtock of their practical knowledge, by any hints of mine, I ſhall not fear being heavily cenſured for having entered thus into minutiæ.

Power expended and power uſefully employed require to be diſtinguiſhed.

I am, Sir,

Your's very reſpectfully,

OLINTHUS GREGORY.

Royal Mil. Academy, Woolwich.
June 10th, 1805.

II.

An Account of ſome Analytical Experiments on a Mineral Production from Devonſhire conſiſting principally of Alumine and Water. By HUMPHRY DAVY, *Eſq. F. R. S. Profeſſor of Chemiſtry in the Royal Inſtitution. From the Philoſophical Tranſactions in* 1805.

I. *Preliminary Obſervations.*

THIS foſſil was found many years ago by Dr. Wavell, in a quarry near Barnſtaple: Mr. Hatchett, who viſited the place in 1796, deſcribed it as filling ſome of the cavities and veins in a rock of ſoft argillaceous ſchiſt. When firſt made known, it

Hiſtory of a Foſſil.

it was conſidered as a zeolite; Mr. Hatchett, however, concluded, from its geological poſition, that it moſt probably did not belong to that claſs of ſtones; and Dr. Babington, from its phyſical characters, and from ſome experiments on its ſolutions in acids, made at his requeſt by Mr. Stocker, aſcertained that it was a mineral body, as yet not deſcribed, and that it contained a conſiderable proportion of aluminous earth.

It is to Dr. Babington that I am obliged for the opportunity of making a general inveſtigation of its chemical nature; and that gentleman liberally ſupplied me with ſpecimens for analyſis.

II. *Senſible Characters of the Foſſil.*

Its ſenſible characters. Radiated hemiſpherical groups; white, ſilky, hard, little tenacious.

The moſt common appearance of the foſſil is in ſmall hemiſpherical groups of cryſtals, compoſed of a number of filaments radiating from a common center, and inſerted on the ſurface of the ſhiſt; but in ſome inſtances it exiſts as a collection of irregularly diſpoſed priſms forming ſmall veins in the ſtone: as yet, I believe, no inſulated or diſtinct cryſtal has been found. Its colour is white, in a few caſes with a tinge of gray or of green, and in ſome pieces (apparently beginning to decompoſe) of yellow. Its luſtre is ſilky; ſome of the ſpecimens poſſeſs ſemi-tranſparency, but in general it is nearly opaque. Its texture is looſe, but its ſmall fragments poſſeſs great hardneſs, ſo as to ſcratch agate.

Other characters.

It produces no effect on the ſmell when breathed upon, has no taſte, does not become electrical or phoſphoreſcent by heat or friction, and does not adhere to the tongue till after it has been ſtrongly ignited. It does not decrepitate before the flame of the blow-pipe; but it loſes its hardneſs, and becomes opaque. In conſequence of the minuteneſs of the portions which it is found, few of them exceeding the ſize of a pea, it is very difficult to aſcertain its ſpecific gravity with any preciſion; but from ſeveral trials I am diſpoſed to believe, that it does not exceed 2,70, that of water being conſidered as 1,00.

III. *Chemical Characters of the Foſſil.*

Chemical habitudes. Soluble in acids and in f. alkalis.

The perfectly white and ſemi-tranſparent ſpecimens of the foſſil are ſoluble both in the mineral acids and in fixed alkaline lixivia by heat, without ſenſibly effervеſcing and without

leaving

leaving any notable refiduum; but a fmall part remains undiffolved, when coloured or opaque fpecimens are expofed to the alkaline lixivia.

Forge heat gave opacity, but did not fufe it. Lofs one-fourth.

A fmall femi-tranfparent piece, acted on by the higheft heat of an excellent forge, had its cryftalline texture deftroyed, and was rendered opaque; but it did not enter into fufion. After the experiment it adhered ftrongly to the tongue, and was found to have loft more than a fourth of its weight. Water and alcohol, whether hot or cold, had no effect on the foffil.

It emitted water.

When it was acted on by a heat of from 212° to 600° Fahrenheit in a glafs tube, it gave out an elaftic vapour, which when condenfed appeared as a clear fluid poffeffing a flight empyreumatic fmell, but no tafte different from that of pure water.

Sulphuric folution afforded fulphate of alumine.

The folution of the foffil in fulphuric acid, when evaporated fufficiently, depofited cryftals which appeared in thin plates, and had all the properties of fulphate of alumine; and the folid matter, when rediffolved and mixed with a little carbonate of potafh, flowly depofited octahedral cryftals of alum.

Muriatic folution contained alumine *only*.

The folid matter precipitated from the folution of the white and femi-tranfparent foffil in muriatic acid, was in no manner acted upon by folution of carbonate of ammonia, and therefore it could not contain any glucine or ittria; and its perfect folubility without refiduum in alkaline lixivia fhewed that it was alumine.

The white varieties contained fome lime:

When the opaque *varieties* of the foffil were fully expofed to the agency of alkaline lixivia, the refiduum never amounted to more than one-twentieth part of the weight of the *whole*. In the white opaque variety, it was merely calcareous earth, for when diffolved in muriatic acid, not in excefs, it gave a white precipitate when mixed with folution of oxalate of ammonia, and did not affect folution of pruffiate of potafh and iron.

and the green, alfo manganefe:

In the green opaque variety, calcareous earth was indicated by folution of oxalate of ammonia: and it contained oxide of manganefe; for it was not precipitated by folution of ammonia; but was rendered turbid, and of a gray colour, by folution of pruffiate of potafh and iron.

the yellow, a trace of iron.

The refiduum of the alkaline folution of the yellow variety, when diffolved in muriatic acid, produced a fmall quantity of white folid matter when mixed with the folution of the oxalate of of ammonia, and gave a light yellow precipitate by expoſure to ammonia; but after this, when neutralized, it did not affect pruſſiate of potaſh and iron, ſo that its colouring matter, as there is every reaſon to believe, was *oxide of iron.*

IV. *Analyſis of the Foſſil.*

Analyſis of 80 grains.
1. Expoſure to heat.

Eighty grains of the foſſil conſiſting of the whiteſt and moſt tranſparent parts that could be obtained, were introduced into a ſmall glaſs tube having a bulb of ſufficient capacity to receive them with great eaſe. To the end of this tube, a ſmall glaſs globe attached to another tube, communicating with a pneumatic mercurial apparatus, was joined by fuſion by means of the blow-pipe.

The bulb of the tube was expoſed to the heat of an Argand lamp; and the globe was preſerved cool by being placed in a veſſel of cold water. In conſequence of this arrangement, the fluid diſengaged by the heat, became condenſed, and no elaſtic matter could be loſt. The proceſs was continued for half an hour, when the glaſs tube was quite red.

Water came over.
19 grains

A very minute portion only of permanently elaſtic fluid paſſed into the pneumatic apparatus, and when examined, it proved to be common air. The quantity of clear fluid collected, when poured into another veſſel, weighed 19 grains, but when the interior of the apparatus had been carefully wiped and dried the whole loſs indicated was 21 grains. The 19 grains of fluid had a faint ſmell, ſimilar to that of burning peat; it was tranſparent, and taſted like diſtilled water: but it ſlightly reddened litmus paper. It produced no cloudineſs in ſolutions of muriate of barytes, of acetite lead, of nitrate of ſilver, or of ſulphate of iron.

very ſlightly acid.

2. Solution of the reſidue in ſulph. acid: precipitation and re-ſolution by alkali. One grain and a quarter of lime remained undiſſolved.

The 59 grains of ſolid matter were diſſolved in diluted ſulphuric acid, which left no reſiduum; and the ſolution was mixed with potaſh, in ſufficient quantity to cauſe the alumine at firſt precipitated again to diſſolve. What remained undiſſolved by potaſh, after being collected and properly waſhed, was heated ſtrongly and weighed; its quantity was a grain and quarter. It was white, cauſtic to the taſte, and had all the properties of lime.

3. Nitric acid was added in exceſs, and then carbonate of am-

The ſolution was mixed with nitric acid till it *became ſour.* Solution of carbonate of ammonia was then poured into it till the effect of decompoſition ceaſed. The whole thrown into a filtrating

filtrating apparatus left ſolid matter, which when carefully washed and dried at the heat of ignition, weighed 56 grains. They were pure alumine: hence the general reſults of the experiments, when calculated upon, indicated for 100 parts of this ſpecimen,

monia. The precip. of alumine, after ignition, was 56 grains.

Of alumine	- -	70
Of lime	- - -	1.4
Of fluid	- -	26.2
Loſs	- - -	2.4

Component parts.

The loſs I am inclined to attribute to ſome fluid remaining in the ſtone after the proceſs of diſtillation; for I have found, from ſeveral experiments, that a red heat is not ſufficient to expel all the matter capable of being volatilized, and that the full effect can only be produced by a ſtrong white heat.

The loſs appears to have been volatilizable matter.

Fifty grains of a very tranſparent part of the foſſil, by being expoſed in a red heat for fifteen minutes, loſt 13 grains; but when they were heated to whiteneſs, the deficiency amounted to 15 grains, and the caſe was ſimilar in other trials.

Different ſpecimens of the foſſil were examined with great care, for the purpoſe of aſcertaining whether any minute portion of fixed alkali exiſted in them; but no indications of this ſubſtance could be obſerved; the proceſſes were conducted by means of ſolution of the unaltered foſſil in nitric acid; the earths and oxides were precipitated from the ſolution by being boiled with carbonate of ammonia; and after their ſeparation, the fluid was evaporated to dryneſs, and the nitrate of ammonia decompoſed by heat, when no reſiduum occurred.

The mineral contains no alkali.

A comparative analyſis of 30 grains of a very pellucid ſpecimen was made by ſolution in lixivium of potaſh. This ſpecimen loſt 8 grains by long-continued ignition, after which it eaſily diſſolved in the lixivium by heat, leaving a reſiduum of a quarter of a grain only, which was red oxide of iron. The precipitate from the ſolution of potaſh, made by means of muriate of ammonia, weighed, when properly treated, 21 grains.

Other analyſes.

Several ſpecimens were diſtilled in the manner above deſcribed, and in all caſes the water collected had ſimilar properties. The only teſt by which the preſence of acid matter in it could be detected, was litmus paper; and in ſome caſes the effect upon this ſubſtance was barely perceptible.

V. *General*

V. *General Obſervations.*

The acid matter in the water was not nitric, mur. or ſulph.

I have made ſeveral experiments with the hope of aſcertaining the nature of the acid matter in the water; but from the impoſſibility of procuring any conſiderable quantity of the foſſil, they have been wholly unſuccefsful. It is, however, evident, from the experiments already detailed, that it is not one of the known mineral acids.

It is foreign to the ſtone.

I am diſpoſed to believe, from the minuteneſs of its proportion, and from the difference of this proportion in different caſes, that it is not eſſential to the compoſition of the ſtone; and that, as well as the oxide of manganeſe, that of iron, and the lime it is only an accidental ingredient, and on this idea the pure matter of the foſſil muſt be conſidered as a chemical combination *of about thirty parts of water and ſeventy of alumine.*

Alumine has an affinity for water.

The experiments of M. Theodore de Sauſſure on the precipitation * of alumine from its ſolutions, have demonſtrated the affinity of this body for water; but as yet I believe no aluminous ſtone, except that which I have juſt deſcribed, has been found, containing ſo large a proportion of water, as thirty parts in the hundred.

Diaſpore exam. by Vauquelin, contains 80 alumine and 16 water.

The diaspore, which has been examined by M. Vauquelin, and which loſes ſixteen or ſeventeen parts in the hundred by ignition, and which contains nearly eighty of alumine, and only three of oxide of iron, is ſuppoſed by that excellent chemiſt to be a compound of alumine and water. Its phyſical and chemical characters differ however very much from thoſe of the new foſſil, and other reſearches are wanting to aſcertain whether the part of it volatilized by heat is of the ſame kind.

Corniſh mineral reſembling the ſubject of this paper.

I have examined a foſſil from near St. Auſtle, in Cornwall, very ſimilar to the foſſil from Barnſtaple in all *its general chemical characters;* and I have been informed, that an analyſis of it, made by the Rev. William Gregor ſome months ſince, proves that it conſiſts of ſimilar ingredients.

Propoſed names.

Dr. Babington has propoſed to call the foſſil from Devonſhire *Wavellite,* from Dr. Wavell, the gentleman who diſcovered it; but if a name founded upon its chemical compoſition be preferred, it may be denominated *Hydrargillite,* from ὑδωρ water, and ἀϱγιλλος clay.

* Journal de Phyſique, Tom. LII. p. 280.

On

III.

On the Aberrations of Light paſſing through Lenſes. By Mr. EZEKIEL WALKER.

THE diſcovery of the aberration of the rays of light, cauſed by their unequal refrangibility, formed a new area in the ſcience of optics. It is the foundation of all *Sir Iſaac Newton's* diſcoveries in light and colours; and alſo the foundation of moſt of the uſeful improvements in the conſtruction of optical inſtruments, that have done ſo much honour to our country. And this ſcience may ſtill derive further improvements from the ſame diſcovery, not only in the conſtruction of inſtruments, but alſo in explaining ſome curious phenomena in nature.

Newton's diſcovery of the aberrations of coloured rays.

But before I attempt to ſhow how the uſe of this property of viſion may be extended, it ſeems neceſſary to give a ſhort account of that kind of aberration which ariſes from the unequal refrangibility of the differently coloured rays of light: the other aberration, or that which is cauſed by the ſpherical figure of the lens, is not here conſidered as being inconſiderable when compared with the former.

Therefore let A C B, *Fig.* 2, *Plate* X. repreſent a plano-convex lens; P A and R B two pencils of white or compound rays of light, falling upon it at the points A and B in a direction parallel to its axis. Alſo let A xv and B yv be the red or leaſt refrangible rays, and A gy and B qx the violet, or moſt refrangible. The red ray from A will cut the violet ray from B at the point x, and the red ray from B will cut the violet ray from A at the point y; through theſe interſections draw the line xy, and this line will be the diameter of the leaſt circular ſpace into which all the rays that fall upon the lens, parallel to its axis, can be collected. And this circle, which for brevity's ſake is called the circle of aberration, is the true focus of the lens or place where the image of the object is formed.

Aberration of light that paſſes through a lens.

Let the ſine of incidence going out of glaſs be n, the ſines of refraction (into air) of the leaſt and the moſt refrangible rays be p and q; then if a plano-convex lens be expoſed with the plane ſide to the ſun, the diameter of the circle of aberration xy (or image of the ſun formed of rays of different refrangibility) is to the diameter of the lens A B, as $q-p$ to $q \times p-2n$.*

* This theorem is well known to mathematicians.

From

From this given ratio of A B to xy it follows, that the image of the ſun within a teleſcope varies with the aperture of the object glaſs.

Sir Iſaac Newton found by moſt accurate experiments that where the ſine of incidence was 50, the ſines of refraction of the red and the violet rays were 77 and 78. Hence $q-p$ is to $q+p-2n$ as 1 to 55. And therefore A B : xy :: 55 : 1; or $xy = \frac{AB}{55}$.

To elucidate this theorem by examples, let the diameter of the object glaſs of a teleſcope, which is 4 inches, be contracted to 3 inches, and afterwards to 2; then the diameters of the circles by aberration formed by parallel rays, will be $\frac{4}{55} = .072$, $\frac{3}{55} = ,054$, and $\frac{2}{55} = .036$ reſpectively.

The ſame Property of Viſion demonſtrated otherwiſe.

Thus, let n repreſent the ſine of incidence, and p and q the ſines of refraction, as before.

The ſine of incidence of every ray, is to its ſine of refraction in a given ratio.*

And the ſine of incidence of the extreme ray P A, varies with the aperture of the lens. For n becomes leſs as P A approaches the axis of the lens E v.

Therefore the ſines of refraction p and q, the angle x A y, and its ſubtenſe xy increaſe or decreaſe with A B. Conſequently the image of the ſun or moon, upon the retina increaſes or decreaſes in magnitude with the pupil of the eye.

Deduction; Now as the rays of artificial light are differently refrangible, it is evident from the given ratio of A B to xy, in which they increaſe or decreaſe at the ſame time that the image of a candle formed in the focus of a convex lens decreaſes with the aperture of the glaſs. For the rays of the ſun and the light of a candle are both governed by the ſame law, in the formation of images in the focus of a lens; but this law does not obtain in the ſame degree in both objects, in conſequence of the rays of the latter being in a more diverging ſtate than thoſe of the former.

—applied in ſupport of the author's experiments. Hence the truth of the reſult of my experiments, which were publiſhed in Vol. IX. page 164 of this Journal, is proved from the diſcoveries of *Sir Iſaac Newton*.

* Newton's Optics, page 64.

What I have further to advance on this ſubject muſt be reſerved until ſome future opportunity, as it would exceed the limits of this paper.

E. WALKER.

Lynn, May 16, 1805.

IV.

New Method of decompoſing the Sulphate of Barytes for preparing the Muriate of that Earth, and Preparation of the Muriate. By Mr. GOETTLING.*

THE muriate of barytes is now in ſuch general uſe, that every improvement in the mode of preparing it muſt meet a favourable reception. This will render the new method of Mr. Goettling acceptable to the public. Muriate of barytes much uſed.

The decompoſition of ſulphate of barytes by means of charcoal requires a ſtrong fire continued a long time, and never ſucceeds completely. This is owing on the one hand to the ſtrongly oxigenated quality of the acidifying principle in the ſulphuric acid, ſo that in its tranſlation to the charcoal it gives out but little caloric; and on the other hand to the difficulty of imparting a certain degree of heat to a mixture, into which a large quantity of a body that is ſo bad a conductor of heat as charcoal enters. To remedy the firſt of theſe defects, I had already propoſed to increaſe the proportion of charcoal a little, and to incorporate with the mixture of charcoal and ſulphate of barytes a twentieth of nitrate of potaſh. To remedy the ſecond, Mr. Goettling adviſes to add muriate of ſoda to the mixture, which ſerves at the ſame time as a conductor of heat and a flux. The following is his method. Decompoſition of ſulphate of barytes by charcoal troubleſome and incomplete. Remedies: to uſe more charcoal and nitrate of potaſh; —and muriate of ſoda.

Four parts of native ſulphate of barytes in fine powder are to be mixed with one part of muriate of ſoda and half a part of charcoal powder. This mixture is to be preſſed hard into a Heſſian crucible, and expoſed for an hour and half to a red heat in a good wind furnace. After it has grown cold, the Mr. Goettling's proceſs. Sulphate of barytes with muriate of ſoda and ſome charcoal are heated together.

* Tranſlated from Van Mons's *Journal de Chimie*, Vol. VI. p. 80. Originally publiſhed in the *Taſchen-Buch fuer Scheidekuenſtler*.

The saline mass is then to be dissolved.

mass is to be reduced to a coarse powder, and boiled for a moment with sixteen parts of water. The liquor is then to be filtered, and kept in well stopped bottles.

The time of exposure to heat may be shortened to one half, if the quantity of muriate of soda be doubled, and the matter occasionally stirred. In this case too, double the quantity of water should be used to lixiviate the mass.

An addition of muriatic acid expels the sulphureous acid and leaves muriates of barytes and of soda.

To prepare muriate of barytes with this lixivium of sulphuret of barytes, which at the same time holds in solution muriate of soda, muriatic acid is to be added in separate portions, till sulphurated hidrogen gas is no longer extricated. The liquor is then to be filtered, a little hot water is to be poured on the residuum, and the liquor is to be evaporated to a pellicle. The lixivium being then filtered afresh, is to be set to crystallize; the muriate of soda, which is much more soluble in water than the muriate of barytes, and not more soluble with heat than without, is not deposited by cooling, and the muriate of barytes crystallizes alone.

These crystals of muriate of barytes are separated by cooling after evaporation.

The remaining lixivium is to be evaporated and set to crystallize again, and this is to be repeated till no more crystals of muriate of barytes are formed.

The barytic salt is perfectly white and pure.

The barytic salt thus obtained, if care be taken not to employ an excess of muriatic acid, is perfectly white, on account of the hydrosulphuret, by which the iron and other metallic substances are precipitated. To be more certain that it contains no muriate of soda, the different products of the crystallization should be mixed together, dissolved, and re-crystallized.

V.

On the Action of Platina and Mercury upon each other. By RICHARD CHENEVIX, *Esq. F. R. S. M. R. I. A, &c. From the Philosophical Transactions for* 1805, *p.* 104.

Freyberg, June 3d, 1804.

Reference to the author's paper on palladium.

ON the 12th of May, 1803, I had the honour of presenting a Paper to the Royal Society,* the object of which was to discover the nature of palladium, a substance just then announced

* Inserted in our Journal, Vol. VII. 85, 176.

to

to the public as a new ſimple metal. The experiments which I had made for this purpoſe led me to conclude that palladium was not what it had been ſtated to be, but that it was a compound of platina and mercury.

Attempts to repeat his experiments have been unſucceſsful.

It was natural to ſuppoſe that a ſubject ſo likely to ſpread its influence throughout the whole domain of chemiſtry, and which tended even to the ſubverſion of ſome of its elements, would awaken the attention of philoſophers. We find accordingly, that it has become a ſubject of enquiry in England, France, and Germany; but the experiments which I had recommended as the leaſt likely to fail, have been found inſufficient to inſure the principal reſult; and I have had the mortification to learn that they have been generally unſucceſsful. I have even reaſon to believe that the nature of palladium is ſtill conſidered by chemiſts, at leaſt with a very few exceptions, as unaſcertained; and that the fixation of mercury by platina is by many regarded as viſionary.

Dr. Wollaſton denies their accuracy, but has not yet publiſhed his own.

The firſt doubts were manifeſted in England; and Dr. Wollaſton very early denied the accuracy of my inquiries. But as he has not publiſhed his experiments, I have had no opportunity of diſcuſſing them. His opinion, however, muſt have ſuch weight in the learned world, that I ſhould have neglected a material fact in the hiſtory of palladium, if I had not mentioned it in this place.

Guyton, in France, coincides with the author.

In France the compound nature of palladium has been more generally credited. When the National Inſtitute was informed of my experiments, a report was ordered to be made upon them, and M. Guyton was the perſon appointed for the purpoſe. He repeated ſome of the experiments, and produced ſome of his reſults. His general concluſion was the ſame as mine.

Vauquelin and Fourcroy,

Meſſrs. Vauquelin and Fourcroy then undertook the ſubject, and they were led by it to the confirmation of the recent diſcovery of Monſ. Deſcotils. The exiſtence of a new metal, which that chemiſt had found in crude platina, received great ſanction from their experiments; and thus the diſcuſſion upon palladium has eſtabliſhed a fact which will be conſidered as intereſting, but which would be much more ſo, were we not already overburthened with ſubſtances which our preſent ignorance obliges us to acknowlege as ſimple.

—infer that palladium contains no mercury; but is platina with the new metal of Descotils.

No sooner were these celebrated chemists convinced of the existence of a new metal in platina, than they concluded that it must play a principal part in the composition of palladium. Shortly after this, in a note to a letter from M. Proust to M. Vauquelin, in which M. Proust expresses his astonishment concerning all he has read upon palladium, Mess. Fourcroy and Vauquelin further declare, as their opinion, that this compound metal does not contain mercury, but is formed of platina and the new metal. Whether this new substance does or does not play a principal part in the formation of palladium, could not be ascertained at the time my experiments were made, because the new metal itself was not then known. But from all that Mess. Fourcroy and Vauquelin have stated, in such of their different memoirs upon this subject as I have seen, the grounds of their supposition have not appeared. May we not refer their opinion, then, to that common propensity of the mind, against which M. Fourcroy has himself warned us with equal justness and eloquence on another occasion, namely, a proneness to be allured by novelty beyond the bounds of rational belief, and to convert principles which are new into principles of universal influence.

Mess. Rose and Gehlen,

Mess. Rose and Gehlen* were the first among the German chemists who instituted experiments upon palladium; and M. Richter has also published a paper on the same subject.

—attempted without success to form palladium by precipitating a mixed solution of platina and mercury by gr. sulphate of iron;

The first attempt of Mess. Rose and Gehlen to form palladium was by the precipitation of a mixed solution of platina and mercury by green sulphate of iron. Their result was precisely that which I had observed when my operations failed altogether, and which of course was the most frequent. This method was repeated twice. The second time the precipitate of platina and mercury was boiled with muriatic acid, in order to free it from iron; but the latter trial was not more successful than the former.

—and also by passing sulphuretted hidrogen through the mixed solution;

Their third experiment was, what they have called, a repetition of that in which I had obtained palladium by passing a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury. Their method was the following.

* Neues Algemeines Journal der Chemie herausgegeben von Hermstadt, Klaproth, Richter, Scherer, Tromsdorff, und Gehlen. Ersten bandes funftes heft.

They

They dissolved one hundred and fifty grains of platina with four hundred and fifty of mercury, and added a solution of hydrosulphuret of potash. They obtained a precipitate which, at first, was black, afterwards gray; but the whole became black by being stirred. To be certain that all the metal was precipitated, they added an excess of sulphuret of potash, and perceived that a part of the precipitate was re-dissolved. The liquor was then filtered, and to that part of it, which contained the re-dissolved precipitate, an acid was added. From this process they obtained a yellow precipitate weighing ninety-one grains; and fifty grains of this exposed to a strong heat, left three-eights of a grain of platina. They obtained no palladium from that part of the precipitate which had not been re-dissolved; and the result of the experiment was complete failure.

by adding hydrosulph. of potash to the m. solution;

—but they obtained no palladium.

I shall not make any observation upon the issue of this process, since, in this case, the best conducted is but too liable to be unsuccessful, and that without any apparent fault in the operator. But as it has been given as a repetition of one of mine, it may not be fruitless to examine how far the repetition was exact.

I had passed a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury, by which means they were precipitated together. My object was so intimately to combine sulphur with these metals, that when exposed to heat, they might (if I may be allowed the expression) be in chemical contact with it at the moment of their nascent metallic state; and as a low temperature suffices, as well to reduce those metals, as to combine palladium with sulphur, I hoped that those effects might be produced before the total dissipation of the mercury. How far my expectation was fulfilled has been stated in my former paper.

They did not exactly repeat the author's experiment. He used the pure gas; they added an alkali;

The sulphuretted hydrogen gas which Messrs. Rose and Gehlen presented to those metals was combined with potash. Now, in the course of docimastic lectures annually delivered by M. Vauquelin at the Ecole des Mines in Paris, when he was Professor at that establishment, it was his constant custom to exhibit an experiment to prove that mercury, precipitated from its solution by many of the alkaline and earthy hydrosulphurets, was redissolved by adding an excess of them.

—and this last substance acting not only on mercury;

It

—but on the platina; It is moreover well known, that there is a ſtrong affinity between potaſh and the oxide of platina, and alſo that when thoſe ſubſtances are brought together in ſolution, a triple ſalt, but little ſoluble, is the reſult. It was to avoid theſe difficulties that I had employed uncombined ſulphuretted hidrogen gas; for the method adopted by Meſſ. Roſe and Gehlen appearing to me to be the application of two divellent forces, I preſumed that it would produce a ſeparation. The reſult of their experiment, which, it appears from their paper, they had not anticipated,

—would have an effect nearly oppoſite to that of the author's proceſs.

ſhews the neceſſity of the precaution I had uſed. The operation which they performed to unite platina and mercury was, in fact, nearly the reverſe of that which they ſuppoſed they had repeated from me, and might have been applied perhaps with a better proſpect of ſucceſs towards the decompoſition of palladium.

They could not fuſe platina.

Meſſ. Roſe and Gehlen ſeem, in many parts of their paper, to queſtion my having fuſed platina; and inform us that although they had expoſed this metal in the furnace of the Royal Porcelain Manufactory of Berlin, in which Wedgewood's pyrometer ceaſed to mark the degree of heat, they could not accompliſh its fuſion. Many of my friends in England have however ſeen the buttons which I obtained, and which were

The author uſed borax.

not few in number. The flux which I had uſed was borax. But no mention is made in any one of the operations of Meſſ. Roſe and Gehlen of borax having been employed.

Particular account of his proceſs.

In many of their attempts they obtained an irregular and porous maſs, which of courſe was of a ſpecific gravity much inferior to that of platina; and it might be inferred from their paper that the diminution of ſpecific gravity, which I had obſerved, was owing to the ſame cauſe. It is true, not only that I had very often obtained ſuch a maſs, but that I had frequently alſo obſerved no diminution whatſoever in the ſpecific gravity of the button which reſulted from my operations. But all thoſe upon which I had founded the concluſions alluded to by Meſſ. Roſe and Gehlen were performed in the following manner,

A Heſſian crucible was lined with lamp black;

and have been repeated ſince. A Heſſian crucible was filled with lamp-black, and the contents preſſed hard together. The lamp-black was then hollowed out to the ſhape of the crucible as far as one-third from the bottom, leaving that much filled with the compreſſed materials; this lining, which adhered ſtrongly to the ſides of the crucible, was made extremely thin,

in

in order not to obſtruct the paſſage of caloric. A cylindrical piece of wood, as a pencil, was then forced into the centre of the thick maſs of lamp-black at the bottom, and the diameter of this rod was determined by the quantity of metal to be fuſed, or varied according to other circumſtances at pleaſure. In general the axis of the cylindrical hole was about three or four times the diameter of the baſis. After withdrawing the rod the crucible was about half filled with borax. Upon this was placed the metal to be fuſed; and if it had been before melted into a cylindrical form, the axis of the metallic cylinder was placed horizontally, and was of courſe perpendicular to the axis of the cylindrical excavation at the bottom of the crucible. More borax was then added to cover the piece of metal, and another quantity of lamp-black was preſſed hard over the whole in order to keep it tight together. An earthen cover was finally luted to the crucible, and in this ſtate it was expoſed to heat in a forge, in which upon another occaſion, I had, in the preſence of Meſſ. Hatchett, Howard, Davy, and others, completely melted a Heſſian crucible lined and prepared in the ſame manner. The fuel which I uſed was the patent coak of Meſſ. Dávey and Sawyer. In the preſent experiments I moderated the heat ſo as not materially to injure the crucible, and upon taking it out of the fire, the lining was generally found ſo compact and ſo firm that it remained in a ſolid maſs after the crucible was broken. When the metallic cylinder occupied the ſpace at the bottom, it was natural to ſuppoſe that it had been fuſed; becauſe in no other ſtate but that of liquidity could it have run into the mould. In order however to prevent all objections I had the precaution to make the hole of a different diameter from the metallic cylinder, and to obſerve whether the neceſſary change in the ſhape of the latter enſued. If after ſuch a teſt, repeated as often as required, I perceived that the metal did not vary in its ſpecific gravity, I thought myſelf authoriſed to conclude that it was exempt from air.

—a ſmall cylindrical cavity was made in the lower part of the lining;

—the metal occupied the middle of a maſs of borax that filled up the cavity of the veſſel;

—ſtrong heat was applied;

—and when the metal loſt its firſt dimenſions and occupied the cylindrical cavity it muſt have been fuſed.

M. Richter ſays that he had hoped to have put himſelf in poſſeſſion of a conſiderable piece of palladium by repeating with minute accuracy the proceſs which I had recommended as the beſt. He precipitated a mixed ſolution of platina and mercury by a ſolution of green ſulphate of iron; and after varying the ſubſequent operations, to which he ſubmitted the product

M. Richter's attempt and concluſions;

product he had obtained by this method, he was led to the following important conclusions amongst others of less consequence. 1st, That two metals, the separate solutions of which are not acted upon by a third body, may be acted upon, and even reduced to the metallic state, by that same body when presented to them in one and the same solution.

2dly. That mercury is capable of entering into combination with platina so, that it cannot afterwards be separated by fire. From the first of these conclusions it is evident, that metals in their metallic state are not incapable of chemical action upon each other; and from the second, that mercury can be fixed (it is purposely that I use the alchemical expression) by platina.

Attempts of Tromsdorff and Klaproth: not successful.

In addition to the chemists abovementioned, I must name two more who in Germany have been occupied by palladium. M. Tromsdorff, in a letter to the authors of the journal already quoted, mentions his having made some fruitless attemps to form this combination; and Klaproth, in a letter to M. Vauquelin published in the *Annales de Chemie*, for Ventose, an 12, likewise says that he could not succeed in producing palladium.

Remarks. The chemists who expected to succeed in a few trials, could not have attended to the author's paper:

Mess. Rose and Gehlen, as well as M. Richter, had conceived from my Paper a reliance on the success of their experiments, which no words of mine had authorised, and have accused me of enforcing the truth of my results with a degree of certainty which their observations do not countenance. M. Richter supposed that the formation of palladium was attended with no difficulty; and in general they have laid so much stress upon this charge, that I should be inclined to think my Paper had not been read by these chemists. In referring to it again, I find there is hardly a page in which I do not mention some failure, and no experiment, of the very few which occasionally succeeded, is related without my stating at the same time that it was repeatedly unsuccessful. As far as regards palladium, it is rather a narration of fruitless attempts than a description of an infallible process, and more likely to create aversion to the pursuit than to inspire a confidence of success. The course of experiments which I had made, as well before as after reading my Paper to the Society, took me up more than two months, and employed me from twelve to sixteen hours almost every day. I had frequently seven

He made many hundreds of experiments, and succeeded completely but four times.

ſeven or eight operations in the forge to perform daily, and I do not exaggerate the number of attempts I made during this time, as well in the dry as in the humid way, in ſtating them to have been one thouſand. Amongſt theſe I had four ſucceſsful operations. I perſevered, becauſe even in my failures I ſaw ſufficient to convince me that I ſhould quit the road to truth if I deſiſted. After all my labour and fatigue I cannot ſay that I had come nearer to my object, of obtaining more certainty in my proceſſes. Their ſucceſs was ſtill a hazard on the dice, againſt which there were many chances; but till others had thrown as often as I had done, they had no ſolid right to deny the exiſtence of ſuch a combination. On this foundation none, I believe, have eſtabliſhed ſuch a right. Meſs. Roſe and Gehlen do not ſay how often their experiments were repeated; but it is probable that if they had been performed very often, theſe authors would not have neglected to mention it. M. Richter ſtates his merely as preparatory to more extenſive reſearches; and M. Tromſdorff, as well as M. Klaproth, mention little more than the fact. If the German chemiſts have concluded againſt my reſults, they have done ſo without juſt grounds, and without having beſtowed upon them that labour and aſſiduity for which they are uſually ſo remarkable.

M. Ritter the great galvaniſt, has eſtabliſhed the order of galvanic relation in bodies.

In this ſtate of uncertainty the compound nature of palladium received an indirect, but a very able, ſupport from ſome experiments of M. Ritter, the celebrated Galvaniſt of Jena. M. Ritter had aſcertained the rank which a greater number of ſubſtances hold in a galvanic ſeries, arranged according to the property they poſſeſs of becoming poſitive or negative when in contact with each other. He had eſtabliſhed the following order, the preceding ſubſtance being in a *minus* relation to that which comes next. Zinc, lead, tin, iron, biſmuth, cobalt, antimony, platina, gold, mercury, ſilver, coal, galena, cryſtallized tin ore, kupfer nickel, ſulphur, pyrites, copper pyrites, arſenical pyrites, graphite, cryſtallized oxide of manganeſe. He had the goodneſs to try palladium in my preſence, and found it to be removed, not only from what I believed to be its conſtituent parts, but altogether from among the metals, and to ſtand between arſenical pyrites and graphite. This reſult led Mr. Ritter into a new and general train of reaſoning, and induced him to undertake the examination of a great

He found palladium to be removed in that order from among the metals to the compound bodies.

great number of alloys, and of a variety of amalgams. He confidered the fubject as a philofopher; and his operations were thofe of a confummate experimentalift. It would be doing him an injuftice to attempt an extract of his ingenious paper, which contains a feries of the moft interefting experiments. I fhall merely obferve for the prefent purpofe, that it very rarely happened that the mixture of two metals bore any determinate relation to the fame metals when feparate; that in every cafe the fmalleft variation in the proportions produced the moft marked effects; and that M. Ritter has furnifhed us with an inftrument calculated to detect the prefence of fuch fmall quantities as have hitherto been confidered as out of the reach of chemiftry. As palladium prefents a very ftriking inftance of the anomaly, to which all compounds feem to be more or lefs fubject, by being removed altogether from the feries of fimple metals, this may ferve to fupport the other proofs of its compound nature.

Other interefting facts.

The objectors to the author's conclufions have not paid attention to the repeated failures of all his methods.

One of the principal objections of thofe who difpute the truth of my conclufions with refpect to palladium, is grounded upon the repeated failure of all the methods I had made ufe of in forming it; but this cannot be of very great weight, when we confider the uncertainty of many other operations of chemiftry. The moft fimple are fometimes liable to fail: and the eafieft analyfes have often given different products in the hands of different chemifts, who yet enjoy indifputable and equal rights to the title of accuracy. The progrefs which we have made in fome parts of the fcience has not removed the obftacles which impede our advancement in others. We have no method of proving the truth of an experiment except by repeating it: yet this often tends to fhow nothing more than contradictory refults, and confequently the fallibility of the art.

A recent cafe in chemiftry which is perfectly analogous to this.

But a recent cafe has occurred which is perfectly analogous to that of palladium. A few years ago Profeffor Lampadius, in diftilling fome fubftances which contained fulphur and charcoal, obtained a liquid product of a peculiar nature. He repeated his experiments, but in vain: and after many fruitlefs attempts abandoned his refearches, and confined himfelf to ftating the fact to the chemical world. Little notice was taken of it, and not much intereft excited by an experiment fo likely to fail. Some time after this Mefs. Clement and Deformes

Deſormes obtained the ſame reſult, and attempted to produce the ſubſtance a ſecond time. They performed a vaſt number of experiments; but their ſucceſs bore no proportion to their diligence and zeal. They publiſhed an account of their proceſs and its conſequences, but gained little credit, as no perſon was fortunate enough to produce the ſame ſubſtance. Many diſbelieved the experiments altogether, and denied the exiſtence of ſuch a combination; whilſt others, leſs inclined to doubt, attributed its formation to fortuitous circumſtances which might never again occur together. In February, 1804, Profeſſor Lampadius, in diſtilling ſome pyritized wood, though with a different intent, obtained the ſame ſubſtance. As he had it now in his power to obſerve the phenomena that attended its formation, he diſcovered, and has communicated to the world, a method of producing it, which never fails. Since his late paper upon the ſubject, as the neceſſary precautions can be followed by every chemiſt, Meſs. Clement and Deſormes have obtained that credit to which their experiments had, in truth, always been entitled; and the formation, of what Profeſſor Lampadius terms his ſulphur-alcohol is no longer a reſult of chance, or accounted for by being ſuppoſed one of thoſe ſubterfuges to which human pride reſorts, in order to ſpare itſelf the confeſſion of human weakneſs.

Diſcovery of the ſulphur-alcohol of Lampadius, which he could not repeat: It was afterwards *once* only verified by Clement and Deſormes.

The inventor re-diſcovered the ſubſtance, and it can be produced at pleaſure.

The obſervation of any new fact becomes a matter of general concern, and truly worthy of philoſophic contemplation, then only, when its influence is likely to be extended beyond the ſingle inſtance to which it owes its diſcovery. Whether water were a ſimple body or a compound could have been of little importance as an inſulated fact; but, connected with the vaſt chain of reaſoning it gave riſe to, it opened a new field for genius to explore. If in the preſent caſe our reſearches were to be confined merely to aſcertaining whether palladium were a ſimple metal or a compound, all the advantages likely to ariſe from the facts obſerved during the inquiry would be loſt; and an object of the moſt comprehenſive intereſt would thus ſink into a controverſy concerning the exiſtence of one more of thoſe ſubſtances, which we have dignified with the name of elements. It was in this point of view that Meſs. Richter and Ritter conſidered the ſubject as far as they went, and

New facts are of value the more extenſive their relations, &c.

 a few

a few facts are stated in my first Paper in support of the opinion, that palladium is but a particular instance of a general truth.

Conclusion: that compound metals may be taken for simple, and the detection be extremely difficult.

By taking the reasoning on this subject then, in its widest extent, we shall be led, I think, to the following conclusion: That metals may exercise an action upon each other, even in their metallic state, capable of so altering some of their principal properties as to render the presence of one or more of them not to be detected by the usual methods. In this is contained the possibility of a compound metal appearing to be simple; but to prove this must be a work of great time and perseverance; and can only be done by considering singly and successively the different cases which it contains, and by instituting experiments upon each. When an affinity which unites two bodies, and so blends their different properties as to make them apparently one, has taken its full effect, it will not be easy to separate them; and this will be more particularly the case when neither of those substances is remarkable for exercising a powerful action upon others. The method of analysis therefore does not promise much success; and the labour of synthesis is sufficient to deter any individual from the undertaking.

Platina and mercury do act on each other, and disguise the properties of each.

It is my intention now to exhibit one example of my position, and to prove that platina and mercury act upon each other, in such a manner as to disguise the properties of both. I shall therefore wave for the present all consideration of palladium, which is in fact but a subordinate instance of the case before us.

Example. Platina is precipitated in the metallic state by green sulph. of iron, if mercury or silver be present; but not else.

When a solution of green sulphate of iron is poured into a solution of platina, no precipitate, nor any other sensible change ensues. This I had already observed, and it has since been confirmed by all who have written upon the subject. But, if a solution of silver or of mercury be added, a copious precipitate takes place. This precipitate contains metallic platina and metallic silver or mercury; some muriate of one or other of the latter metals is also present, as it is not easy to free the solution of platina from all superfluous muriatic acid. But these salts are of no importance in the experiment, and can be separated by such methods as a knowledge of their chemical properties will easily suggest. The proper object of consideration is the reduction of the platina to the metallic state, which

which does not happen when it is alone. I have tried to produce the ſame effect with other metals and platina, but I have not obſerved any thing ſimilar. It is therefore fair to conclude, that when a ſolution of platina is precipitated in a metallic ſtate by a ſolution of green ſulphate of iron, either ſilver or mercury is preſent.

Nitrate of ſilver added to mur. platina throws down a triple muriate of pl. and ſilver.

The precipitation of a mixed ſolution of platina and ſilver requires no further caution than to free the ſalt of platina as much as poſſible from muriatic acid; for as I obſerved in my former Paper, the effect of nitrate of ſilver poured into muriate of platina, is to produce a precipitate, not of muriate of ſilver, but of a triple muriate of platina and ſilver. It was by this experiment that I then proved the affinity of theſe two metals; for when ſilver is not preſent, muriate of platina is among the moſt ſoluble ſalts. The beſt method of preſenting the three ſolutions of platina, ſilver, and green ſulphate of iron to each other, is firſt to pour the filtered ſolution of the laſt into the ſolution of platina, and then, after mixing them thoroughly together, to add the ſolution of ſilver by degrees, and to ſtir them conſtantly. In this, as in all ſimilar operations, the preſence of all acids, ſalts, &c. excepting thoſe neceſſary for the operation, ſhould be avoided; and if proper proportions have been uſed, and all circumſtances attended to, the precipitation of theſe two metals will be very complete.

Preciſe method of precip. pl. and ſilv. in the metallic ſtate by ſulph. iron.

The ſame, but with ſolution of mercury.

But the precipitation by a ſolution of mercury requires to be further conſidered, as the ſtate of oxidizement of this metal, as well as the acid in which it is diſſolved, produces a conſiderable modification in the reſult. In the firſt place the oxide, at the minimum of oxidizement, diſſolved in muriatic acid, is unfit for the experiment; and even the red oxide diſſolved in the ſame acid, or corroſive ſublimate, is not the moſt advantageous. When a warm ſolution of the latter is poured into a mixed ſolution of platina and green ſulphate of iron alſo warm, as in the caſe of ſilver, theſe ſubſtances are brought into contact under the moſt favourable circumſtances. Yet even thus the precipitation is ſlowly and imperfectly formed, often not till ſeveral hours have elapſed; and ſometimes a very great deficiency of weight is obſerved, between the quantities uſed and thoſe recovered directly by this method. If a ſolution of nitrate of mercury be uſed, the effect is produced more

more rapidly, and the precipitate is more abundant. The precipitation of muriate of platina by nitrate of ſilver, and the combination which enſues from it, ſuggeſted to me an experiment which I muſt ſtate at length, as from the reſult of it conſequences are deduced which modify ſome of the experiments of my former Paper.

Mercury diſſolved at the minimum of oxidation will fall in a triple ſalt with platina without any addition of ſulph. iron.

It occurred to me that a method of uniting platina and mercury without the intervention of any other metal, or of any ſubſtance but the ſolvents of theſe metals might be accompliſhed as in the caſe of ſilver and platina. I therefore poured a ſolution of nitrate of mercury, which ſolution being at the minimum of oxidizement, conſequently formed an inſoluble muriate with muriatic acid, into a ſolution of muriate of platina. The reſult was a triple ſalt of platina and mercury, which when the mercury was completely and totally at the minimum of oxidizement was nearly inſoluble. To procure it in this ſtate it is ſufficient to put more metallic mercury into dilute nitric acid than the nitric acid can diſſolve, and to boil them together. This triple ſalt of platina and mercury ſhall be preſently examined. From this it is evident than to produce the union of platina and mercury, the latter being at its minimum of oxidizement in nitric acid the addition of green ſulphate of iron is ſuperfluous.

Not ſo if the mercury be at the maximum of oxidizement.

But if mercury be raiſed to its maximum of oxidizement in nitric acid the caſe is different, for no precipitation occurs till the green ſulphate of iron is added. The moſt advantageous method for precipitating platina and mercury by green ſulphate of iron is, I believe, the following. Mix a ſolution of platina with a ſolution of green ſulphate of iron, both warm, and add to them a ſolution of nitrate of mercury at the maximum of oxidizement alſo warm. It is neceſſary to avoid exceſs of acid, ſalt, &c. in this as in all ſuch caſes. With due care the precipitation of both metals will then be complete.

Whether mercury be precipitable, ſingly, by green ſulphate of iron? &c.

By comparing the experiments made with mercury and platina with thoſe made with ſilver and platina, a ſtriking reſemblance will be found. This induced me to purſue the analogy, and to examine whether, independently of the action of platina, mercury had not the ſame property of being precipitated by green ſulphate of iron as ſilver. Nitrate of ſilver is precipitated by green ſulphate of iron, but muriate of ſilver is not

not ſenſibly acted upon by the ſame reagent. The inſolubility of muriate of ſilver might be alledged as the cauſe of this, if I had not tried the experiment by pouring nitrate of ſilver into green muriate of iron, in which caſe all the ſubſtances were preſented to each other in ſolution. The reſult was not reduction, but muriate of ſilver and nitrate of iron. This fact reſts upon a much more extenſive baſis than mere mechanical circumſtances; and, if purſued with the intention it deſerves, it would lead us into the wide expanſe of complicated affinities and their relations. From reaſoning alone we ſhould be diſpoſed to think that an acid, ſo eaſily decompoſed as the nitric, would be ſufficient to prevent the reduction of a metal which it can diſſolve. But on the one hand it can ſpend its oxygen upon a part of the oxide of the green ſulphate of iron, while on the other its affinity for oxide of ſilver is not powerful enough to retain it, when there is another part of the oxide of iron preſent to deprive it of oxygen. But the affinity of muriatic acid for oxide of ſilver, one of the ſtrongeſt at preſent known, is ſufficient to counterbalance all the other forces. There are many other inſtances of the ſame kind.

Mercury in nitric acid is precipitated (metallic) by gr. ſulph. of iron.

If then a ſolution of green ſulphate of iron be brought into contact with either ſoluble or inſoluble muriate of mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be diſſolved in nitric acid, and green ſulphate of iron be added, the mercury is precipitated in the metallic ſtate.

Theſe remarkable facts modify the reſults formerly ſtated.

Theſe experiments are much ſtronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general conſideration but in their application to the preſent ſubject. They moſt materially modify and are indiſpenſable to the accuracy of the reſults I formerly ſtated; but I was not aware of them at the time I firſt engaged in the inveſtigation of this ſubject. I can alſo now explain a very material difference between ſome proportions obſerved by M. Richter and myſelf in an experiment which that chemiſt had made as a repetition of one of mine.

Precipitation of gold and mercury by gr. ſulphate of iron: repeated by Richter.

I had poured a ſolution of green ſulphate of iron into a ſolution of 100 parts of gold and 1200 of mercury, and had obtained a precipitate conſiſting of 100 of gold and 774 of mercury. M. Richter repeated, as he terms it, this experiment;

ment; that is, he used 100 of gold and 300 of mercury, and obtained a precipitate weighing 102. He is surprised at the difference of weight between our results, which might be owing to his *method* of *repeating* the experiment; but the real cause of this difference lies, as I suppose, in my having accidentally used nitrate instead of muriate of mercury. I had never observed that with mercury and silver this operation had failed, and it must have been, because, on account of the known effect of muriatic salts upon those of silver, I had naturally avoided using a muriate of mercury.

The state of oxidation of the nitrate of mercury used with the solution of gold is of consequence: If the minimum of oxidation prevails, calomel and metallic gold fall down;

But the state of the nitrate of mercury which is used with a solution of gold is not indifferent. As green sulphate of iron reduces mercury when dissolved in nitric acid, as well as gold, it is necessary to mix the solutions of those metals before the green sulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidizement, a precipitate is immediately formed upon mixing the solutions of gold and mercury. Calomel is produced by the muriatic acid of the solution of gold and the oxide of mercury; whilst the gold is reduced to the metallic state by a portion of the oxide of mercury becoming more oxidized, and forming the soluble muriate. The precipitate consists of calomel, of metallic gold, and of a very small portion of mercury which I believe to be in the same state; my reason for thinking so, is, that I have often observed, that a glass vessel in which I had sublimed some of it, was lined with a thin gray metallic coat.

but if the maximum, then nothing falls till the green sulphate of iron is added.

If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green sulphate of iron is added. But at any rate the precipitation of gold and mercury, or of silver and mercury by green sulphate of iron, cannot be adduced as an argument to support the affinity of these metals, since the effect is the same, whether they are separate or united.

These preliminary considerations were necessary as well for the rectification of my former experiments as for the pursuit of my present object; and now to return to platina.

Experiments with platina.
1. Much of the highly oxid. sol. of mercury poured into a mixed solution of plat.

Exper. 1. If a solution of highly oxidized nitrate of mercury be poured into a mixed solution of platina and green sulphate of iron, the first action which takes place passes between the muriatic acid of the solution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained in in folution. This effect makes it advantageous to ufe a greater quantity of the folution of mercury than is merely capable of drawing down the given quantity of platina along with itfelf in the form of a metallic precipitate. When this precipitate is wafhed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but thofe of the mercury and the green fulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer diffolves any part of it, there ftill remains more undiffolved matter than the original weight of the platina ufed in the experiment. By expofure to heat little more is left in general than the original platina; and fometimes even a diminution may be obferved; for as the experiment is not attended with uniform fuccefs, it does not always happen that the whole of the platina is precipitated, but a portion of it will fometimes refift the action of the green fulphate of iron, even when fufficient mercury has been ufed. Before the precipitate has been expofed to heat it is diffolved more eafily than platina by nitro-muriatic acid; and the folution when nearly in a neutral ftate gives a copious metallic precipitate, (yet not equal to the quantity employed,) when boiled with a folution of green fulphate of iron.

and gr. fulph. of iron, throws down platina and mercury united.

Part of the latter is defended from nitric acid.

Heat drives off the mercury.

The compound precipitate is more foluble than plat. in n. m. acid, and gives much metal. precip. by add. of f. of gr. f. of iron.

Exper. 2. When a mixed folution of platina and mercury is precipitated by metallic iron, a quantity equal to the fum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate fo formed, the original weight of platina, together with a confiderable increafe, remains behind, nor can nitric acid fenfibly diminifh it. It yields more eafily than platina to the action of nitro-muriatic acid, and its folution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green fulphate of iron. If this precipitate be expofed to a ftrong heat after it has been boiled with nitric acid, it lofes a great part of its weight, and the platina alone will generally be found to remain.

Exp. 2. Metallic iron throws down the pl. and merc. from a mixed folution. Nitric acid does not deprive this metal of all its mercury. It is more foluble in n. m. acid; and precip. by gr. f. of iron. Heat ufually expels the mercury from this.

Exper. 3. When a quantity of ammoniacal muriate of platina is treated according to the method of Count Muffin Pufhkin to form an amalgam, and, after being rubbed for a confiderable time with mercury, is expofed in a crucible to a heat gradually increafed till it becomes violent, a metallic powder remains in the

Exp. 3. Amalgam (from amon. mur. of plat.) ftrongly heated, leaves a powder, acted on by n. m acid, and copioufly precip. by gr. f. of iron.

the crucible. This powder is acted upon by nitro-muriatic acid, and when the solution is neutralized, a copious precipitate is formed upon the addition of green sulphate of iron. This effect takes place even after the metal has been fused in the manner described in the former part of this paper.

Exp. 4. Sulphur added in the last Exp. causes a greater precip.

Exper. 4. If sulphur be added to the ingredients recommended by Count Mussin Pushkin, and the whole treated as in the last experiment, the quantity of precipitate caused by green sulphate of iron in the nitro-muriatic solution of the button which results from the operation, is generally more considerable.

Exp. 5. Sulphur rubbed with am. m. of plat. easily melts. Mercury being added, strong fusion gives a button, sol. in n. m. acid and precipitable by gr. s. of iron.

Exper. 5. If sulphur be rubbed for some time with ammoniacal muriate of platina, and the mixture be introduced into a small Florence flask, it can be melted on a sand-bath. If mercury be then thrown into it, and the whole be well stirred together and heated, it may afterwards be exposed to a very strong fire and melted into a button. If this be dissolved in nitro-muriatic acid, it will give a precipitate, as in the former cases, by green sulphate of iron.

Exp. 6. The precip. from sol. of plat. and merc. by sulph. hydrogen being fused affords a sol. precip. by gr. s. of iron.

Exper. 6. If a current of sulphuretted hydrogen gas be sent through a mixed solution of platina and mercury, and the precipitate which ensues be collected, the metal may be reduced by heat; and with the addition of borax, it may be melted into a button which will not contain any sulphur. Green sulphate of iron causes a precipitate in the solution of this metal also.

Exp. 7. So likewise the precip. by phosphate of ammon.

Exper. 7. If to a mixed solution of platina and mercury, phosphate of ammonia be added; a precipitate takes place. If this be collected and reduced, it will be acted upon by green sulphate of iron poured into its solution, in the same manner as the metallic buttons in the preceding examples.

Exp. 8. Nitr. of merc. at min. of oxidiz. precipitates mur. of pl. The metallic compound dissolved in n. m. acid is precipitable by gr. s. of iron.

Exper. 8. I have already mentioned that when a solution of nitrate of mercury, at the minimum of oxidizement, is poured into a solution of muriate of platina, a mercurial muriate of platina is precipitated. The supernatant liquor may be decanted and the residuum washed; if this be reduced and afterwards dissolved in nitro-muriatic acid, it will yield a precipitate with green sulphate of iron. This method appears to me to be the neatest for combining platina and mercury, as the action which takes place is independent of every substance except the metals themselves.

Exper.

Exper. 9. One of the moſt delicate teſts that I have obſerved in chemiſtry is recent muriate of tin, which detects the preſence of the ſmalleſt portion of mercury. When a ſingle drop of a ſaturate ſolution of neutralized nitrate or muriate of mercury is put into 500 grains of water, and a few drops of a ſaturate ſolution of recent muriate of tin are added, the liquor becomes a little turbid, and of a ſmoke-gray colour. If theſe 500 grains of liquid be diluted with ten times their weight of water, the effect is of courſe diminiſhed, but ſtill it is perceptible. I had on a former occaſion obſerved the action of recent muriate of tin upon a ſolution of platina. If a ſolution of recent muriate of tin be poured into a mixed ſolution of platina and mercury, not too concentrated, it can hardly be diſtinguiſhed from a ſimple ſolution of platina. But if too much mercury be preſent, the exceſs is acted upon as mercury; and the liquor aſſumes a darker colour than with platina alone.

Exp. 9. Recent muriate of tin is the moſt delicate teſt of mercury. It does not indicate the mercury in a mixed ſolution of that metal and platina.

From all theſe experiments it is evident that mercury can act upon platina, and confer upon it the property of being precipitated in a metallic ſtate by green ſulphate of iron. By *Experiments* 1 and 2, it is proved, 1ſt, That platina can protect a conſiderable quantity of mercury from the action of nitric acid; and 2dly, That mercury can increaſe the action of nitromuriatic acid upon platina. From *Experiments* 3, 4, 5, 6, 7, 8, it appears that mercury can combine with platina in ſuch a manner as not to be ſeparated by the degree of heat neceſſary to fuſe the compound, ſince after the fuſion it retains that property, which is eſſentially characteriſtic of the preſence of mercury in a ſolution of platina. The 8th *Experiment* proves that the action of mercury upon platina is not confined to the metallic ſtate; but that theſe metals can combine and form an inſoluble triple ſalt with an acid which produces a very ſoluble compound with platina alone. The 9th *Experiment* ſhows that platina can retain in ſolution a certain quantity of mercury, and prevent its reduction by a ſubſtance which acts moſt powerfully to that effect, when platina is not preſent. That part of the general poſition therefore which is the object of this paper is proved, if theſe experiments, upon being repeated by other chemiſts, ſhall be found to be accurate.

Hence, 1, 2. Platina protects mercury from nitric acid, and mercury renders platina more ſubject to nitromur. acid: 3—8. Platina retains mercury in the ſtrong heat of fuſion. 8. Mercury and platina act on each other in ſaline compoſition. 9. Platina defends mercury in ſolution from being reduced by m. of tin.

One or two of the above experiments ſeem to be in contradiction to ſome that I have ſtated in my paper upon palladium; for

Remark on palladium.

for in the present examples platina protects mercury against the action of nitric acid; whereas in palladium the mercury is not only acted upon itself, but it conduces to the solution of platina in the same acid. I am well aware of this objection; but confining myself to my present object, I shall wave all further discussion of it till another opportunity. In the mean time, however, it may be laid down as an axiom in chemistry, that the strongest affinities are those, which produce in any substance the greatest deviation from its usual properties.

The compounds of platina and mercury are subject to great variations, &c.

When a button of the alloy of platina and mercury as prepared by any of the above methods, is dissolved in nitro-muriatic acid, and afterwards precipitated by green sulphate of iron, the entire quantity of the alloy used is seldom obtained. A considerable portion of platina resists the action of green sulphate of iron, and remains in solution. This may be looked upon as the excess of platina, and can be recovered by a plate of iron. Hence it appears that less mercury is fixed, than can determine the precipitation of the entire quantity of platina; yet in this state it can draw down a greater quantity of the latter, than when it is merely poured into a mixed solution of platina, not before so treated. Indeed the whole of these experiments tend, not only to show that these two metals exercise a very powerful action upon each other, but that they are capable of great variation in the state of their combination; and also that substances possessing different properties have resulted from my attempts to combine platina with mercury.

Investigation of the quantity of mercury thus fixed. It is supposed or inferred to be about 17 mercury to 83 platina, with the specific gravity 16.

This observation furnished me with a method of ascertaining, or at least of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been seldom attended with full success, I mention the result with the entire consciousness of the uncertainty to which it is subject. I observed the increase of weight, which the original quantity of platina had acquired in some cases after it had been treated with mercury, and fused into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green sulphate of iron from a solution of this alloy, and supposed it to contain the whole quantity of mercury found as above. But, even if attended with complete success, there is a chemical reason which must make us refuse our assent to this estimate. It is possible, and not

not unlikely, that a portion of mercury may be retained in ſolution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean reſult, however, was that the precipitate by green ſulphate of iron conſiſted of about 17 of mercury, and 83 of platina, when the ſpecific gravity was about 16.

With regard to palladium, leſt it ſhould be ſuppoſed that either my own obſervations, or thoſe of others have given me cauſe to alter my opinion; I will add, that I have as yet ſeen no arguments of ſufficient weight to convince me, in oppoſition to experiment, that palladium is a ſimple ſubſtance. Repeated failure in the attempt to form it I am too well accuſtomed to, not to believe that it may happen in well conducted operations; but four ſucceſsful trials, which were not performed in ſecret, are in my mind a ſufficient anſwer to that objection. By determining the preſent queſtion we may overcome the prepoſſeſſion conceived by many againſt the poſſibility of rendering mercury as fixed, at an elevated temperature, as other metals; we may be led to ſee no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path between the viſions of alchemy on the one hand, and the equally unphiloſophical prejudices on the other, which they are likely to create. In the courſe of experiments juſt now related, I have ſeen nothing but what tends to confirm my former reſults, yet the only means which I can, after all, preſcribe for ſucceeding, is perſeverance.

The author continues to think that palladium is a compound, ſince four ſucceſsful experiments were made of forming it.

To aſcertain whether the opinion of Meſſ. Fourcroy and Vauquelin, that the new metal was the principal ingredient in palladium had any juſt foundation, I obſerved the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

Platina, purified in Fourcroy and Vauquelin's method, is equally combinable with mercury.

I might have added ſome more experiments to corroborate the evidence I have adduced to prove my aſſertion of the fixation of mercury by platina; but Meſſ. Vauquelin and Fourcroy have promiſed the Inſtitute of France a continuation of their reſearches, and M. Richter concludes his paper with ſaying that he will return to the ſubject. From the labours of ſuch perſons ſome great and important fact muſt iſſue, and I hope that the preſent ſubject will not be excluded from their conſideration. The facts contained in this paper cannot be

These chemiſts and M. Richter have promiſed to purſue the ſubject.

fubmitted to too fevere a fcrutiny; and no judge can be more rigid or more competent than the very perfon who was the firft to doubt my former experiments. But it is neceffary to be obferved by whoever fhall think them worth the trouble of verifying, that even thefe experiments are liable to fail unlefs proper precautions are ufed: that I have never operated upon lefs than one hundred grains; and that the refults, which I have ftated, however fimple they may appear, have been the conftant labour of fome weeks.

POSTSCRIPT.

Dr. Wollafton's palladium in crude platina proves nothing; and it may have been the product of the amalgamating procefs.

Since this paper was written Dr. Wollafton has publifhed fome experiments upon platina. He has found that palladium is contained in very fmall quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. Wollafton. I have not yet feen a copy of his paper; but I fhall merely obferve here that, whatever be the quantity of palladium found in a natural ftate, no conclufion can be drawn as to its being fimple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events, the amalgamation to which platina is fubmitted before it reaches Europe, is fufficient to account for a fmall portion of palladium.

VI.

Method of obviating the Neceffity of Lifting Ships. By Mr. ROBERT SEPPINGS, *of Chatham Yard**.

Great faving and advantage of fufpending inftead of lifting fhips.

THe method here to be defcribed of fufpending, inftead of lifting, fhips, for the purpofe of clearing them from their blocks; affords a very great faving to the public; and abridges two-thirds of the time formerly ufed in this operation. From the faving of time another very important advantage is derived, namely, that of enabling large fhips to be docked, fufpended, and undocked, the fame fpring tides. Without enumerating the inconveniencies arifing, and, perhaps, injuries, which fhips are liable to fuftain, from the former

* From the Tranfactions of the Society of Arts, who voted him the gold medal, 1804.

practice

practice of lifting them, and which are removed by the present plan; that which relates to manual labour deſerves particular attention; twenty men being ſufficient to ſuſpend a firſt rate, whereas it would require upwards of 500 to lift her. The ſituation which Mr. Seppings held in Plymouth-yard, attached to him, in a great degree, the ſhoring and lifting of ſhips, as well as the other practical part of the profeſſion of a ſhipwright. Here he had an opportunity of obſerving, and indeed it was a ſubject of general regret, how much time, expenſe, and labour, were required in lifting a ſhip, particularly ſhips of the line. This induced him to conſider whether ſome contrivance could not be adopted to obviate theſe evils. And it occurred to him, that if he could ſo conſtruct the blocks on which the ſhip reſts, that the weight of the ſhip might be applied to aſſiſt in the operation, he ſhould accompliſh this very deſireable end. In September 1800, the ſhoring and lifting the San Joſef, a large Spaniſh firſt-rate, then in dock at Plymouth, was committed to his directions; to perform which, the aſſiſtance of the principal part of the artificers of the yard was requiſite. In conducting this buſineſs, the plan, which will be hereafter deſcribed, occured to his mind; and from that time, he, by various experiments, proved his theory to be correct: the blocks, conſtructed by him, upon which the ſhip reſts, being ſo contrived, that the facility in removing them, is proportionate to the quantity of preſſure; and this circumſtance is always abſolutely under command, by increaſing or diminiſhing the angle of three wedges, which conſtitute one of the blocks; two of which are horizontal, and one vertical. By enlarging the angle of the horizontal wedges, the vertical wedge becomes of conſequence more acute; and its power may be ſo increaſed, that it ſhall have a great tendency to diſplace the horizontal wedges, as was proved by a model, which accompanied the ſtatement to the Society; where the power of the ſcrew is uſed as a ſubſtitute for the preſſure of the ſhip.

Twenty men ſupply the place of 500.

Hiſtory.

The contrivance is a block, compounded of two horizontal wedges, acting under an obtuſe vertical wedge,

which by the ſhip's weight, can, if needful, diſplace the others.

Mr. Seppings cauſed three blocks to be made of hard wood agreeable to his invention, and the wedges of various angles. The horizontal wedges of the firſt block were nine degrees; of the ſecond, ſeven; and of the third, five; of courſe, the angle of the vertical wedge of the firſt block was 162 degrees; of the ſecond 166; and of the third, 170 Theſe blocks, *or* wedges,

Experiments.

Angles of the wedges.

wedges, were well executed, and rubbed over with soft soap for the purpose of experiment. They were then placed in a dock, in his Majesty's yard at Plymouth, in which a sloop of war was to be docked; on examining them after the vessel was in, and the water gone, they were all found to have kept their situations, as placed before the ship rested upon them. Shores in their wake were then erected to sustain the ship, prior to the said blocks being taken from under the keel. The process of clearing them was, by applying the power of battering-rams to the sides of the outer ends of the horizontal wedges; alternate blows being given fore and aft; by which means they immediately receded, and the vertical wedges were disengaged. It was observed, even in this small ship, that the block which was formed of horizontal wedges of nine degrees, came away much easier than those of seven, and the one of seven, than that of five. In removing the aforesaid blocks by the power of the battering-rams, which were suspended in the hands of the men employed, by their holding ropes passed through holes for that purpose, it was remarked by Mr. Seppings, that the operation was very laborious to the people; they having to support the weight of the battering-rams, as well as to set them in motion. He then conceived an idea of affixing wheels near the extremity of that part of the rams, which strikes the wedges. This was done before the blocks were again placed; and it has since been found fully to answer the purpose intended, particularly in returning the horizontal wedges to their original situations, when the work is performed for which they were displaced; the wheels also giving a great increase power to the rams, and decrease of labour to the artificers; besides which, the blows are given with much more exactness. The same blocks were again laid in another dock, in which a two-decked ship of the line was docked. On examination they were found to be very severely pressed, but were removed with great ease. They were again placed in another dock, in which a three-decked ship of the line was docked. This ship having in her foremast and bowsprit, the blocks were put quite forward, that being the part which presses them with the greatest force. As soon as the water was out of the dock, it was observed, that the horizontal wedges of nine and seven degrees had receded some feet from their original situations. This afforded Mr. Seppings a satisfactory proof, which experience has since demonstrated, (though

A sloop rested upon them, and shored up.

The horizontal wedges were then driven out,

by battering-rams,

supported on wheels.

Other experiments.

It was found that the weight of the vessel was capable of pressing out the wedges;—

(though many perſons before would not admit of, and others could not underſtand, the principle) that the facility of removing the blocks or wedges, was proportionate to the quantity of preſſure upon them. The block of five degrees kept its place, but was immediately cleared, by applying the power of the battering-rams to the ſides of the outer ends of the horizontal wedges. The above experiments being communicated to the Navy Board, Mr. Seppings was directed to attend them, and explain the principle of his invention; which explanation, farther corroborated by the teſtimonials of his then ſuperior officers, was ſo ſatisfactory, that a dock was ordered to be fitted at Plymouth under his immediate directions. The horizontal wedges in this, and in the other docks, that were afterwards fitted by him, are of caſt iron, with an angle of about five degrees and a half, which, from repeated trials, are found equal to any preſſure, having in no inſtance receded, and, when required, were eaſily removed. The vertical wedge is of wood, lined with a plate of wrought iron, half an inch thick. On the bottom of the dock, in the wake of each block, is a plate of iron three quarters of an inch thick, ſo that iron at all times acts in contact with iron.

and therefore that if made of the proper angle they would be ſecure, and always capable of being driven out.

The beſt angle is about $5\frac{1}{2}$ degrees for caſt iron wedges.

The placing the ſuſtaining ſhores, the form and ſizes of the wedges, and battering-rams, &c. alſo the proceſs of taking away, and again re-placing, the wedges of which the block is compoſed, are alſo exemplified by a model.

The dock being prepared at Plymouth, in Auguſt, 1801, the Canopus, a large French 80-gun ſhip, was taken in, and reſted upon the blocks; and the complete ſucceſs of the experiment was ſuch, that other docks were ordered to be fitted at Sheerneſs and Portſmouth dock-yards, under Mr. Seppings's directions. At the former place a frigate, and at the latter a three-decked ſhip, were ſuſpended in like manner. This happened in December, 1802, and January, 1803; and the reports were ſo favourable, as to cauſe directions to be given for the general adoption of theſe blocks in his Majeſty's yards. This invention being thought of national conſequence, with reſpect to ſhips, but particularly thoſe of the navy, government has been pleaſed to notice and reward Mr. Seppings for it.

A large 80-gun ſhip ſuſpended by this method.

The time required to diſengage each block, is from one to three minutes after the ſhores are placed: and a firſt-rate fits on

Each block may be diſengaged in three minutes.

on about fifty blocks. Various are the caufes for which a fhip may be required to be cleared from her blocks, viz. to fhift the main keel; to add additional falfe keel; to repair defects; to caulk the garboard feams, fcarples of the keel, &c. Imperfections in the falfe keel, which are fo very injurious to the cables, can in the largeft fhip be remedied in a few hours by this invention, without adding an additional fhore, by taking away blocks forward, amid-fhips, and abaft, at the fame time; and when the keel is repaired in the wake of thofe blocks, by returning them into their places, and then by taking out the next, and fo on in fucceffion. The blocks can be replaced in their original fituations, by the application of the wheel battering-rams to the wedges, the power of which is fo very great, that the weight of the fhip can be taken from the fhores that were placed to fuftain her. There were one hundred and fix fhips of different claffes, lifted at Plymouth dock-yard, from the 1ft of January, 1798, to the 31ft of December, 1800; and, had the operation of lifting taken lefs time, the number would have been very confiderably increafed; for the faving of a day is very frequently the caufe of faving the fpring tide, which makes the difference of a fortnight. The importance of this expedition, in time of war, cannot be fufficiently eftimated.

It is not required to fufpend the fhip in all cafes. For the repairs may be done by fucceffive removal of blocks.

The lifting of fhips was a frequent operation in the navy.

This invention may be applied with great advantage, whenever it is neceffary to erect fhores, to fupport any great weights, as, for inftance, to prop up a building during the repair of its foundation, &c. Captain Wells, of his Majefty's fhip Glory, of 98 guns, ufed wedges of Mr. Seppings's invention for a fid of a top-gallant maft of that fhip. In 1803, the top-gallant mafts of the Defence, of 74 guns, were fitted on this principle by Mr. Seppings: and, from repeated trials, fince fhe has been cruizing in the North Sea, the wedge fids have been found in every refpect to anfwer.

This invention is of value in other undertakings.

Fid of a top gallant maft,

But it is Mr. Seppings's wifh that it fhould be underftood, that the idea of applying this invention to the fid of a top-gallant maft originated with Capt. Wells, who well underftood the principle, and had received from him a model of the invention.

applied by Capt. Wells,

When it is required to ftrike a top-gallant maft, the top ropes are hove tight, and the pin which keeps the horizontal wedges in their place, is taken out, by one man going aloft for that purpofe; the other horizontal wedge is worked in the fid

Manœuvre of ftriking the maft, &c.

fid, as ſhown in the drawing and model that accompany this ſtatement. The upper part of the fid hole is cut to form the vertical wedge. The advantage derived from fidding top-gallant maſts in this way is, that they can be ſtruck at the ſhorteſt notice, and without ſlacking the rigging, which is frequently the cauſe of ſpringing and carrying them away, particularly thoſe with long pole heads. The angle of the horizontal wedges for the fids of maſts ſhould be about twenty degrees. It can now be done without ſlacking the rigging.

The above Account was accompanied with Certificates from Sir John Henſlow, Surveyor of the Navy; Mr. M. Didram, maſter-ſhipwright of Portſmouth-Yard; and Mr. John Carpenter, foreman of Sheerneſs Dock-yard, confirming Mr. Seppings's ſtatement. Certificates.

Reference to the Engraving of Mr. Seppings's method of obviating the neceſſity of lifting Ships. Plate XI.

This plan and ſection of a ſeventy-four gun ſhip deſcribes the method of obviating the neceſſity of lifting ſhips, when there may be occaſion to put additional falſe keels to them, or to make good the imperfections of thoſe already on; alſo when it may be neceſſary to caulk the garboard ſeams, ſcarples the keel, &c. by which means a very conſiderable part of the expenſe will be ſaved, and much time gained. The blocks are cleared, and again returned by the following proceſs. A ſufficient number of ſhores are placed under the ſhip to ſuſtain her weight, and ſet taught, ſtationed as near the keel as the working of the battering-rams fore and aft will admit. Avoid placing any oppoſite the blocks, as they would in that caſe hinder the return of the wedges with the battering-rams. A blow muſt then be given forward on the outer end of the iron wedges with the battering-rams in a fore and aft direction, which will cauſe them to ſlide aft, as ſhown in the plan. The battering rams abaft then return the blow, and the wedges again come forward; by the repetition of this operation, the wedges will be with great eaſe cleared, and the angular block on the top will drop down. When the work is performed, the block muſt be replaced under the keel, and the wedges driven back by working the rams athwart-ſhips, as deſcribed in the ſection. Deſcription and reference to the plate, &c. Inſtructions.

N. B. In

N. B. In returning the iron wedges, to avoid ſtraining the angular blocks, it is propoſed to leave a few of them out forward and aft, and ſtop the ſhip up, by laying one iron wedge on the other, as ſhown at Fig. 1, Plate XI.

To facilitate the buſineſs, blocks may be cleared forward and aft at the ſame time, ſufficient to get in place one length of falſe keel. If the falſe keel ſhould want repairing, it may be done without any additional ſhores, by clearing one block at a time, and when the keel is repaired in the wake of that block, return the wedges, as above directed, and clear the next, &c.

Section and Plan, Plate XI. *Fig.* 2.

Parts of the ſection and plan.

A. Keelſon.
B. Ceiling.
C. Floor timber.
D. Dead or riſing wood.
E. Plank of the bottom.
F. Keel and falſe keel.
G. Angular blocks with a half-inch iron-plate bolted to them.
H. Caſt-iron wedges.
I. Iron plate of three-fourths of an inch thick on the bottom of the dock.
K. Battering-rams, with wheels, and ropes for the hands.
L. Caſt-iron wedges, having received a blow from forward.
M. Shores under the ſhip to ſuſtain her weight.

Fig. 3, repreſents part of a top-gallant maſt fitted with a wedge fid.
a. Top-gallant maſt.
b. Fid, with one horizontal wedge worked on it.
c. Moveable wedge, with the iron ſtrap and pin over it, to keep it in its ſituation.
d. Truſſel trees.

VII. *On*

VII.

On muscular Motion. *By* ANTHONY CARLISLE, *Esq. F. R. S. being the Croonian Lecture, read before the Royal Society, November* 8, 1804.

ANIMAL physiology has derived several illustrations and additions, from the institution of this lecture on muscular motion; and the details of anatomical knowledge have been considerably augmented by descriptions of muscular parts before unknown. Introduction.

Still, however, many of the phenomena of muscles remain unexplained, nor is it to be expected that any sudden insulated discovery shall solve such a variety of complicated appearances.

Muscular motion is the first sensible operation of animal life: the various combinations of it sustain and carry on the multiplied functions of the largest animals: the temporary cessation of this motive faculty is the suspension of the living powers, its total quiescence is death. Muscular motion.

By the continuance of patient, well directed researches, it is reasonable to expect much important evidence on this subject and, from the improved state of collateral branches of knowledge, together with the addition of new sources, and methods of investigation, it may not be unreasonable to hope for an ultimate solution of these phenomena, no less complete, and consistent, than that of any other desideratum in physical science.

The present attempt to forward such designs is limited to circumstances which are connected with muscular motion, considered as causes, or rather as a series of events, all of which contribute, more or less, as conveniencies, or essential requisites, to the phenomena; the details of muscular applications being distinct from the objects of this lecture.

No satisfactory explanation has yet been given of the state or changes which obtain in muscles during their contractions or relaxations, neither are their corresponding connections with the vascular, respiratory, and nervous systems, sufficiently traced. These subjects are therefore open for the present enquiry, and, although I may totally fail in this attempt to elucidate

Neither the changes in muscles during action, nor their connections with the other parts of the system have been explained.

cidate any one of the subjects proposed, nevertheless I shall not esteem my labour useless, or the time of the Royal Society altogether unprofitably consumed, if I succeed in pointing out the way to the future attainment of knowledge so deeply interesting to mankind.

The muscle itself is fibrous.

The muscular parts of animals are most frequently composed of many substances, in addition to those which are purely muscular. In this gross state, they constitute a flexible, compressible solid, whose texture is generally fibrous, the fibres being compacted into fasciculi, or bundles of various thickness. These fibres are elastic during the contracted state of muscles after death, being capable of extension to more than one-fifth of their length, and of returning again to their former state of contraction.

The enveloping membrane is elastic.

This elasticity, however, appears to belong to the enveloping reticular or cellular membrane, and it may be safely assumed that the intrinsic matter of muscle is not elastic.

The attraction of cohesion, in the parts of muscle, is strongest in the direction of the fibres, it being double that of the contrary, or transverse direction.

Muscles are irritable during life.

When muscles are capable of reiterated contractions and relaxations, they are said to be alive, or to possess irritability. This quality fits the organ for its functions. Irritability will be considered, throughout the present lecture, as a quality only.

They have less cohesion lengthways when dead.

When muscles have ceased to be irritable, their cohesive attraction in the direction of their fibres is diminished, but it remains unaltered in the transverse direction.

Experiment in proof.

The hinder limbs of a frog attached to the pelvis being stripped of the skin, one of them was immersed in water at 115° of Fahrenheit, during two minutes, when it ceased to be irritable. The thigh bones were broken in the middle, without injuring the muscles, and a scale affixed to the ancle of each limb: a tape passed between the thighs was employed to suspend the apparatus. Weights were gradually introduced into each scale, until, with five pounds avoirdupois, the dead thigh was ruptured across the fleshy bellies of its muscles.

repeated.

The irritable thigh sustained six pounds weight avoirdupois, and was ruptured in the same manner. This experiment was repeated on other frogs, where one limb had been killed by a watery solution of opium, and on another where essential oil of of cherry laurel * was employed: in each experiment, the irritable limb sustained a weight one-sixth heavier than the dead limb.

The same doctrine confirmed by effects in the living subject.

It may be remarked, in confirmation of these experiments, that when muscles act more powerfully, or more rapidly, than is equal to the strength of the sustaining parts, they do not usually rupture their fleshy fibres, but break their tendons, or even an intervening bone, as in the instances of ruptured tendo achillis, and fractured patella. Instances have however occurred, wherein the fleshy bellies of muscles have been lacerated by spasmodic actions; as in tetanus the recti abdominis have been torn asunder, and the gastrocnemii in cramps; but in those examples it seems that either the antagonists produce the effect, or the over-excited parts tear the less excited in the same muscle. From whence it may be inferred, that the attraction of cohesion in the matter of muscle is considerably greater during the act of contracting, than during the passive state of tone, or irritable quiescence, a fact which has been always assumed by anatomists from the determinate forces which muscles exert.

Differences observeable in the colour, texture, &c. of muscular parts.

The muscular parts of different classes of animals vary in colour and texture, and not unfrequently those variations occur in the same individual.

The muscles of fishes and vermes are often colourless, those of the mammalia and birds being always red: the amphibia, the accipenser, and squalus genera, have frequently both red and colourless muscles in the same animal.

Some birds, as the black game †, have the external pectoral muscles of a deep red colour, whilst the internal are pale.

In texture, the fasciculi vary in thickness, and the reticular membrane is in some parts coarse, and in others delicate: the heart is always compacted together by a delicate reticular membrane, and the external glutæi by a coarser species.

Origin of muscle in the egg. Punctum saliens

An example of the origin of muscle is presented in the history of the incubated egg, but whether the rudiments of the punctum saliens be part of the cicatricula organised by the parent, or a structure resulting from the first process of incubation, may be doubtful: the little evidence to be obtained on this point seems in favour of the former opinion; a regular

* Distilled oil from the leaves of the *Prunus Laurocerasus*.

† *Tetrao tetrix*. Lin.

confirmation of which would improve the knowledge of animal generation by ſhewing that it is gemmiferous. There are ſufficient analogies of this kind in nature, if reaſoning from analogies were proper for the preſent occaſion.

The punctum ſaliens, during its firſt actions, is not encompaſſed by any fibres diſcoverable with microſcopes, and the vaſcular ſyſtem is not then evolved, the blood flowing forwards and backwards, in the ſame veſſels. The commencement of life in animals of complex ſtructure is, from the preceding fact, like the ultimate organization of the ſimpler claſſes.

Muſcles of birds are formed out of the albumen, with a ſmall portion of vitellus and atmoſ. fluid,

It is obvious that the muſcles of birds are formed out of the albumen ovi, the vitellus, and the atmoſpheric air, acted upon by a certain temperature. The albumen of a bird's egg is wholly conſumed during incubation, and the vitellus little diminiſhed, proving that the albumen contains the principal elementary materials of the animal thus generated; and it follows that the muſcular parts, which conſtitute the greater proportion of ſuch animals when hatched, are made out of the albumen, a ſmall portion of the vitellus, and certain elements, or ſmall quantities of the whole compound of the atmoſphere.

and they do not differ from thoſe of the mammalia.

The muſcles of birds are not different, in any reſpect, from thoſe of quadrupeds of the claſs of mammalia.

The anatomical ſtructure of muſcular fibres is generally complex, as thoſe fibres are connected with membrane, blood-veſſels, nerves, and lymphæducts; which ſeem to be only appendages of convenience to the eſſential matter of muſcle.

Muſcular fibre is cylindric; membranous without, and pulpy within.

A muſcular fibre, duly prepared by waſhing away the adhering extraneous ſubſtances, and expoſed to view in a powerful microſcope, is undoubtedly a ſolid cylinder, the covering of which is reticular membrane, and the contained part a pulpy ſubſtance irregularly granulated, and of little coheſive power when dead.

The ultimate fibres.

A difficulty has often ſubſiſted among anatomiſts concerning the ultimate fibres of muſcles; and, becauſe of their tenuity, ſome perſons have conſidered them infinitely diviſible, a poſition which may be contradicted at any time, by an hour's labour at the microſcope.

Arteries.

The arteries arboreſce copiouſly upon the reticular coat of the muſcular fibre, and in warm-blooded animals theſe veſſels

are

are of fufficient capacity to admit the red particles of blood, but the intrinfic matter of mufcle, contained within the ultimate cylinder, has no red particles.

The arteries of mufcles anaftomofe with correfponding veins; but this courfe of a continuous canal cannot be fuppofed to act in a direct manner upon the matter of mufcle.

The capillary arteries terminating in the mufcular fibre muft alone effect all the changes of increafe in the bulk, or number, of fibres, in the replenifhment of exhaufted materials, and in the repair of injuries; fome of thefe neceffities may be fuppofed to be continually operating, It is well known, that the circulation of the blood is not effential to mufcular action; fo that the mode of diftribution of the blood veffels, and the differences in their fize, or number, as applied to mufcles, can only be adaptations to fome fpecial convenience.

Vafcularity is not infinite, but perceptibly limited.

Another prevalent opinion among anatomifts, is the infinite extenfion of vafcularity, which is contradicted in a direct manner by comparative refearches. The feveral parts of a quadruped are fenfibly more or lefs vafcular, and of different contextures; and, admitting that the varied diameter of the blood veffels difpofed in each fpecies of fubftance, were to be conftituted by the grofs fenfible differences of their larger veffels only, yet, if the ultimate veffels were in all cafes equally numerous, then the fole remaining caufe of diffimilarity would be in the compacting of the veffels. The vafa vaforum of the larger trunks furnifh no reafon, excepting that of a loofe analogy, for the fuppofition of vafa vaforum extended without limits. Moreover, the circulating fluids of all animals are compofed of water, which gives them fluidity, and of animalized particles of defined configuration and bulk; it follows that the veffels through which fuch fluids are to pafs, muft be of fufficient capacity for the fize of the particles, and that fmaller veffels could only filtrate water devoid of fuch animal particles: a pofition repugnant to all the known facts of the circulation of blood, and the animal economy.

Capillary arteries.

The capillary arteries which terminate in the mufcular fibre, muft be fecretory veffels for depofiting the mufcular matter, the lymphæducts ferving to remove the fuperfluous extravafated watery fluids, and the decayed fubftances which are unfit for ufe.

 The

and lymphæducts.

The lymphæducts are not so numerous as the blood vessels, and certainly do not extend to every muscular fibre: they appear to receive their contained fluids from the interstitial spaces formed by the reticular or cellular membrane, and not from the projecting open ends of tubes, as is generally represented. This mode of receiving fluids out of a cellular structure, and conveying them into cylindrical vessels, is exemplified in the corpora cavernosa, and corpus spongiosum penis, where arterial blood is poured into cellular or reticular cavities, and from thence it passes into common veins by the gradual coarctation of the cellular canals.

In the common green turtle, the lacteal vessels universally arise from the loose cellular membrane, situated between the internal spongy coat of the intestines and the muscular coat. The cellular structure may be filled from the lacteals, or the lacteals from the cellular cavities. When injecting the smaller branches of the lymphæducts retrograde in an œdematous human leg, I saw, very distinctly, three orifices of these vessels terminating in the angles of the cells, into which the quicksilver trickled. The preparation is preserved, and a drawing of the appearance made at the time. It was also proved, by many experiments, that neither the lymphæducts, nor the veins, have any valves in their minute branches.

Nerves of voluntary muscles.

The nerves of voluntary muscles separate from the same bundles of fibrils with the nerves which are distributed in the skin, and other parts, for sensation; but a greater proportion of nerve is appropriated to the voluntary muscles, than to any other substances, the organs of the senses excepted.

Origin of the nerves of volition.

The nerves of volition all arise from the parts formed by the junction of the two great masses of the brain, called the Cerebrum and Cerebellum, and from the extension of that substance throughout the canal of the vertebræ. Another class of muscles, which are not subject to the will, are supplied by peculiar nerves; they are much smaller, in proportion to the bulk of the parts on which they are distributed, than those of the voluntary muscles; they contain less of the white opaque medullary substance than the other nerves, and unite their fibrils, forming numerous anastomoses with all the other nerves of the body, excepting those appropriated to the organs of the senses. There are enlargements at several of these junctions,

Muscles for involuntary motion.

junctions, called Ganglions, and which are compofed of a lefs proportion of the medullary fubftance, and their texture is firmer than that of ordinary nerves.

Ganglions.

The terminal extremities of nerves have been ufually confidered of unlimited extenfion; by accurate diffection however, and the aid of magnifying glaffes, the extreme fibrils of nerves are eafily traced as far as their fenfible properties, and their continuity extends. The fibrils ceafe to be fubdivided whilft perfectly vifible to the naked eye, in the voluntary mufcles of large animals, and the fpaces they occupy upon fuperficies where they feem to end, leave a remarkable excefs of parts unoccupied by thofe fibrils. The extreme fibrils of nerves lofe their opacity, the medullary fubftance appears foft and tranfparent, the enveloping membrane becomes pellucid, and the whole fibril is deftitute of the tenacity neceffary to preferve its own diftinctnefs; it feems to be diffufed and mingled with the fubftances in which it ends. Thus the ultimate terminations of nerves for volition, and ordinary fenfation, appear to be in the reticular membrane, the common covering of all the different fubftances in an animal body, and the connecting medium of all diffimilar parts.

The extremities of nerves are definite in their termination.

By this fimple difpofition, the medullary fubftance of nerve is fpread through all organized, fenfible, or motive parts, forming a continuity which is probably the occafion of fympathy. Peculiar nerves, fuch as the firft and fecond pairs, and the portio mollis of the feventh, terminate in an expanfe of medullary fubftance which combines with other parts and membranes, ftill keeping the fenfible excefs of the peculiar medullary matter.

Diffufion of nervous matter.

The peculiar fubftance of nerves muft in time become inefficient; and, as it is liable to injuries, the powers of reftoration, and repair, are extended to that material. The re-union of nerves after their divifion, and the reproduction after part of a nerve has been cut away, have been eftablifhed by decifive experiments. Whether there is any new medullary fubftance employed to fill up the break, and, if fo, whether the new fubftance be generated at the part, or protruded along the nervous theca from the brain, are points undetermined: the hiftory of the formation of a fœtus, the ftructure of certain monfters, and the organization of fimple animals, all feem to favour

Reftoration or repair of nerves.

favour the probability, that the medullary matter of nerves is formed at the parts where it is required, and not in the principal feat of the cerebral medulla.

Whether the matter of nerve be not extenfively mixed in all irritable parts.

This doctrine, clearly eftablifhed, would lead to the belief of a very extended commixture of this peculiar matter in all the fenfible and irritable parts of animals, leaving the nerves in their limited diftribution, the fimple office of conveying impreffions from the two fentient maffes with which their extremities are connected. The moft fimple animals in whom no vifible appearances of brain or nerves are to be found, and no fibrous arrangement of mufcles, may be confidered of this defcription: Mr. John Hunter appeared to have had fome incomplete notions upon this fubject, which may be gathered from his reprefentation of a materia vitæ in his Treatife on the Blood, &c. Perhaps it would be more proper to diftinguifh the peculiar matter of mufcle by fome fpecific term, fuch, for example, as materia contractilis.

Peculiar adaptation for the nerves of electric animals.

A particular adaptation for the nerves which fupply the electrical batteries of the torpedo, and gymnotus, is obfervable on the exit of each from the fkull; over which there is a firm cartilage acting as a yoke, with a mufcle affixed to it, for the obvious purpofe of compreffion: fo that a voluntary mufcle probably governs the operations of the battery.

The matter of the nerves, and brain, is very fimilar in all the different claffes of animals.

The external configuration of animals is not more varied than their internal ftructure.

Configuration and ftructure of the various claffes of animals.

The bulk of an animal, the limitation of its exiftence, the medium in which it lives, and the habits it is deftined to purfue, are each, and all of them, fo many indications of the complexity or fimplicity of their internal ftructure. It is notorious that the number of organs, and of members, is varied in all the different claffes of animals; the vafcular and nervous fyftems, the refpiratory, and digeftive organs, the parts for procreation, and the inftruments of motion, are feverally varied, and adapted to the condition of the fpecies. This modification of anatomical ftructure is extended in the loweft tribes of animals, until the body appears to be one homogeneous fubftance. The cavity for receiving the food is indifferently the internal, or external furface, for they may be inverted, and ftill continue

Very fimple animals.

tinue to digeſt food; the limbs or tantacula may be cut off, and they will be regenerated without apparent inconvenience to the individual: the whole animal is equally ſenſible, equally irritable, equally alive: its procreation is gemmiferous. Every part is pervaded by the nutritious juices, every part is acted upon by the reſpiratory influence, every part is equally capable of motion, and of altering its figure in all directions, whilſt neither blood-veſſels, nerves, nor muſcular fibres, are diſcoverable by any of the modes of inveſtigation hitherto inſtituted.

In all animal ſtructures deſign is evident.

From this abſtract animal (if ſuch a term may be admitted) up to the human frame, the variety of acceſſory parts, and of organs by which a complicated machinery is operated, exhibit infinite marks of deſign, and of accommodations to the purpoſes which fix the order of nature.

In the more complicated animals much of their materials are not alive;

In the more complicated animals, there are parts adapted for trivial conveniences, much of their materials not being alive, and the entire offices of ſome liable to be diſpenſed with. The water transfuſed throughout the interſticial ſpaces of the animal fabric, the combinations with lime in bones, ſhells, and teeth; the horns, hoofs, ſpines, hairs, feathers, and cuticular coverings, are all of them, or the principal parts of their ſubſtance, extra-vaſcular, inſenſible, and unalterable by the animal functions after they are completed. I have formed an opinion, grounded on extenſive obſervation, that many more parts of animal bodies may be conſidered as inanimate ſubſtances; even the reticular membrane itſelf ſeems to be of this claſs, and tendons, which may be the condenſed ſtate of it; but theſe particulars are foreign to the preſent occaſion.

ſo that animated matter may be connected with inanimate.

The deduction now to be made, and applied to the hiſtory of muſcular motion, is, that animated matter may be connected with inanimate; this is exemplified in the adheſions of the muſcles of multi-valve, and bi-valve ſhell fiſh, to the inorganic ſhell, the cancer Bernhardus to the dead ſhells of other animals, and in the tranſplantation of teeth. All of which, although ſomewhat contrary to received opinion, have certainly no degree of vaſcularity, or vital connection with the inhabitant; theſe ſhells being liable to tranſudations of cupreous ſalts and other poiſonous ſubſtances, whilſt the animal remains uninjured. A variety of proofs to the ſame effect

effect might be adduced, but it would be disrespectful to to this learned body to urge any farther illustrations on a subject so obvious.

Division of the parts of an animal destroys the conformation;

The effects of subdivision, or comminution of parts among the complicated organized bodies, is unlike that of mineral bodies: in the latter instance, the entire properties of the substance are retained, however extensive the subdivision; in the former substances, the comminution of parts destroys the essential texture and composition, by separating the gross arrangements of structure upon which their specific properties depend. From similar causes it seems to arise, that animals of minute bulk are necessarily of simple structure: size alone is not, however, the sole cause of their simple organization, because examples are sufficiently numerous wherein the animal attains considerable bulk, and is of simple structure, and *vice versâ*: but, in the former, the medium in which they live, and the habits they assume, are such as do not require extensive appendages, whilst the smaller complex animals are destined to more difficult, and more active exertions. It may be assumed however, as an invariable position, that the minutest animals are all of simple organization.

but less the more simple the structure.

Life may on a small scale be supported with simple materials; bulky animals require variety of organs.

Upon a small scale, life may be carried on with simple materials; but the management, and provisions for bulky animals, with numerous limbs, and variety of organs, and appendages of convenience, are not effected by simple apparatus; thus, the skeleton which gives a determinate figure to the species, supports its soft parts, and admits of a geometrical motion, is placed interiorly, where the bulk of the animal admits of the bones being sufficiently strong, and yet light enough for the moving powers; but the skeleton is placed externally, where the body is reduced below a certain magnitude, or where the movements of the animal are not to be of the floating kind: in which last case the bulk is not an absolute cause. The examples of testaceous vermes, and coleopterous, as well as most other insects, are universally known.

Thus large animals have their bones within them, smaller have them without.

Crystalline of the eye, muscular.

The opinion of the muscularity of the crystalline lens of the eye, so ingeniously urged by a learned member of this Society, is probably well founded; as the arrangement of radiating lines of the matter of muscle, from the centre to the circumference of the lens, and these compacted into angular masses, would produce specific alterations in its figure.

This

This rapid ſketch of the hiſtory of muſcular ſtructure has been obtruded before the Royal Society to introduce the principal experiments, and reaſonings which are to follow: they are not ordered with ſo much exactneſs as becomes a more deliberate eſſay, but the intention already ſtated, and the limits of a lecture are offered as the apology.

Temperature has an eſſential influence over the actions of muſcles, but it is not neceſſary that the ſame temperature ſhould ſubſiſt in all muſcles during their actions; neither is it eſſential that all the muſcular parts of the ſame animal ſhould be of uniform temperatures for the due performance of the motive functions. *Temperature of muſcles.*

It appears that all the claſſes of animals are endowed with ſome power of producing thermometrical heat, ſince it has been ſo eſtabliſhed in the amphibia, pices, vermes, and inſecta, by Mr. John Hunter; a fact which has been verified to my own experience; the term "cold-blooded" is therefore only relative. The ratio of this power is not, however, in theſe examples, ſufficient to preſerve their equable temperature in cold climates, ſo that they yield to the changes of the atmoſphere, or the medium in which they reſide, and moſt of them become torpid, approaching to the degree of freezing water. Even the mammalia, and aves, poſſeſs only a power of reſiſting certain limited degrees of cold; and their ſurfaces, as well as their limbs, being diſtant from the heart, and principal blood-veſſels, the muſcular parts ſo ſituated are ſubject to conſiderable variations in their temperature, the influence of which is known. *All animals do produce heat; but are alſo affected by external communication.*

In thoſe claſſes of animals which have little power of generating heat, there are remarkable differences in the ſtructure of their lungs, and in the compoſition of their blood, from the mammalia and aves. *The colder animals.*

Reſpiration is one of the known cauſes which influences the temperatures of animals: where theſe organs are extenſive, the reſpirations are performed at regular intervals, and are not governed by the will, the whole maſs of blood being expoſed to the atmoſphere in each circulation. In all ſuch animals living without the tropics, their temperature ranges above the ordinary heat of the atmoſphere, their blood contains more of the red particles than in the other claſſes, and their muſcular irritability ceaſes more rapidly after violent death, *Reſpiration: its organs in animals of higher temperature;*

The

and in the cold-blooded:

The respirations of the animals denominated "cold-blooded," are effected differently from those of high temperature; in some of them, as the amphibia of Linnæus, the lungs receive atmospheric air, which is arbitrarily retained in large cells, and not alternately, and frequently changed. The fishes, and the testaceous vermes, have lungs which expose their blood to water, but whether the water alone, or the atmospheric air mingled with it furnish the changes in the pulmonary blood, is not known.

in insects.

In most of the genera of insects, the lungs are oborescent tubes containing air, which, by these channels, is carried to every vascular part of the body. Some of the vermes of the simpler construction have no appearance of distinct organs, but the respiratory influence is nevertheless essential to their existence, and it seems to be effected on the surface of the whole body.

In all the colder animals, the blood contains a smaller proportion of the red colouring particles than in the mammalia, and aves; the red blood is limited to certain portions of the body, and many animals have none of the red particles.

Experiment: Cold-blooded animals were included in water over mercury. After some days they died; but no gas was emitted, nor was the water changed.

The following animals were put into separate glass vessels, each filled with a pound weight of distilled water, previously boiled to expel the air, and the vessels inverted into quicksilver; *viz.* one gold fish, one frog, two leeches, and one fresh-water muscle.* These animals were confined for several days, and exposed in the sun in the day-time, during the month of January, the temperature being from 43° to 48°, but no air bubbles were produced in the vessels, nor any sensible diminution of the water. The frog died on the third day, the fish on the fifth, the leeches on the eighth, and the fresh-water muscle on the thirteenth. This unsuccessful experiment was made with the hope of ascertaining the changes produced in water by the respiration of aquatic animals, but the water had not undergone any chemical alteration.

Hybernating animals can live under confined respiration.

Animals of the class mammalia which hybernate, and become torpid in the winter, have at all times a power of subsisting under a confined respiration, which would destroy other animals not having this peculiar habit. In all the hybernating mammalia there is a peculiar structure of the heart, and its principal veins; the superior cava divides into two trunks; the left, passing over the left auricle of the heart, opens into

Peculiarity of structure in the heart and its veins.

* *Mytilus Anatinus.*

the

the inferior part of the right auricle, near to the entrance of the vena cava inferior. The veins usually called azygos, accumulate into two trunks, which open each into the branch of the vena cava superior, on its own side of the thorax. The intercostal arteries and veins in these animals are unusually large.

Their habit and circumstances that accompany the torpid state, &c.

This tribe of quadrupeds have the habit of rolling up their bodies into the form of a ball during ordinary sleep, and they invariably assume the same attitude when in the torpid state: the limbs are all folded into the hollow made by the bending of the body; the clavicles, or first ribs, and the sternum, are pressed against the fore part of the neck, so as to interrupt the flow of blood which supplies the head, and to compress the trachea: the abdominal viscera, and the hinder limbs are pushed against the diaphragm, so as to interrupt its motions, and to impede the flow of blood through the large vessels which penetrate it, and the longitudinal extension of the cavity of the thorax is entirely obstructed. Thus a confined circulation of the blood is carried on through the heart, probably adapted to the last weak actions of life, and to its gradual recommencement.

The manner in which that state commences and is afterwards terminated.

This diminished respiration is the first step into the state of torpidity; a deep sleep accompanies it; respiration then ceases altogether; the animal temperature is totally destroyed, coldness and insensibility take place, and finally the heart concludes its motions, and the muscles cease to be irritable. It is worthy of remark that a confined air, and a confined respiration, ever precede these phenomena: the animal retires from the open atmosphere, his mouth and nostrils are brought into contact with his chest, and enveloped in fur; the limbs become rigid, but the blood never coagulates during the dormant state. On being roused, the animal yawns, the respirations are fluttering, the heart acts slowly and irregularly, he begins to stretch out his limbs, and proceeds in quest of food. During this dormancy, the animal may be frozen, without the destruction of the muscular irritability, and this always happens to the garden snail*, and to the chrysalides of many insects during the winter of this climate.

(The conclusion in our next.)

* *Helix nemoralis.*

Description

VIII.

Deſcription of a Boring Tube, in general Uſe in America; but leſs known in this Country.

Deſcription of the American borer.

FIG. 2. *Plate* X. exhibits a very ſimple and ingenious borer, conſiſting of the common center bit of the carpenters followed by a wide flat thread ſcrew, hammered up from a plate of iron or ſteel. It is ſaid that they are uſed to bore holes ſeveral feet in length, and the peculiar property poſſeſſed by this inſtrument is, that it clears the cutting without requiring to be drawn out, as is the caſe with the augur, the gimblet, and other ſimilar tools. I do not, however, think that it would have this effect in boring perpendicularly down to conſiderable depths; but for horizontal or ſlightly inclined holes, its effect muſt fully anſwer.

Popular explanation of its action.

It may not at firſt occur to the reader why the introduction of this tool into a hole which muſt contain the wood that formerly blocked it up, ſhould not be attended with ſome degree of impediment or jamming; but this difficulty will vaniſh, when it is conſidered that the cuttings are, partly by their weight, and partly by friction againſt the internal cylindrical ſurface, prevented from revolving along with the ſcrew. The conſequence is that they are preſſed againſt its thread, and ſlide along it towards the handle. And as this motion or ſhifting of the thread is quicker than the motion of boring, by which the whole tool is carried inwards, the cuttings muſt come out with a velocity nearly equal to the difference of theſe two motions.

IX.

Geographical and Topographical Improvements. By JOHN CHURCHMAN, *Eſq. M. Imp. Acad. of Sciences, Peterſburgh.**

Great value of topographical knowledge.

IT appears to be a matter of much importance to the people of any country, at all times, whether in war or peace, to poſ-

* From the Tranſ. of the Soc. of Arts for 1804, who voted the ſilver medal to him for the ſame.

ſeſs

ſeſs a complete knowledge of its ſurface. In war, ſuch knowledge is abſolutely neceſſary for defence; in peace, for improving the country to the beſt advantage.

Now, ſince geography may be improved, an eaſy and accurate method to lay down maps of mountainous countries and hilly eſtates, will perhaps prove uſeful, as it will ſhow at a ſingle view the true ſhape and comparative height of the ground without the art of painting.

Utility of a correct delineation of hills, &c.

As mountains are apt to eclipſe each other, a perſpective view is ſeldom very extenſive, the rules of which fall ſhort of giving an accurate idea of any hilly country; becauſe ſuch a view, though ſtrictly true in one particular place, is not ſo in any other. The altitudes of mountains appear in proportion to the diſtance from the eye, and no rule in geometry has been found ſufficient to determine diſtances from any ſingle ſtation. Neither can a bird's-eye view of an eſtate aſcertain the depth of valleys or the height of mountains. But the method here propoſed will be found equally capable of giving the true ſhape of any ground above or below water. It may be ſucceſsfully applied to ſea charts, and will prevent much confuſion, ariſing from the tedious method of diſtinguiſhing ſoundings by a multitude of figures.

Mountains cannot be uſefully ſhewn in perſpective.

Explanation.

Suppoſe a full deſcription is required of any iſland in the ocean. Firſt, let an accurate map be laid down in the common way; and let the perpendicular height between the higheſt point of land and the ocean be divided into any number of equal parts. Suppoſe theſe equal diviſions are 100, 200, 300, 400 feet above the low-water mark. From the different points of theſe ſeveral diviſions, let horizontal lines be run with a good theodolite, and ſpirit level annexed, all round the iſland. If the work is well done, each line will end where it began; and if the bearings and diſtances of theſe ſeveral lines are truly laid down on the map, the crooked courſes of them will clearly ſhow the ſhape of the ground over which they paſs. For example: if any horizontal line paſſes by the ſide of a ſteep hill, it will incline towards the ocean, or approach the next horizontal line below it. When the ſame line croſſes a ſtream of running water or a valley, it will naturally bend up the ſide of the ſaid ſtream, until it can croſs it without loſing the

New method. Make a good map or plan. Mark the point of higheſt elevation and other points differing in elevation by equal meaſures. Run lines with the theodolite and level from theſe points; in which if the ſurvey be good, they will again terminate. Theſe lines laid on the map will ſhew the figure of the country.

the level; or, in other words, it will bend towards the centre of the iſland. Hence, by a little practice, the ſhape of the ſeveral horizontal lines on the map will give as clear an idea to the mind, of the ſhape of any country over which they paſs, as a ſight of the country itſelf can convey to the eye. But to obtain a mathematical and true knowledge of the altitude and declivity of any part of the country, we have the following propoſition:

Trigonometrical rule for the declivities.

As the perpendicular height of any one horizontal line above another is to the radius: ſo is the horizontal diſtance between the horizontal lines meaſured on the map at any particular place: to the co-tangent of declivity at that place.

Note.---If the horizontal diſtance between any two horizontal lines on the map is equal to the perpendicular height of any horizontal line above another, the angle of altitude, or declivity, of any hill will be 45 degrees.

Advantages;

The preſent improvement, which I believe to be entirely new, will be found to poſſeſs the following advantages:

—to military men,

1ſt. Military men are well acquainted with the many advantages always to be gained from the exact repreſentation of high grounds. By this method, we are able to give the angle of altitude, the angle of declivity, and perpendicular height of every hill; likewiſe the comparative height of different hills, the beſt route by which the high grounds may be gradually aſcended, and where heavy burthens can be drawn up with moſt eaſe.

—and for domeſtic and economical purpoſes.

2dly. Experience has ſufficiently ſhown, that the inhabitants of low grounds are ſubject to different kinds of ſickneſs, from which thoſe living at places elevated to a certain degree are exempt. A map on this improved plan will point out the moſt proper ſituation for building dwelling-houſes. It will be uſeful in botany, in diſcovering or cultivating ſome kinds of plants which flouriſh beſt at particular diſtances above the level of the ocean. It will trace the line of vegetation on the ſides of lofty mountains, whoſe tops are covered with eternal ſnow.

—to direct agricultural improvements,

3dly. Some high lands are known to produce good grain, while low lands afford graſs more abundantly; but moſt grounds produce good graſs, over which a moderate quantity of running water is conveyed. A plan of any country in this way will ſhow all the ground that can be irrigated; where water-

water-works may be erected; where navigable canals may be cut; and where high-ways and rail-roads may be laid out on the beſt and moſt level ground.

4thly. The ſubterraneous treaſures of the mineral and foſſil kingdoms are generally found in ſtrata; and if they are not truly horizontal, they make a certain angle with the horizon. A map on this projection may enable the mineralogiſt to follow any one ſtratum, at places even far diſtant from each other. —and mine reſearches.

Problem.

To find the true declivity of any piece of ground, in any map laid down on the principles of the preſent plan. Examples computed.

Example 1ſt. for D. *ſee Plate* IV.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 4 feet -	60206
	10.60206
To the co-tangent of the declivity, 45° -	10.00000

Example 2d. for B.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 8 feet -	90309
	10.90309
To the co-tangent of the declivity, 26° 34′	10.30103

Example 3d. for C.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 18 feet	1.25527
	11.25527
To the co-tangent of the declivity, 12° 32′	10.65321

The annexed ſurvey, *Plate* IX. of a ſmall lake and artificial mountain in the garden of his Excellency Count de Strogonoff, near St. Peterſburgh, has been cloſed by the tables of the difference of latitude and departure, as follows: A ſurvey according to this method.

N 30 E-

		N	S	E	W
N 30 E	$2\frac{1}{2}$	2.2	—	1.2	—
N 35 E	2	1.6	—	1.1	—
N 75 E	2	.5	—	1.9	—
N 55 E	2	1.1	—	1.6	—
N 45 E	3	2.1	—	2.1	—
N 52 W	2	1.2	—	—	1.6
N 59 W	3	1.5	—	—	2.5
S 56 W	12	—	6.7	—	9.9
S 60 E	7	—	3.5	6.1	—
		10.2	10.2	14.0	14.0

X.

A Memoir on the refining of Lead in the large Way. Containing ſome Reflections on the Inconveniencies reſulting from Cupels made of Aſhes; with a new and œconomical Method of conſtructing thoſe Cupels. By CITIZEN DUHAMEL.*

Proceſs for refining lead. Cupels made of aſhes.

EVERY one knows that in order to effect the ſeparation of ſilver from lead, a proceſs called refining or cupellation is made uſe of, which is effected in a baſon called a cupel; and it is likewiſe known that this baſon is formed of the aſhes or incinerated remains of animal or vegetable ſubſtances, after depriving them by waſhing of what ſaline matter they may contain.

The great quantity of wood aſhes required for making theſe cupels, and the difficulty of procuring it, have long ago induced me to ſeek for a more ſimple and leſs expenſive means of forming theſe veſſels.

Early obſervation that the oxide of lead penetrates chemical veſſels, while ſilver, if preſent, remains metallic.

The early chemiſts having obſerved that lead becomes oxided and converted into what is called litharge, when it is exposed

* This memoir is tranſlated from the Memoirs of the French National Inſtitute, Vol. III. at the requeſt of a correſpondent.

poſed

poſed to fire with the contact of atmoſpheric air, while the ſilver it may contain, preſerves its metallic form; it only remained for them to contrive a method of ſeparating theſe two metals. They were led to this method by obſerving that the oxide of lead, in its ſtate of liquefaction eaſily penetrates the ſubſtances with which it may be in contact, particular bone aſhes, without deſtroying the figure of the veſſel made of that material. In fact there is no ſubſtance whatever which is better adapted to form the ſmall cupels for aſſaying.

For want of bone aſhes the aſhes of wood are uſed for cupels; they have many inconveniences.

The difficulty, and often the impoſſibility of procuring three or four buſhels of bone aſhes in Germany, for each time of refining, has led to the adoption of the aſhes of wood. But not to mention that theſe are of conſiderable price, and not always to be had, they preſent another inconvenience, by often riſing and floating on the ſurface of the lead. When this happens, the proceſs muſt fail; and it does happen as often as the aſhes are ill prepared, or the cupel not ſufficiently or irregularly beaten, or that the openings left for the evaporation of the humidity are not properly diſpoſed, or enough in number, or cloſed by a portion of the ſcoriæ upon which the floor is made to receive the aſhes. This floor ought to be conſtructed of the moſt porous bricks, in order that the water with which the aſhes muſt be wetted may penetrate them and evaporate into the bed of ſcoria, and eſcape through the opening at the bottom of the furnace.

The elaſticity of this aqueous vapour frequently cauſes exploſions, which not only diſorder the cupel, but even the maſonry of the furnace, if it be not properly conſtructed.

Cupelling of lead.

In order to aſcertain the proportion of ſilver in any quantity of lead, it is only neceſſary to paſs a few pennyweights into a ſmall cupel of bone aſhes placed under the muffle of an aſſayer's furnace; as the lead becomes oxided it is imbibed in the cupel, and at length the phenomenon of brightening takes place upon the metallic button. This appearance ſhews that all the lead is diſſipated, and the remaining ſilver in a pure ſtate.

Refining in the large way cannot be done by the aſſaying proceſs wherein the oxide is abſorbed.

In the large way of refining, the ſame object of ſeparating the ſilver from the lead is aimed at, but the lead is not intended to penetrate into the cupel, which in fact is impracticable. For the total abſorption of this metal would require a much greater quantity of aſhes, with the conſumption of a ten fold portion of time and fuel; besides which, the loss in recovering the lead by fusion of the cupel is very expensive and the product less considerable than in the common way. The oxide of lead obtained in this last method may be easily fused, and reduced if needful; but it is an article of value in the arts, and therefore very acceptable in the market in its state of oxide.

Vessels of clay used in smelting lead.

The lead ores and litharge may be fused as is done in England and Britanny, in a reverbatory furnace, of which the floor or bason is formed of moistened and rammed clay. These floors resist the action of fire as well as that of the oxide of lead during six or eight months constant work.

These are applicable to refining.

The durability of these basons of earth first gave me a notion of the method I shall propose for refining furnaces, in which the intention is to oxide the lead in order to obtain litharge, and not to cause it to be totally absorbed in the cupels, as is done when the metal is assayed, to shew how much silver it contains.

It would be preferable that no absorption took place.

In the operation upon a large scale the cupel, though of ashes absorbs only part of the lead, as I have already remarked, observing at the same time that it would be much more advantageous to obtain the whole converted into litharge, of which the reduction into lead is infinitely more easy than that of the oxide contained in the ashes, which resist fusion, and afford a scoria that always contains some metal.

English process. 12 tons of lead are oxided with scarcely any absorption.

Upon a cupel of ashes rammed into an oval ring of iron, about five feet in length and three and a half in width, the English refine in succession about twelve ton of lead, which becomes converted into fine merchantable litharge, with the exception of the small portion that penetrates the cupel, of which the thickness is less than three inches. This cupel is supported under the roof of the furnace by two bars of iron. The litharge is driven by the blast of bellows towards the anterior part of the furnace, whence it falls without interruption upon the area of the foundery; and at the same time to supply the space which this subtraction of oxide would leave, a pig of lead is gradually advanced into the interior of the furnace, placed on one side of the nozel of the bellows. This lead, by its gradual fusion, keeps the cupel full till towards the end of the operation.

 I have

I have given this ſketch of the Engliſh proceſs only to ſhew that it is poſſible to refine lead with very little expence of aſhes for forming the cupels. Thoſe here mentioned do not abſorb eighty pounds of oxide out of the large quantity of lead thus refined.

The veſſel is very cheap.

Hence we ſee that metallurgiſts have always endeavoured to obtain the greateſt poſſible quantity of litharge and the leaſt of aſhes containing oxide; but as they did not imagine they could depart from the docimaſtic proceſs, they have conſtantly made their cupels of aſhes.

We have ſeen that in the ſmall proceſs of cupellation the lead penetrates the aſhes as it becomes oxided, and that when no more lead remains the ſmall button of ſilver remains pure at the bottom in the ſpherical form. This operation is effected with more ſpeed becauſe the ſurface of the bath is always convex in theſe ſmall veſſels, and conſequently the litharge runs off on all ſides towards the edge of the cupel, where it is immediately imbibed.

The litharge runs off in the ſmall proceſs;

This is not the caſe in large cupels of ſeveral yards diameter. Bellows muſt be uſed, not only to accelerate the oxidation by their blaſt, but to drive the litharge towards the paſſage or gutter which is left for its iſſue.

—but cannot in the large.

The inconveniencies and even the impoſſibility of cauſing all the lead to penetrate the aſhes of theſe large cupels have been already remarked. This muſt be evident on reflecting that the oxidation can take place only at thoſe parts of the bath which are expoſed to the contact of the air and the blaſt of the bellows. The litharge near the middle of the baſon not being diſpoſed to flow towards the edge, would cover and defend the metal from any farther oxidation. Hence it is that the operators have found themſelves obliged to drive out the litharge by the mechanical action of a ſtream of air from bellows.

It muſt be driven off by bellows.

The oxidation therefore takes place only at the ſurface of the lead, and not lower; if it were otherwiſe, the aſhes of the cupel would be penetrated with oxide to a depth which would be more unequal the longer the operation laſted. Now I have always remarked that the portion of aſhes thus imbibed in the large refinery is not thicker towards the center of the baſon than towards its edge, though the lead remains thirty or forty times longer at the bottom than near the edges, becauſe the bath

The oxidation is effected only at the ſurface.

bath conſtantly diminiſhes till all the lead is reduced into litharge, and nothing at laſt remains but the button of ſilver at the bottom of the cupel.

Other reaſons why the large proceſs cannot be made by abſorption.

That the whole of the lead is abſorbed in the cupel of aſſay, ariſes from the unequal action of heat upon every part of the ſmall veſſel. As the cupel in the large way preſents only its ſuperior ſurface to the action of the heat, the oxide thus imbibed ceaſes to penetrate at the place where the temperature is no longer in a ſtate to hold the oxiding fuſion. For this reaſon it is that the whole of the cupel is throughout impregnated to an equal depth, and it is impoſſible to cauſe all the lead to penetrate the aſhes.

Litharge is ſought as a product in the large way.

From the preceding obſervations it will be eaſy to conclude that though the aſſay of lead muſt be made in ſmall cupels of bone aſhes, in order that the whole of the oxided metal may either be abſorbed or partly evaporated; yet the caſe is very different in the large operation, where the object is to proceed with celerity, and to obtain as much litharge as poſſible.

Addition of ſand to the wood aſhes, &c.

I have before ſtated that the wood aſhes uſed in forming large cupels are expenſive, and frequently not to be procured in ſufficient quantity; to which I have added their being ſubject to blow up or riſe entirely, which occaſions a conſiderable loſs. It muſt further be mentioned, that in order to give more weight and conſiſtence to theſe cupels it is often neceſſary to mix a conſiderable quantity of ſand with them, particularly if the lead ſhould contain foreign ſubſtances, ſuch as arſenic, cobalt, antimony, zinc, tin and other matters. If the lead be merely-arſenical, after having taken off the firſt ſcum, it is uſual to throw from time to time, on the whole ſurface of the bath, about 20lbs. of iron ſcales or granulated crude iron. This iron being lighter than the lead, floats on the top and abſorbs the arſenic, after which it is cleared away, and then the litharge is formed without any obſtacle. This method is uſed in Saxony.

The neceſſity of adding ſand to the aſhes of the cupels ought to have led to a diſcovery which I propoſe: it is as follows.

New Conſtruction of the Cupels or Baſons for refining Lead.

New conſtruction of the baſons or veſſels for refining lead.

Without making any change in the maſonry of the furnaces for refining by what we call the German method, it is only neceſſary to be careful to make a ſufficient number of vents in their

their baſe for the evaporation of the moiſture, and to diſpoſe of them ſo as moſt effectually to anſwer this purpoſe. Theſe channels or vents are to be covered with a bed of ſcoria, upon which a pavement is to be made of the moſt porous bricks, and of the thickneſs of a ſingle brick.

On this area or pavement, which ought to be concave like the baſe upon which the aſhes of the ordinary cupels are placed, muſt be laid a quantity of founder's ſand, a little moiſtened. If it be not adheſive enough, a little clay may be added, in order to give the requiſite ſolidity, and the whole carefully mixed. The ſand muſt be rammed down in the ſame manner as is done to conſolidate the aſhes in the uſual way, and a baſon for refining muſt be formed, equally rammed in all its parts. The thickneſs of this cupel ſhould be about ſix inches; and it may be made in two layers, as we ſhall hereafter obſerve.

The cupel is made of ſand with a little clay inſtead of bone aſhes.

After the baſon has been in all parts uniformly beaten or rammed down, it will be proper to ſift over the whole ſurface two or three quarts of wood aſhes, which may be made to adhere by ramming.

When the cupel is thus prepared, the head of the furnace muſt be lowered, and a moderate fire kept up for ſeveral hours, in order to evaporate part of the water from the ſand. The reſt will be driven out, without inconvenience, through the vents during the refining.

It may be dried before uſe;

After a ſufficient drying, which may even be diſpenſed with; the head is to be raiſed, the cupel, ſuffered to cool a little, and ſtraw or hay then laid upon it, and upon this the pigs or pieces of lead, which are to be gently put down, in order that their weight may not make impreſſions in the ſand. The ſtraw is uſed for this purpoſe, in our method as well as the common method; and it would be convenient that the lead ſhould be caſt in iron hemiſpherical moulds or pots inſtead of the priſmatic form, as theſe pieces would be leſs ſubject to damage the cupel.

—but this is not abſolutely neceſſary.

Method of charging with lead.

When the quantity of lead neceſſary to fill the cupel is arranged in the furnace, the head is to be lowered and luted all round with clay, after which the fire is to be applied as in the uſual proceſſes.

When the lead is in perfect fuſion, and the bath covered with droſs and coally matter from the ſtraw, this ſkum muſt be raked

Fuſion. Clearing of the ſurface.

raked off through the paſſage left for the litharge, by means of a wooden rake about a foot long, with an iron handle of ſufficient length to reach every part of the bath.

Method of blowing, &c.

When the lead has been ſeveral times ſkimmed, and begins to become red, the bellows muſt be ſet into action, gently at firſt, and afterwards more ſtrongly. Their nozles muſt be ſo diſpoſed that the blaſt may be directed towards the center of the bath, and in order that the wind may be urged upon the ſurface of the metal, each nozle muſt have a ſmall round plate of iron adapted to it. Theſe ſmall flaps or valves, called papillons (flies or butterflies) are uſed in the German fineries. They have an hinge at top, and at every ſtroke they riſe about half way from their perpendicular poſition towards the level, ſo that by reflecting the air downwards upon the lead, they haſten its oxidation.

Gutter for the litharge.

When all the droſs has been removed, and the lead is of a good red heat and covered with litharge, a little gutter muſt be made, with an hook appropriated for this purpoſe, in the ſand of the cupel. This muſt be carefully done until the bottom of the gutter anſwers to the level of the bath. The litharge driven by the blaſt of the bellows will flow out of this paſſage, and fall upon the hearth of the foundery.

Inſtructions.

When the operator perceives that only a ſmall quantity of litharge remains near the gutter, he will ſtop its eſcape with a ſmall quantity of moiſtened aſhes; but as ſoon as the lead ſhall be again covered with oxide, the paſſage muſt be opened and made deeper as the quantity of matter becomes depreſſed; taking care that no lead eſcapes, particularly towards the end of the operation, as it would carry along with it a large portion of ſilver which would be loſt.

In this manner the proceſs is to be carried on until the ſurface of the ſilver exhibits thoſe flaſhes which are called the brightening, taking care to raiſe the fire in proportion to the diminution of the bath, particularly towards the end, when the ſilver is collected; and as this metal is much more difficult to be kept in fuſion than its ſmall alloy of lead, the refining will be imperfect, unleſs the temperature be raiſed, and inſtead of about one twentieth of lead, which the ſilver uſually retains in the German method, it will remain much more highly charged. This would render it more difficult to be treated in the ſecond operation, called the ſilver refining, or by the Germans *ſilver brenen*, by which it is rendered pure.

Thofe who are accuftomed to refine lead in the German method, will find no difficulty in following mine. For though the cupel is made of fand inftead of afhes, there is no difference in the manipulations.

This method admits of keeping up the charge during work,

We have feen that the Englifh refine a great quantity of lead on a fmall cupel. The fame may be done in the method here defcribed, by adding in proportion as the lofs by oxidation takes place. Suppofing the capacity of the cupel to be fuch as to contain about five ton of lead, we might continue the operation to three times that quantity in a fingle procefs, which would not have the inconveniences of the Englifh method.

and the cupel may be repeatedly ufed.

I flatter myfelf that a well-made cupel of fand may be ufed for feveral refinings, without requiring to be made up again every time like thofe of afhes; but in thefe circumftances, and before the lead is put in, it is neceffary to fill with well beaten fand the gutter or opening which was before made for carrying off the litharge. Care muft be taken in doing this to remove with a chiffel that kind of glaze which the oxide of the lead leaves behind it. With this precaution, after wetting the part where the new moiftened fand is to be applied, they will firmly unite together.

From the long duration of the earthen floors of thofe reverberatory furnaces in which lead ores and even litharge are fufed, as was before mentioned, we have no reafon to apprehend any bad confequences from the oxide of lead, which acts only on the furface of the cupel, and penetrates to an inconfiderable depth.

Recovery of the abforbed lead.

After one or two refinings, this cruft of oxide may be taken off and fufed in a blaft furnace, in order to recover the lead; a procefs no lefs eafy than that of reducing the metal which exifts in much greater quantity in the ordinary cupels. We therefore obtain a larger quantity of litharge, which is one advantage, and in addition to this, the lofs in filver which accompanies the abforbed lead will be lefs. For in the fmall quantity of precious metal which accompanies the oxide, it is found by experiment, that the proportion is greater in the abforbed lead than in that which is driven over in litharge.

To make the cupel of clay inftead of fand would be more expenfive and lefs convenient.

Inftead of fand we might make ufe of clay in conftructing our cupels, as is done in the hearths of the reverbatory furnaces of Brittany; but it would then be neceffary to pound the

the earth repeatedly for ſeveral days, otherwiſe it would crack, and theſe cracks, which would become wider by the ſhrinking from heat, would afford a lodgment for ſome of the lead; an inconvenience which ſand, even if rather loamy, does not preſent. It muſt alſo be remarked, that a cupel of clay would become too hard to admit of the excavation for carrying off the litharge; ſo that this part at leaſt would require to be made of ſand or aſhes.

Two kinds of ſand.

It will be advantageous to uſe two kinds of ſand in forming a baſon of the cupel, the one fine ſuch as the founders' ſand, and the other coarſe. The latter may form the firſt ſtratum, which, after being well rammed with the implements uſed for this purpoſe, muſt be left about three inches in thickneſs. Upon this the fine and ſomewhat loamy ſand is to be ſpread and rammed like the firſt. A ſlight degree of moiſture muſt be uſed with both theſe, in order that they may more ſolidly adhere together. The lower ſtratum being more coarſe, will facilitate the eſcape of the humidity,

Repair or renewal of the cupel.

It will not be neceſſary to diſturb the lower ſtratum of ſand when a new cupel is to be made; and even of this laſt that portion which has not imbibed any oxide may be uſed along with the new ſand intended to be applied. The lower ſtratum muſt not be touched during this renewal, for fear of mixing coarſe ſand with the fine. This inconvenience may be guarded againſt by ramming upon the ſurface of the coarſe ſand a bed of a thin facing of aſhes, at which the operator muſt ſtop when he takes away the upper ſtratum.

We have remarked that the founders' ſand muſt be rather loamy, and that if it be not ſo, it will be neceſſary to add a ſmall quantity of clay to render it adheſive; but as it is neceſſary that this clay ſhould be equally diffuſed through the maſs, it may be diffuſed in the water with which the ſand is to be ſprinkled, and the whole muſt be carefully mixed.

Abſorption is of no advantage in the old proceſs.

It might be objected, that ſince the cupels of ſand do not abſorb ſo much litharge as thoſe of aſhes, more time will be required to complete the refining, becauſe the oxide inſtead of being in part abſorbed, muſt by this new proceſs be driven out of the furnace. This however is a ſubject which ought not to be conſidered as of any importance; for the blaſt of the bellows well directed will cauſe the oxide of litharge to flow out more abundantly through the gutter than if the abſorption took place.

I have

I have seen operators in Germany, who, when they constructed their cupels, had the precaution to form a small circular cavity in the middle, the depth of which was proportioned to the quantity of silver, which from the assay they knew to be contained in the lead of one operation. By this contrivance there were no insulated grains of the metal left on the surface of their cupel, but the whole of the silver formed a perfectly round cake in the middle of the center excavation. I would advise the same ingenious expedient to be used in the cupels of sand. Improvement. Cavity to receive the silver.

I am well assured that the cupels I propose, if made with care and attention, will succeed perfectly, and that, independent of their convenience beyond the others, they will be found very economical. I am desirous that, for the advantage of metallurgy, this method should be generally used, and its benefits will prove that we ought not always to follow with servility the established usages nor the common working processes. Conclusion.

XI.

Letter from Mr. James Stodart, *explaining the Method of gilding upon Steel by Immersion in a Liquid, which has lately engaged the public Attention in various Articles of Manufacture.*

To Mr. NICHOLSON.

Dear Sir,

A Considerable degree of public interest and curiosity has lately been excited by the exhibition of instruments of steel coated or gilt with gold. The discovery, although not altogether new, does not appear to be very generally known; and as its application to various manufactures promises to be both elegant and useful, I have, with a view of saving some time and expence to others who may be inclined to make further experiments, added a short account of a method which with me has succeeded perfectly well. I wish here in justice to observe, that this discovery of the method belongs more to my friend Mr. Hume, chemist, Long-Acre, than to myself. With that gentleman's kind assistance I had but few difficulties to Instruments of steel gilt with gold.

Account of the process. Three parts sulphuric ether are added to one of solution of gold. The gold is taken up by the ether.

to overcome. The following is our method: To a saturated solution of gold in nitro-muriatic acid, add about three times the quantity of pure sulphuric ether: Agitate them together for a short time. The gold will soon be taken up by the ether in the form of a muriate, or nitro-muriate of gold, leaving the remaining acid colourless at the bottom of the vessel, which must now be drawn off by means of a stop-cock, or other similar contrivance. The acid being discharged, the instrument to be gilt having been previously well polished and wiped very clean, is to be dipped for an instant into the ethereal solution, and on withdrawing it, as instantly washed by agitation in clean water. This is essential to get clear of a small portion of acid necessarily taken up with the metal; and if this be neatly done, the surface of the steel will be completely and very beautifully covered with gold. Some little degree of dexterity is required to perform the whole operation well.

The instrument to be dipped into the ethereal solution.

Essential oils do not succeed well.

I have tried some of the essential oils, knowing that they will take the gold from nitro-muriatic acid; but as far as I went they did not apply for the purpose of gilding: and as I had found all I wanted in ether, I certainly did not prosecute the other experiments with much industry.

I remain, with much respect,

Dear Sir,

Your obliged servant,

J. STODART.

Strand, June 24, 1805.

XII.

On the peculiar Noise emitted by Water before it acquires the Temperature of boiling; which is commonly denoted by the Word Simmering. W. N.

Introduction.

SOME time ago a philosophical friend who favoured me with a visit, mentioned in conversation that the simmering of water before it boils had formed the subject of enquiry between himself and other curious examiners of natural appearances, as being a fact not yet well explained. It seemed to me very remarkable, as I dare say it will to my readers, that any of of the facts relating to the boiling of water, concerning which so much has been said and written, should still remain in obscurity. Having, myself, been in the habit of considering it as the consequence of a rapid escape of interspersed air from the heated water, I requested he would say why he thought the phenomenon repugnant to that supposition. My friend proceeded in reply to state, that when water is first put on the fire, in a metallic vessel, the vessel itself gradually becomes lined with bubbles; that these bubbles become detached and rise, so that the whole surface, or at least the bottom, becomes clear again; that, soon afterwards a rattling noise is heard, sharp and metallic, which encreases in loudness until it almost perfectly resembles the pouring of small shot into the vessel; that the fluid continues in a state of tranquillity and transparency during this state; and lastly, that when the noise is loudest the state of ebullition suddenly comes on, the peculiar noise of simmering ceases all at once, and nothing is heard but the soft and moderate noise of aquatic agitation as long as the boiling lasts.

Particular description of the act of simmering.

This account, so remarkable for its precision and accuracy, shewed clearly that my notion, which I believe is the common opinion, was ill founded. After a little meditation, it appeared evident to me that the noise of simmering must arise from the collapsion of steam bubbles, formed at the bottom of the vessel, and condensed almost instantly upon their ascent in the fluid not yet heated to the boiling point. In support of this opinion I shewed him a common experiment with the water-hammer. This instrument, which is made and sold by the glass-blowers and barometer-makers, consists of a tube, nearly a foot in length and about three quarters of an inch in diameter, terminating in a globe of about two inches in diameter; the other end of the tube being closed. The outer extremity of the globe ends in a capillary tube, through which as much water has been introduced as rather more than fills the globe itself. This water has been boiled in the vessel or instrument, and at the time of boiling, when all the internal cavity not containing water was filled with steam to the almost total exclusion of air, the capillary aperture was hermetically closed. The instrument thus completed is found when cold to contain water and a space nearly vacuous, and the experiment from which it derives its name of

It seems to arise from the sudden production and condensation of steam.

Description of the water-hammer.

Singular noise of water agitated in vacuo.

of the water-hammer, is that of agitating its contents. The remarkable effect it exhibits is, that the parts of the water strike against the glass and against each other, with the sharp noise usually produced by the collision of hard bodies. Another experiment, which I alluded to, is that, if the ball be held downwards so as to become filled with the water, and it be then slowly raised up, so as to bring the tube nearer to the horizontal position, the heat of the hand which holds the tube will produce vapour or steam strong enough to prevent the fluid from running down to its level from the upper surface of the globe; but a little farther inclination causes it to descend, and a bubble of the steam enters the globe itself and ascends through the fluid. But it scarcely arrives at the top before it is condensed, and the water collapses with a smart stroke or noise, so as to fill the globe again. The succession of these bubbles and their condensation take place so rapidly, in a well made instrument, that ten or twelve collapsions occur in every second of time. The fact, and the obvious remarks I made upon it, convinced my friend that I had suggested the proper explanation.

Another experiment of steam produced and condensed twelve times in a second,

applied to explain the effect of simmering.

Objections.

It was my intention however to have heated some water in a vessel in order to observe and ascertain the progressive appearances, but I had not done it when I again had the pleasure to meet this intelligent observer. He complimented me upon the ingenuity of my solution, but having himself since repeated the experiment of boiling water, it seemed from his report that ingenuity was all the value it could claim. "Take a bright tin vessel" said he "and heat water in it; you will hear the noise, but no bubbles are to be seen."

I took the earliest opportunity of making some experiments, the particulars and results of which are as follow:

Exp. 1. Water was heated in a glass retort. The simmering was attended with steam bubbles.

Exp. 1. A small glass retort, the body of which is about two inches in its shortest or horizontal diameter, was suspended so that its neck was elevated about twenty degrees above the level. Water was then poured in to fill the body and the greatest part of the neck. My intention in filling the neck was, that I might be able to observe whether any greater or more sudden rise took place before the period of boiling than the well known expansion of the fluid. A small spirit-lamp was placed beneath the bulb. The coldness of the water in the vessel immediately condensed a portion of the water which issued

iſſued from the flame itſelf, and formed drops on the outſide. As the included water became heated this condenſed water evaporated, and left the ſurface again clear; and at this period the diſſeminated air began to ſeparate and gave a duſty appearance to the inner ſurface, which laſted about three minutes. At the end of this time the inner ſurface began to clear; the peculiar noiſe of ſimmering was heard; and bubbles were ſeen ſuddenly appearing and collapſing; the retort itſelf being agitated and the ſurface of the water riſing and falling by ſtarts. The bubbles were pointed at top, ſomewhat reſembling ſmall flames ſuddenly appearing and vaniſhing at different parts of the ſurface. In the courſe of one minute they grew larger and larger and collapſed at greater heights, until at length they eſcaped through the fluid without being condenſed. This was the inſtant of ebullition or boiling and at this period the noiſe of ſimmering ceaſed and that of boiling was heard.

Exp. 2. Water was heated in a larger glaſs veſſel. The effects were rather more evident.

Exp. 2. As this effect appears to ariſe decidedly from the upper water being colder than that near the bottom of the veſſel, it was natural to infer that the appearances would be different according to its figure and magnitude. I therefore took a bolt-head, or ſpherical glaſs body, with a ſtraight neck: Its diameter was four inches; and when it was filled, a column of water eight inches long ſtood in its upright neck. The thickneſs of the glaſs at its bottom was conſiderable. At 35 minutes after three the lamp was lighted. At 40 minutes bubbles of gas roſe ſingly, and very little of the duſty appearance was ſeen. At 58 minutes the noiſe of ſimmering began, and the collapſing bubbles were plentiful and diſtinct. Little ſtreams or fountains of ſteam roſe from particular points and were condenſed; and ſome globes of half an inch diameter aſcended clear of the bottom and collapſed in the fluid above. At one minute after four the bubbles reached the top of the fluid without collapſing, and at this time the noiſe of ſimmering ceaſed. The lamp was then blown out.

Exp. 4. The experiment was repeated in a copper veſſel.

Exp. 4. A bright copper hemiſphere, four inches in diameter, was filled with water at the temperature of 60 degrees. At four hours three minutes the lamp was lighted and water became condenſed on the outſide. At five minutes the inſide ſurface had a duſty appearance, from bubbles of air immediately over the flame. The temperature was then 110°, and the

the outſide dry. At ſix minutes bubbles of gas or air were detached and roſe, the temperature being 126°, and the vapour of ſteam being viſible from the ſurface of the water. At eight minutes the inſide ſurface was coated with large bubbles or beads of air; temperature 150°. At nine minutes, temp. 165°, much vapour. At 9½ min. temp. 175°, the bottom was clear of bubbles, and the noiſe of ſimmering began. At 10 min. temp. 184°, the ſteam collapſing bubbles were very evident, though, from their pointed ſhape, they were not immediately obvious to an obſerver looking ſtraight down into the ſhining veſſel. At 11 min. temp. 180°, noiſe very loud and bubbles more and larger. At 12 min. temp. 185°, ſome of the bubbles broke at the ſurface, and the noiſe was leſs. At 12¼ min. temp. 204°, the water boiled, and the ſimmering noiſe was ſucceeded by that of boiling. At 14 min. the lamp was extinguiſhed.

Exp. 5. Water already heated does not ſimmer ſo much or ſo loudly as water quickly raiſed from a low temperature.

Exp. 5. The water was ſuffered to cool to 170°, and the lamp was then lighted again, namely, at four hours 18 minutes. At 20 min. temperature 180°, the ſimmering began; but not till after the ſteam bubbles were ſeen very large: and ſoon afterwards, at 204°, they roſe through the fluid, and the boiling took place by fountains or ſtreams of bubbles riſing from particular points. The ſimmering noiſe in this experiment was much leſs than before.

The thermometer was placed horizontally, with the greateſt part of its tube, and part of its bulb, out of the water. When the bulb was plunged in the water, it ſhewed 208°.

Concluſion.

From theſe facts it appears to be clearly eſtabliſhed, that the cauſe firſt mentioned, namely, the condenſation of ſteam bubbles in their aſcent through the cold fluid above, is the occaſion of the noiſe of ſimmering. In the fifth experiment the ſuperincumbent water was hotter than that beneath, and conſequently the ſimmering could not be produced but by ſteam at a higher temperature, and even then the collapſion of the water was leſs ſudden and the noiſe leſs loud.

Experiments

XIII.

Experiments on Wootz. By Mr. DAVID MUSHET. *From the Philoſophical Tranſactions,* 1805.

THE following experiments were made at the requeſt of Sir Joſeph Banks, on five cakes of wootz, with which he ſupplied me for that purpoſe. As the cakes, which were numbered 1, 2, 3, 4, 5, were not all of the ſame quality, it will be proper firſt to deſcribe the differences obſervable in their external form and appearance. Experiments on five cakes of wootz.

No. 1 was a denſe ſolid cake, without any flaw or fungous appearance upon the flat, or, what I ſuppoſe to be, the upper ſide. The round or under ſurface was covered with ſmall pits or hollows, two of which were of conſiderable depth; one through which the ſlit or cut had run, and another nearly as large towards the edge of the cake. Theſe depreſſions, the effects, as I ſuppoſe, of a ſpecies of cryſtallization in cooling, were continued round the edges, and even approached a little way upon the upper ſurface of the wootz. Deſcription of the cake, No. 1.

The cake was a quarter of an inch thicker at one extremity of the diameter than at the other, from which I infer, that the pot or crucible, in which this cake had been made, had not occupied the furnace in a vertical poſition. Its convexity, compared to that of the other five, was ſecond. Upon breaking the thin fin of ſteel, which connects the half cakes together, I found it to poſſeſs a very ſmall denſe white grain. This appearance never takes place but with ſteel of the beſt quality, and is leſs frequent in very high ſteel, though the quality be otherwiſe good. Fractured very ſteely.

Upon examining the break with attention, I perceived ſeveral laminæ and minute cells filled with ruſt, which in working are never expected to unite or ſhut together. The grain otherwiſe was uniformly regular in point of colour and ſize, and poſſeſſed a favourable appearance of ſteel.

No. 2. This cake had two very different aſpects; one ſide was denſe and regular, the other hollow, ſpongy, and protuberant. The under ſurface was more uniformly honey-combed than No. 1; the convexity in the middle was greater, but towards the edges, particularly on one ſide, it became flatter. The grain expoſed by breaking was larger, bluer in colour, and Deſcription of the cake of wootz, No. 2.

and more ſparkling than No. 1. In breaking, the fracture tore but ſlightly out, and diſplayed the ſame unconnected laminæ with ruſty ſurfaces, as was obſerved in No. 1. Beſide theſe, two thin fins of malleable iron projected from the unſound ſide, and ſeemed incorporated with the maſs of ſteel throughout. Towards the centre of the break, and near to the excreſcence common to all the cakes, groups of malleable grains were diſtinctly viſible. The ſame appearance, though in a ſlighter degree, manifeſted itſelf in various places throughout the break.

Cake of wootz, No. 3.

No. 3. The upper ſurface of this cake contained ſeveral deep pits, which ſeemed to reſult from the want of proper fluidity in fuſion. They differed materially from thoſe deſcribed upon the convex ſides of No. 1 and 2, and were of that kind that would materially effect the ſteel in forging.

The under or convex ſide of this cake preſented a few cryſtalline depreſſions, and thoſe very ſmall; the convexity was greater than that of No. 1 and 2, the fracture of the fin almoſt ſmooth, and only in one place exhibited a ſmall degree of tenacity in the act of parting. In the middle of the break, about half an inch of ſoft ſteel was evident; and in different ſpots throughout numerous groups of malleable grains, and thin laminæ of ſoft blue tough iron made their appearance.

Cake of wootz, No. 4.

No. 4. Was a thick denſe cake poſſeſſed of the greateſt convexity; the depreſſions upon the under ſide were neither ſo large, nor ſo numerous as thoſe in No. 1 and 2, nor did they approach the upper ſurface of the cake further than the acute edge. This ſurface had the moſt evident marks of hammering to depreſs the feeder, or fungous part of the metal, which in the manufacturing ſeems the gate or orifice by which the metal deſcends in the act of gravitation.

The break of this cake, however favourable as to external appearance, was far from being ſolid. Towards the feeder it ſeemed looſe and crumbly, and much oxidated. The grain divided itſelf into two diſtinct ſtrata, one of a denſe whitiſh colour, the other large and bluiſh, containing a number of ſmall ſpecks of great brilliancy. Several irregular lines of malleable iron pervaded the maſs in various places, which indicated a compound too heterogeneous for good ſteel.

Cake of wootz, No. 5.

5th cake. This was materially different in appearance from any of the former. It had received but little hammering, yet

was

was ſmooth and free from depreſſions, or honey-comb on both ſurfaces. The feeder, inſtead of being an excreſcence, preſented a deep concave beautifully cryſtallized.

In breaking, the fracture tore out conſiderably, but preſented a very irregular quality of grain. That towards the under ſurface was ſmall and uniform, but towards the flat or upper ſurface it increaſed in ſize, and in the blueneſs of its colour, till it paſſed into the ſtate of malleable iron.

The break of this ſteel, though apparently ſoft, was the leaſt homogeneous of the whole, and throughout it preſented a very brilliant arrangement of cryſtal, which in other ſteel is always viewed with ſuſpicion.

General Remark.

Uniformly the grain and denſity of the wootz are homogeneous, and free from malleable iron towards the under or round ſurface; but always the reverſe towards the feeder or upper ſide.

Remarks in Forging.

The appearances on forging the cake of wootz, No. 1.

No. 1. One-half of the cake was heated ſlowly by an annealing heat to a deep red, and put under a ſharp broad-mouthed chiſſel with a ſmall degree of taper. It cut with difficulty, was reheated, and cracked a little towards one end of the ſlit or cut originally in the cake.

The heat in this trial was ſo moderate, that I was afraid that the crack had ariſen from a want of tenacity, occaſioned by the heat being too low.

The other half was heated a few ſhades higher, and ſubjected to the ſame mode of cutting; before the chiſſel had half way reached the bottom, the piece parted in two in the direction of the depreſſion made by the cutting inſtrument. The additional heat in this inſtance proved an injury, while the cracking of the ſteel in both caſes, particularly the former, was a certain proof of the abundance, or rather of the exceſs of the ſteely principle.

The fractures of both half cakes, now obtained for a ſecond time, were materially different from that obtained by the ſimple diviſion of the cake. The grain was nearly uniform, diſtinctly marked, but of too gray a colour for ſerviceable ſteel. Two of the quarters being drawn into neat bars under hand hammers

The appearances on forging the cake of wootz, No. 1.

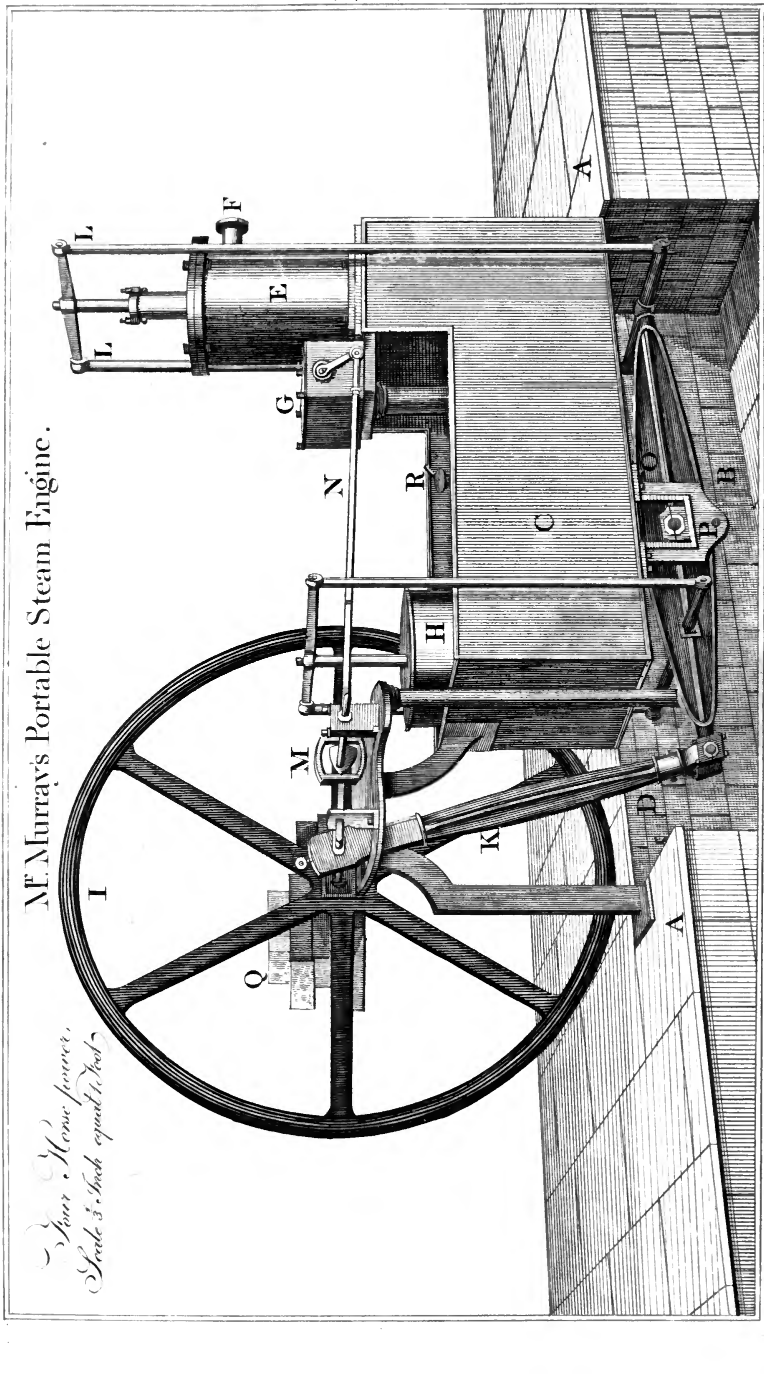
mers at a low heat, one of them contained a number of cracks and fissures. The fracture was gray, tore out a little in breaking, but was otherwise yolky and excessively dense. A small bar of penknife size was improved greatly in drawing down, and had only one crack in thirteen inches of length. The grain and fracture were both highly improved by this additional labour; the tenacity of the steel was greater, and it stood firmly under the hammer at a bright red heat.

The other two quarters of this cake were squared a little, and successively put under a tilt hammer, of two hundred weight, going at the rate of three hundred blows per minute, and drawn into small penknife size. One of the bars from an outside piece, always the most solid, was entirely free from cracks, and had only one small scale running upon one side.

These bars exhibited a tougher break, than those drawn by hand; the colour was whiter, and the grain possessed a more regular and silky appearance.

(To be continued.)

*** *I have received a letter from Mr.* Boswell, *in which he expresses an opinion, that it is unfair in the Old Correspondent whose letter appeared in our last Number to have applied Mr. B's apology to one particular part of his paper; as he conceives, that it ought to have been considered as indicating the spirit in which his whole communication was written. I have inserted this short notice out of respect to the writer; but have declined inserting the letter itself, because the controversy can have no farther importance to the readers of this Journal, after the subject itself has been exhausted.*


Mr. Murray's Portable Steam Engine.
Four Horse power.
Scale 3/4 Inch equal 1 Foot.
A
B
C
D
E
F
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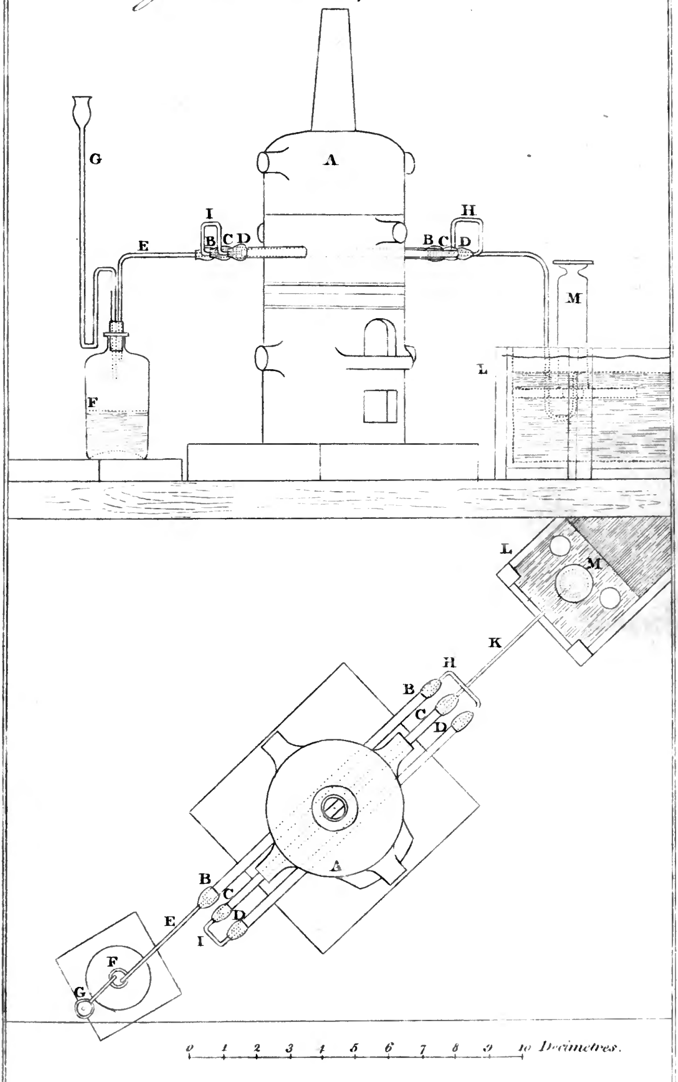
C. Livesey sc.

Apparatus of Baruel for making the Gaseous Oxide of Carbon.



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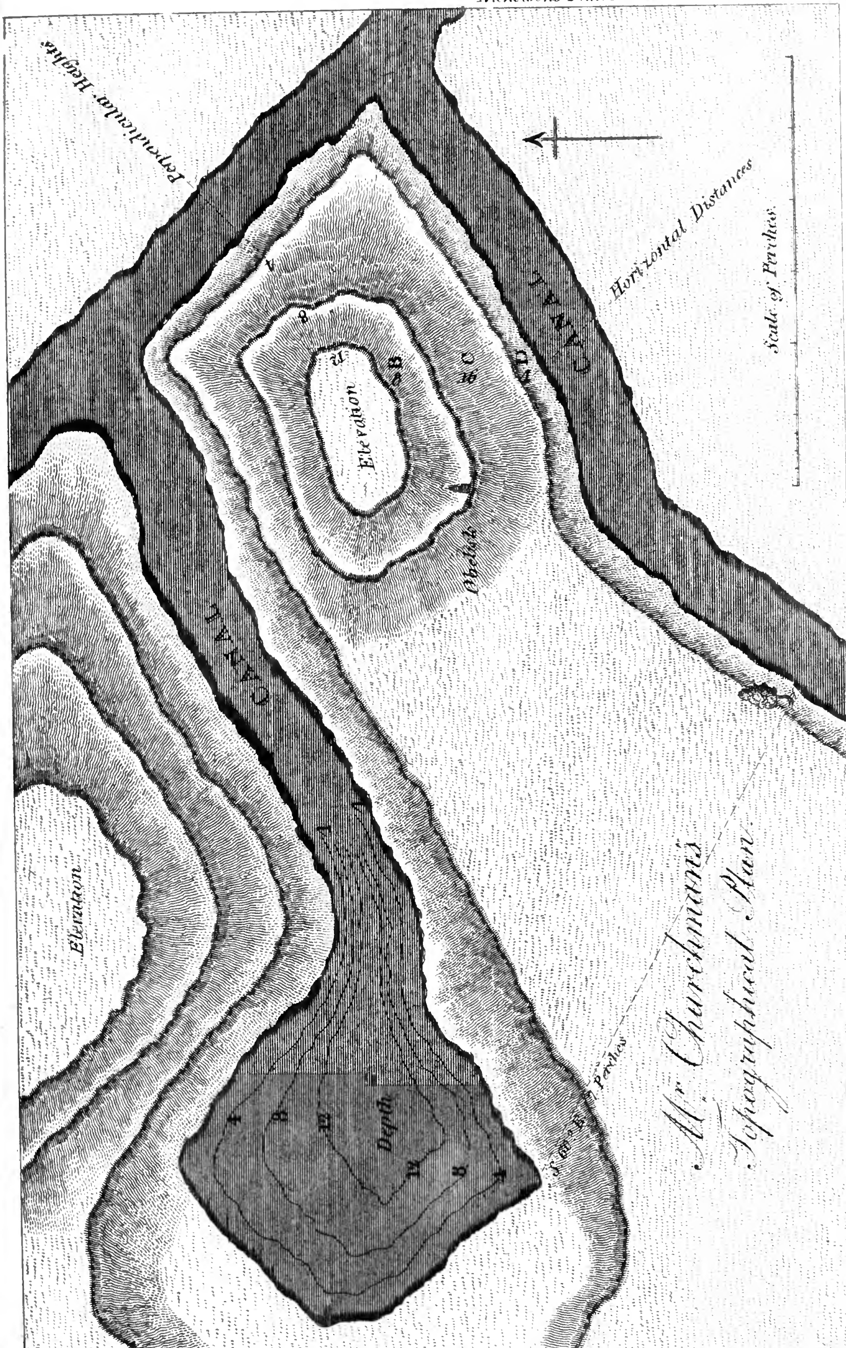
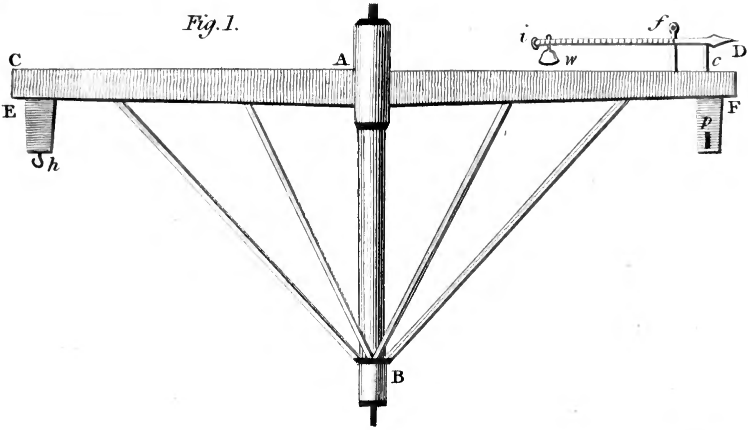


Mutlow Sc. Russell Ct.

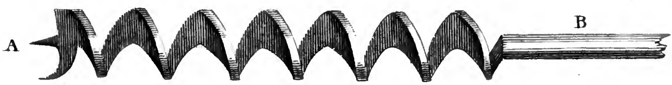
Mr. Gregory's Contrivance to determine Horse-power in Mills.

Fig. 1.



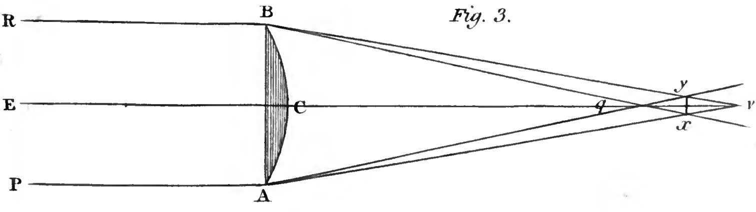
American Borer.

Fig. 2.

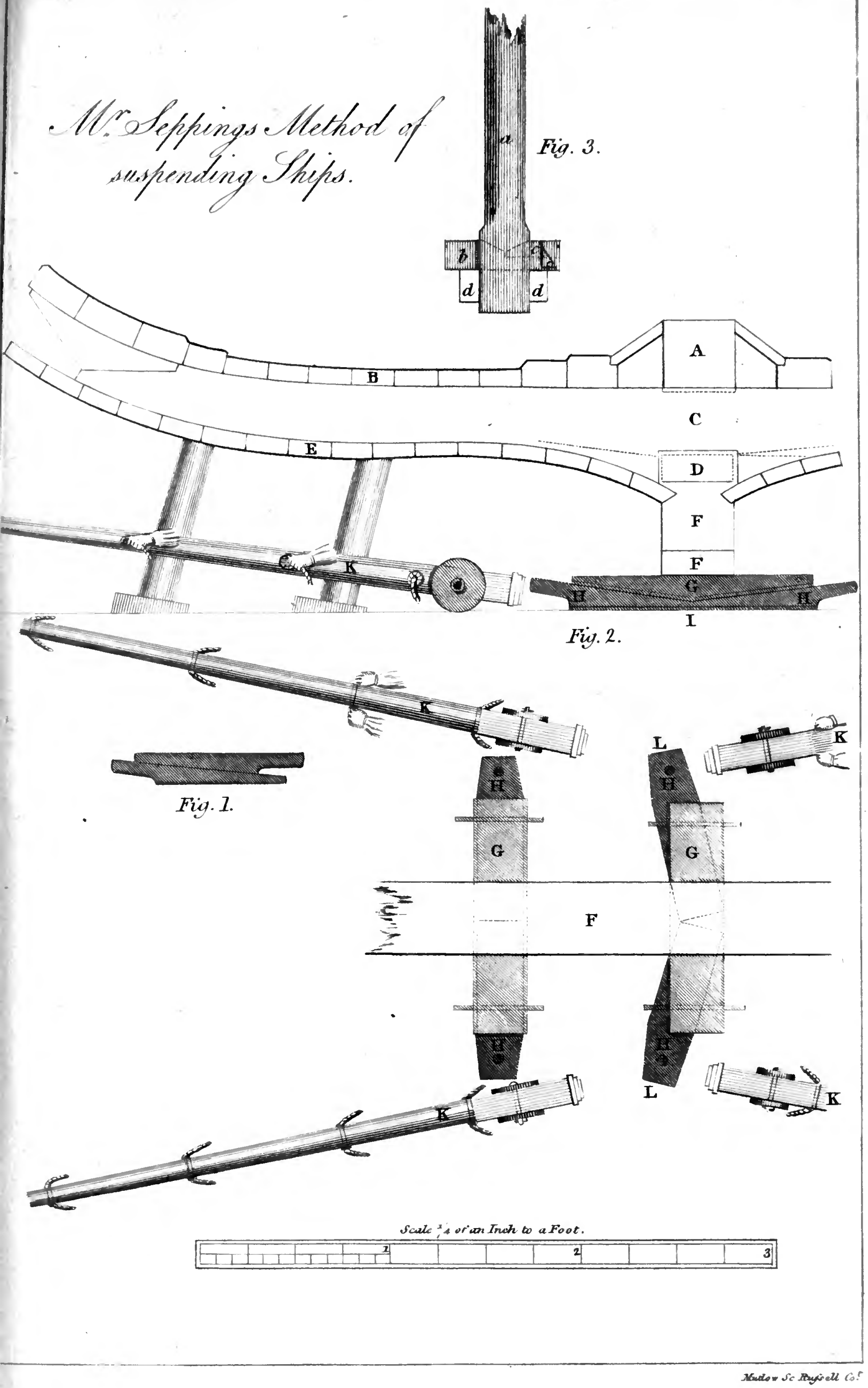


Mr. Ez. Walker on Light.

Fig. 3.



Mutlow sc. Russell Ct.


Mr. Seppings Method of suspending Ships.
Fig. 3.
a
b
c
d
d
A
B
C
D
E
F
F
G
H
H
I
K
Fig. 2.
Fig. 1.
K
K
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L
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H
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G
F
H
H
Scale 3/4 of an Inch to a Foot.
1
2
3

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1805.

ARTICLE I.

An Account of ſome new Experiments which prove that the Temperature at which the Denſity of Water is a Maximum, is ſeveral Degrees of the Thermometer above the freezing Point. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c. Received July* 16, 1805, *from the Author; with a Letter dated Munich, 25th June,* 1805.

IN my ſeventh eſſay, in which I have treated of the propagation of heat in fluids, and alſo in a paper publiſhed in the Philoſophical Tranſactions for the Year 1804, Part I.; in which I have given an account of a curious phenomenon frequently obſerved on the Glaciers of Chamouny, I have aſcribed the melting of ice which is placed (by deſign, or by accident) below the ſurface of ice-cold water, to currents of warmer water, which, in certain caſes, are ſuppoſed to deſcend in that ice-cold liquid: but as this ſuppoſed fact has lately been called in queſtion by ſeveral perſons, and as the explanations I have founded on it muſt fall to the ground, unleſs it can be ſupported, I have been induced to re-conſider the matter, and to give it a careful and thorough inveſtigation.

Phenomena which have been explained from the maximum denſity of water being at higher than freezing temperature:

the fact queſtioned.

It was first announced by De Luc.

The fundamental fact on which the supposition is grounded, which was announced many years ago by Mr. De Luc; namely, that the temperature at which the density of water is a maximum, is considerably higher than that at which that fluid freezes, is indeed so very extraordinary, and appears to be the cause of so many interesting phenomena, that too much pains cannot be taken to put it beyond doubt.

New method of proving it.

As the methods hitherto used for determining that important point have, by some at least, been considered as insufficient, I shall take the liberty to propose another, by which the fact in question may, I think, be demonstrated directly; and without any nice calculations, or any very difficult or delicate experiments.

Let the following experiments, (which it will be easy to repeat) speak for themselves.

Apparatus. In the middle of a thin cylindrical brass vessel, a thin brass cup is supported.

Having provided a cylindrical vessel, (A Fig. 1. Plate XII.) open above, made of thin sheet brass, which is $5\frac{1}{2}$ inches in diameter, and four inches deep, supported on three strong legs, $1\frac{1}{4}$ inches high; I placed in it a thin brass cup, (B) two inches in diameter at its bottom, (which is a little convex downwards) $2\frac{8}{10}$ inches wide at its brim; and $1\frac{3}{10}$ inches deep; which cup stands on three spreading legs made of strong brass wire, and of such form and length, that when the cup is introduced into the cylindrical vessel, it remains firmly fixed in the axis of it, and in such a situation, that the bottom of the cup is elevated just $1\frac{1}{4}$ inches above the bottom of the cylindrical vessel.

In the middle of the brass cup is supported

In the middle of this cup there stands a vertical tube of thin sheet brass, $\frac{1}{2}$ an inch in diameter, and $\frac{6}{10}$ of an inch in length, open above, which serves as a support for another smaller cup (C) which is made of cork; the brim of which is on the same horizontal level with the brim of the larger brass cup, in which it is placed.

an hemispherical cup of cork,

This cork cup, which is spherical, (being something less than half of an hollow sphere) is one inch in diameter at its brim, measured within $\frac{4}{10}$ of an inch deep, and $\frac{1}{4}$ of an inch in thickness. It is firmly attached to the vertical tube on which it stands, by means of a cylindrical foot, $\frac{1}{2}$ an inch in diameter, and $\frac{1}{4}$ of an inch high; which, when some force is employed, enters into the opening of the vertical tube.

On

On one ſide of this cork cup there is a ſmall opening, which receives, and in which is confined the lower extremity of the tube of a ſmall mercurial thermometer, (D). The bulb of this thermometer, which is ſpherical, is juſt $\frac{3}{10}$ of an inch in diameter, and it is ſo fixed in the middle of the cup, that its centre is juſt $\frac{1}{4}$ of an inch above the bottom of the cup; conſequently it does not touch the cup any where, nor does any part of it project above the level of its brim.

having in its center the bulb of

The tube of this thermometer, which is ſix inches in length, has an elbow near its lower end, at the diſtance of one inch from its bulb, which elbow forms an angle of about 110 degrees, and the thermometer is ſo fixed in the cup, that the ſhort branch of its tube, namely, that to the end of which the bulb is attached, lies in an horizontal poſition, while the longer branch (to which a ſcale, made of ivory, and graduated according to Fahrenheit is affixed,) projects obliquely upwards and outwards, in ſuch a manner that the freezing point of the ſcale lies juſt above the level of the top of the cylindrical veſſel in which the cups are placed.

a thermometer.

The cork cup, which was turned in the lathe, is neatly formed, and in order to cloſe the pores of the cork, it was covered, within and without, with a thin coating of melted wax; which was poliſhed after the wax was cold.

Other particulars.

The thermometer was fixed to the cork cup by means of wax, and in doing this, care was taken to preſerve the regular form of the cup, both within and without.

The vertical braſs tube which ſupports this cup in the axis of the braſs cup, is pierced with many holes, in order to allow a free paſſage into it, and through its ſides, to the water employed in the experiments.

Having attached about ſix ounces of lead to each of the legs of the braſs cup, in order to render it the more ſteady in its place, it was now introduced with its contents, into the cylindrical veſſel, and the veſſel was placed in an earthen baſin (E) ſeven inches in diameter below, 11 inches in diameter at its brim, and five inches deep, and was ſurrounded on all ſides with pounded ice.

This apparatus was placed in a pan, and ſurrounded with ice.

Several flat cakes of ſolid ice were now put into the cylindrical veſſel, and faſtened down upon its bottom, and under the bottom of the braſs cup, and a circular row of other long pieces of ice were placed, in a vertical poſition, round the outſide

Ice was placed and ſecured in the cylindrical veſſel; but not in the cups, and ice-cold water was poured into all the remaining ſpace.

outſide of the brim of the braſs cup, between it and the vertical ſide of the cylindrical veſſel, which vertical pieces of ice reached upwards to within about $\frac{1}{10}$ of an inch of the top or brim of the cylindrical veſſel; and when this was done, ice-cold water was poured into this veſſel till the ſurface of that cold fluid ſtood juſt one inch above the level of the brim of the cork cup.—Both cups were of courſe ſubmerged and filled with ice-cold water, and ſurrounded on all ſides by ſolid cakes of ice.

The cups remained thus ſubmerged for one hour.

After things had remained in this ſituation more than an hour, during which time the cold water in the cylindrical veſſel, and that in the cups, was frequently agitated with the ſoft end of a ſtrong feather, and the cups and the water in every part of the veſſel appearing to be exactly at the temperature of freezing: I proceeded to make the following deciſive experiment.

Experiment. No. 1.

Experiment I. A cone of metal at 42° was plunged in the ice-cold water, juſt above the cork cup:

A ſolid ball (F) of tin having been provided, two inches in diameter, with a cylindrical projection on the lower ſide of it, one inch in diameter, and half an inch long, ending in a conical point, which projected (downwards) half an inch farther; this ball (to which was fixed a ſtrong iron wire ſix inches in length, which ſerved as a handle to it,) having been made to acquire the temperature of 42° F. by keeping it immerſed near half an hour in a large quantity of water at that temperature, was placed as expeditiouſly as poſſible over the middle of the cork cup, and in ſuch a ſituation that the whole of the deſcending conical point (half an inch in length) was immerſed in the ice-cold water in the cylindrical veſſel, the extremity of that point being juſt half an inch perpendicular above the upper ſide of the bulb of the thermometer which lay in the cork cup.

It was foreſeen, that if the water in contact with the cone became denſer by the heat, it would deſcend and raiſe the thermometer.

I knew that the particles of ice-cold water which were thus brought into contact with the conical point, could not fail to acquire ſome ſmall degree of heat from that relatively warm metal; and I concluded, that if the particles of water ſo warmed ſhould in fact become *heavier* than they were before, in conſequence of this ſmall increaſe of temperature, they muſt neceſſarily *deſcend* in the ſurrounding lighter ice-cold liquid, and as the heated metallic point was placed directly

directly over the cork cup, and fixed immoveably in that ſituation, I foreſaw that the deſcending current of warm water muſt neceſſarily fall into that cup, and at length fill it, and that the preſence of this warm water in the cup would be announced by the riſing of the thermometer.

It did, in fact, raiſe the thermometer in the cup;

The reſult of this very intereſting experiment was juſt what I expected: the conical metallic point had not been in contact with the ice-cold water more than 20 ſeconds, when the mercury in the thermometer began to riſe, and in three minutes it had riſen three degrees and a half, namely, from 32° to 35½°, when five minutes had elapſed, it had riſen to 36°, when an end was put to the experiment.

but it did not affect a thermometer near the ſurface of the water;

Another ſmall thermometer, placed juſt below the ſurface of the ice-cold water, and only $\frac{2}{10}$ of an inch from the upper part of the conical point, on one ſide of it, did not appear to be ſenſibly affected by the vicinity of that warm body.

nor another near the outſide of the cup.

A third thermometer, the bulb of which was placed in the braſs cup, juſt on the outſide of the cork cup, and on a level with its brim, ſhewed that the water which immediately ſurrounded the cork cup, remained conſtantly at the temperature of freezing, during the whole time that the experiment laſted.

The metal at 42° could not the water to the ſame temp.

As I well knew, from the reſults of the experiments on the propagation of heat in a ſolid bar of metal, of which an account has been given in a memoir preſented to the firſt claſs of the National Inſtitute of France, on the 7th of May 1804, that the ice-cold water in this experiment could not poſſibly acquire from a contact with the conical metallic point, a temperature ſo high as that of 42°, I was by no means ſurpriſed to find that the thermometer belonging to the cork cup roſe no higher.

A ſomewhat greater heat propoſed to be applied.

In order to ſee if it could not be made to riſe not only higher, but alſo more rapidly, by employing the metallic ball heated to ſuch a temperature as it might by ſuppoſed would be ſufficient to heat thoſe particles of ice-cold water which ſhould come into contact with its conical point, to the temperature at which the denſity of water is ſuppoſed to be a maximum, I made the following experiments.

Experiment. No. 2.

Experiment 2. The former experiment was

Having removed the ball, I gently bruſhed away the warm water, which, in the laſt experiment had been lodged in the

cavity

repeated; the cone being heated to 60°.

cavity of the cork cup, (and which ftill remained there, as was evident from the indication of the thermometer belonging to the cup,) I placed feveral fmall cakes of ice in the cylindrical veffel, which ice floating on the furface of the water in the veffel, prevented that water from receiving heat from the air of the atmofphere, which at that time was at the temperature of 76° F. And as the cork cup had been a little heated by the warm water in the foregoing experiment, time was now given it to cool.

As foon as the cup, and the whole mafs of the water in the cylindrical veffel appeared to have acquired the temperature of freezing, I carefully removed the cakes of ice which floated on the furface of the water, and introduced once more the projecting conical point belonging to the metallic ball into the ice-cold water in the veffel, placing it exactly in the fame place which it had occupied in the foregoing experiment; but this ball, inftead of being at the temperature of 42° F. as before, was now at the temperature of 60° F.

The effect was more rapid and more confiderable.

The refult of this experiment was very ftriking, and if I am not much miftaken, affords a direct, unexceptionable, and demonftrative proof, not only that the maximum of the denfity of water is in fact at a temperature which is feveral degrees above the point of freezing; but alfo, that warm currents do actually fet downwards in ice-cold water, whenever a certain fmall degree of heat is applied to the particles of that fluid which are at its furface.

Particulars,

The conical metallic point had been in its place no more than ten feconds when I diftinctly faw that the mercury in the thermometer belonging to the cork cup was in motion; and, when 50 feconds had elapfed, it has rifen four degrees, viz. from 32° to 36°.

When two minutes and a half had elapfed, (reckoning from the moment when the metallic point was introduced into the cold water,) the thermometer had rifen to 39°, and at the end of fix minutes to $39\frac{7}{8}$°, when it began to fall; but very flowly however, for at the end of eight minutes and a half it was at $39\frac{3}{4}$°.

A thermometer near the outfide of the cup was not affected.

A fmall mercurial thermometer, the bulb of which was placed on one fide of the cork cup, at the diftance of about $\frac{2}{10}$ of an inch from it, fhewed no figns of being in the leaft affected by the heat communicated to the ice-cold water by the metallic ball.

This

This experiment was repeated three times the ſame day, (the 13th of June 1805,) and always with nearly the ſame reſults.

The mean reſults of theſe four experiments were as follows:

Tabulated reſults of the experiments repeated, with the the cone at a low heat.

Time elapſed reckoned from the beginning of the experiment. Min.	Sec.		Temperature of the water in the cork cup, as ſhewn by the thermometer.
0	0	- - - - - - -	32°
At 0	10	began to riſe - - - -	32+
At 0	23	had riſen to - - - -	33
0	28	- - - - - - -	34
0	35	- - - - - - -	35
0	48	- - - - - - -	36
1	3	- - - - - - -	37
1	35	- - - - - - -	38
2	32	- - - - - - -	39
3	41	- - - - - - -	$39\frac{1}{2}$
4	48	- - - - - - -	$39\frac{3}{4}$
6	5	- - - - - - -	$39\frac{7}{8}$

As I had found by ſome of my experiments made in the year 1797, of which an account is given in my ſeventh eſſay, part I. that water at the temperature of about 42° F. and conſequently what we ſhould call very cold, melted conſiderably more ice, when ſtanding on it, than an equal quantity of boiling hot water, in the ſame ſituation, I was very curious to ſee whether the thermometer, the bulb of which lay in the cork cup, would not alſo be leſs heated by the ball when it ſhould be applied *very hot,* to the ſurface of the water, than when its temperature was much lower.

Whether a much greater heat in the metal would affect the water in the cup leſs.

To determine that point I made the following experiment,

Experiment. No. 3.

The cylindrical veſſel with its contents having been once more reduced to the uniform temperature of freezing water, the metallic ball was heated in boiling water, and being as expeditiouſly as poſſible taken out of that hot liquid, its projecting conical point was ſuddenly ſubmerged in the ice-cold water, as in the former experiments.

Experiment 3. The experiment repeated with the cone heated to 212°.

The

The refult of this experiment was very interefting, and it feems to me to throw much light on the fubject of thefe inveftigations.

Effect on the thermometer in the cup much lefs.

It was not till 50 feconds had elapfed that the thermometer began to fhew any figns of rifing, and at the end of one minute and feven feconds it had only rifen two degrees.

In the foregoing experiments, when the metallic ball was fo much colder, the thermometer began to rife in ten feconds, and at the end of one minute and three feconds it had rifen five degrees.

This difference is very remarkable; and if it does not prove the exiftence, and great efficacy of currents, in conveying heat in fluids, I muft confefs that I do not fee how the exiftence of any invifible mechanical operation, the progrefs of which does not immediately fall under the cognizance of our fenfes, can ever be demonftrated

As the experiment made with the ball heated in boiling water appeared to me to be very interefting, I repeated it twice, and its refults were always nearly the fame.

Tabulated refults of experiments repeated; with the cone at a confiderable heat.

The mean refults of thefe three experiments were as follows:

Time elapfed, reckoned from the beginning of the experiment.	Min.	Sec.		Temperature of the water in the cork cup, as fhewn by the thermometer.
	0	0	- - - - - - - -	32°
At	0	50	the thermometer begun to rife.	32+
At	1	2	had rifen to - - - - -	33
	1	7	- - - - - - - -	34
	1	18	- - - - - - - -	35
	2	2	- - - - - - - -	36
	3	2	- - - - - - - -	36½
	4	17	- - - - - - - -	37
	6	12	- - - - - - - -	38
	7	17	- - - - - - - -	38⅛
	9	0	- - - - - - - -	38¼
	12	0	- - - - - - - -	38¼
	14	0	- - - - - - - -	38¼

Comparative view of the experiments.

By comparing the mean refults of thefe experiments with the mean refults of thofe in which the ball was at the temperature

rature of 60°, we may fee how much more rapidly the thermometer in the cork cup acquired heat when the metallic ball was *relatively cold* than when it was at the temperature of boiling water; and it is more than probable that it was not till after the conical metallic point had been confiderably cooled, by a contact with the ice-cold water, that thofe ftreams of moderately warmed water began to defcend from it, by which the thermometer was at length heated.

When the very hot cone was ufed, a thermometer at the furface was heated; —but a lower one was not.

In the experiments made with the ball heated in boiling water, a fmall thermometer, placed juft under the furface of the ice-cold water, on one fide of the metallic point, began to rife very rapidly as foon as the hot ball was fixed in its place; but another thermometer placed about half an inch lower, on one fide of the cork cup, remained to all appearance at perfect reft, from the beginning of the experiment to the end of it.

Explanation:

The explanation of all thefe appearances is fo eafy that it would be a wafte of time to fay much on that fubject. It may however be ufeful to recapitulate the principal phenomena, and fhew in what manner they tend to eftablifh the facts which they are brought to prove.

That the flightly heated water defcends;

Every body muft fee, at the firft glance, that in all thefe experiments the heat which caufed the thermometer to rife was *carried* down into the cork cup by defcending currents of warm water; and it is evident that water which defcends muft of neceffity be fpecifically heavier than that in which it defcends.

—and the greateft denfity is rather below 40°.

From the refults of thefe experiments we may conclude that the denfity of water is a maximum when that fluid is at a temperature fomewhat lower than that of the *fortieth degree* of Fahrenheit's thermometer.

Hotter water is lighter and afcends.

In all the foregoing experiments, more or lefs warm water defcended through the ice-cold water into the cork cup; but the refults of the experiments which were made with the metallic ball heated in boiling water, fhew evidently that when the particles of water at or near the furface of a quiefcent mafs of ice-cold water, are by any means heated to a temperature feveral degrees above that of 40° of the fcale of Fahrenheit, fuch particles, fo heated, become fpecifically lighter than ice-cold water, and confequently cannot defcend in that cold and denfer liquid.

With the hot cone ſome of the fluid aſcended and ſome deſcended.

In the experiments in queſtion, (with the hot ball) ſome of the particles of the water, namely, thoſe which came firſt into contact with the conical metallic point, were heated to a higher temperature than that at which they were diſpoſed to ſink in ice-cold water; and theſe roſe and ſpread themſelves over the ſurface of the cold liquid; but others, which happened to acquire leſs heat, deſcended in it, and after filling the cork cup, overflowed, no doubt its brim, and then deſcending to the bottom of the braſs cup, and coming into contact with that ice-cold metal, were there cooled, and there remained at reſt.

Cork a good non-conductor.

As cork is an excellent non-conductor of heat, the warm water accumulated in the cork cup during an experiment retained its heat a long time after the heated ball was removed, notwithſtanding its being ſurrounded on all ſides, and even covered immediately by ice-cold water; (which by the by is a pretty ſtrong proof that water is by no means a good conductor of heat) care however was taken, not only to remove this warm water after each experiment, by bruſhing it away with the ſoft end of a ſtrong feather or quill, but alſo to cool the cup, and reduce it to the temperature of freezing water; which laſt operation was found to be much accelerated by bruſhing it out frequently with the feather. In order that this feather might itſelf be ice-cold, it was ſuffered to remain conſtantly in the ice-cold water, in the cylindrical veſſel.

Cautions.

Apparatus by which the cone was fixed.

I muſt not forget to give an account of the means uſed for fixing the metallic ball in its place. This was done in a very ſimple and effectual manner. A ſlip of ſtrong tin (G H) ſix inches long, and $2\frac{1}{4}$ inches wide, with a circular hole in the middle of it, one inch in diameter, being laid horizontally on the top or brim of the cylindrical veſſel, in ſuch a manner that the center of that circular hole coincided with the axis of the cylindrical veſſel, the ſhort cylindrical projection belonging to the ball being introduced into that hole, the ball was firmly ſupported in its proper place.

The quantity of ice-cold water in the cylindrical veſſel was ſo regulated that the whole of the conical point being ſubmerged, the ſurface of the water was on a level with the lower end of the cylinder, or, which is the ſame thing, with the baſe of the inverted cone.

4 When

When not only the whole of the conical point, but a part alſo of the ſhort cylinder were immerſed in the ice-cold water, the warmed water appeared to be thrown into eddies in its deſcent, which diſperſing about prevented its falling regularly in one continued ſtream into the cork cup.

The conical figure preferable for the immerſed metal.

To *conclude*, I would juſt obſerve, that although the foregoing experiments appear to me to be perfectly unexceptionable, and that their reſults afford demonſtrative proof of the facts which they were contrived to eſtabliſh; yet, when attempts are made by experiments ſimilar to theſe, to determine whether heat can be made to paſs *downwards* in water which is at a higher temperature than that at which its denſity is a maximum, difficulties occur which appear to me to be quite inſurmountable.

It is not eaſy to make concluſive experiments at elevated temperatures:

The fluidity of water is ſo perfect, or the mobility of its particles ſo great, that the liquid at the ſurface, which is firſt heated and rarefied, immediately ſpreads far and wide, and meeting with the ſides of the containing veſſel, heats them, and this heat, ſo acquired, making its way downwards (as well as upwards) in the ſolid ſubſtance of which the veſſel is conſtructed, raiſes the temperature of the lower ſtrata of the fluid in contact with it, which moving towards the axis of the veſſel, communicates heat to a thermometer placed there, below the ſurface of the water.

For the ſides of the veſſel greatly modify the effects by conducting the heat, &c.

That theſe various operations do in fact take place, nobody can doubt: and it appears to me to be the more probable that all the heat which a thermometer placed below the ſurface of warm water acquires when a great degree of heat is applied to the particles of that fluid which are at the ſurface, is in fact received from the ſides of the containing veſſel, not only becauſe the thermometer acquires heat ſo very ſlowly, but alſo, and more eſpecially, becauſe this heat is acquired much more ſlowly when the containing veſſel is wide than when it is narrow; and alſo when it is made of a ſubſtance which is a good conductor of heat than when it is conſtructed of a ſubſtance which is a bad conductor of heat, as I have found by experiment. But as this particular enquiry is foreign to my preſent purpoſe, I ſhall not enlarge on it in this place.

Facts which ſhew the probability that conſiderable heats are chiefly tranſmitted downwards through fluids by the agency of the veſſel.

II.

Description of a Press for preserving botanical Subjects; with an Account of the Success of the Improvement in the Art of Blasting pointed out by Mr. JESSOP *in this Journal. In a Letter from Mr.* THO. HARRISON.

To Mr. NICHOLSON.

SIR,

Introductory obs. on preserving plants.

BEING desirous to make a collection of the plants which grow in the neighbourhood of Kendal, and to preserve them in an herbarium, I was naturally led to a consideration of the different methods, which have been used by botanists, of drying and preserving specimens; but finding none so complete and expeditious as I could wish, I therefore adopted a plan of my own, which I am persuaded will be found to answer every purpose that the botanist can require. The real utility which is to be derived by a young botanist from the preservation of plants, is the power of future examination, and to answer the purpose the natural appearance of each specimen ought to be preserved as calculated to produce this effect; for since the seed-vessels and stems of plants occupy much more space in thickness than the leaves, in order to preserve the latter from shrivelling, the two former must generally be bruised in such a manner as totally to preclude any accurate investigation afterwards: and this on trial is found to be the case: besides, all the plants that happen to be in the press at the same time, however various their texture, are subjected to the same degree of pressure.

Remarks on the methods now in use.

The plan recommended by T. Velley, Esq. seems equally exceptionable; I mean the method of placing the plant when fresh between several sheets of blotting-paper, and ironing it with a large smooth heater pretty strongly warmed, till all the moisture is dissipated. By this method it is evident that the parts of fructification must be much bruised: it may preserve the colour of the blossoms better than any other, but this in the science of botany is not very essential, at least if it was, I believe that no method hitherto discovered will succeed universally in arresting the fading beauty of delicate flowers. The next plan which is given by Dr. Withering, is one used by by a Mr. Whateley a ſurgeon, but his contrivance, though certainly better adapted to preſerve the parts of fructification and the ſhape of the ſtem entire than the two former, I found would take up much more time than a country ſurgeon can well ſpare; therefore this led me to make what I thought an improvement on Mr. Whateley's plan, and the experienced botaniſt is left to judge of my ſucceſs from the following deſcription; this I can ſay for it, that it anſwers completely (of courſe) to my own wiſh; but further, that Mr. John Gough, of Middleſhaw, who has the beſt collection of plants in this county, regrets that he had not the ſame contrivance, and has urged me much to ſend you the following deſcription of my preſs.

This inſtrument conſiſts of 17 oblong boxes, each, excepting the uppermoſt and lowermoſt, is made of four ſides of well-ſeaſoned oak wood, two inches deep and one-fourth of an inch thick, dove-tailed together; the two end-pieces of which have two notches each in the middle wherein to place the fingers in lifting it; and the bottom conſiſts of canvas glued and nailed to the wooden frame: in each corner is fixed a ſmall triangular piece of wood reaching half an inch from the bottom upwards (for a purpoſe hereafter to be explained). Theſe boxes are made ſo as to be placed one within another ſucceſſively upwards, the loweſt (I am ſpeaking only of the 15 with canvas bottoms) meaſures 20 inches by 16 on the outſide, and the higheſt 12 inches by 8. The bottomeſt, or carriage on which all the others are ſupported, is much ſtronger, the ſides of it being two inches deep and three quarters of an inch thick: its bottom is of wood of the ſame thickneſs, all over perforated with holes one-third of an inch in diameter, and reſts on four iron caſtors, one at each corner, to render the whole more eaſily moveable; at each end of it there is an iron handle: This box is 20 inches by 16 within, and will therefore barely receive the largeſt of the boxes with canvas bottoms. The uppermoſt, or 17th box, is of the ſame conſtruction as the canvas boxes, excepting that its bottom is of wood of the ſame thickneſs as its ſides, with a number of holes pierced through of the ſame magnitude as thoſe in the carriage, and canvas glued to the under ſide of it. To complete this preſs, two folds of blotting-paper are to be placed

The author's new preſs deſcribed. It conſiſts of a ſeries of trays or boxes, having (all but the loweſt) canvas bottoms. They contain ſand, and the plants are laid between box and box.

placed over the holes in the bottom of the carriage, in which, as well as all the canvas-boxes, muſt be put an inch in depth of the fineſt ſand, waſhed and ſifted; river ſand is moſt eligible, as being leaſt angular; which ſand muſt be made level on the top by drawing over it a rule that reaches one inch into each box: theſe boxes are placed one within another, and each by containing an inch depth of ſand, raiſes the incumbent frame one inch, ſo that when they are thus charged and placed one upon another, the whole makes a truncated pyramid of 18 inches in height, and weighing about 1 cwt. The uppermoſt box may be filled with ſand, or, if more weight be neceſſary, with ſhot. To this box I have added (what is by no means eſſential) a cap, conſiſting of the ſides of three boxes fixed one within another, each projecting one inch above its inferior, for the ſake of uniformity; the top is covered by a thin board: when this cap is placed upon the uppermoſt box, the whole has not an inelegant appearance.

Reference to the engraving.

Leſt the deſcription ſhould not be ſufficiently clear, I have ſketched the following figure, repreſenting a perpendicular ſection of the preſs viewed on one ſide of it. *Plate XV.*

Fig. 1. repreſents a perpendicular ſection of the preſs made lengthwiſe.

a. The end of the bottomeſt box or carriage.

b. Its bottom part perforated with holes.

c. The end of the firſt box with a canvas bottom.

d. The canvas bottom.

e. The ſand which but half fills the box.

f. The 17th or uppermoſt box, with a perforated wooden bottom.

g g g. Repreſent the end of the cap formed of the frames of three boxes, in the empty ſpace of which may be placed the botaniſt's pocket box.

Fig. 2. repreſents the rule wherewith the ſand is made level in the boxes; it is 18 inches long.

h. A notch one inch deep.

i. The edge made thin.

k. A notch an inch long.

Fig. 3. repreſents the end of a canvas box.

l l. The two notches to place the fingers in when the box is to be lifted.

The

The Method of uſing it.

After a ſpecimen is ſelected, a ſlip of paper is to be attached to it, containing its name, place of growth, and time of gathering. The boxes are then to be taken off by three or four at a time, until the number removed may be judged to be of ſufficient weight to produce the neceſſary preſſure. On the ſand in the box thus ſelected for uſe, a ſingle fold of blotting-paper muſt be placed; upon that the plant, taking care to preſerve as much as poſſible the natural poſition of its characteriſtic parts: Over the plant a ſecond fold of paper muſt be laid, and then the boxes are to be replaced. If the plant be woody and require much preſſure, place it near the bottom; but if ſucculent, near the top; and herein conſiſts one great advantage of this preſs over every other, ſince the preſſure can be ſelected or varied at pleaſure, and all the parts of the plant preſſed equally; for if the plant be ſucculent, the preſſure can ſcarcely be too ſlight at firſt, and may be gradually increaſed without ever bruiſing the tendereſt part; and if, on the other hand, ſtrong preſſure be required, the plant may be placed in the bottomeſt box; and if the weight of ſand be not thought ſufficient, any number of the boxes next above it may be loaden with ſmall ſhot, by which the preſſure may be increaſed until the boxes be full, when the preſſure exerted would be much greater than the ſtrongeſt plants could bear without having ſome of its eſſential parts defaced. Another advantage which led me to give a preference to this method of preſſing plants, was the great ſaving of time; for though my profeſſional avocations ſhould require my immediate preſence at a diſtance, and thus drag me from this innocent amuſement when in the buſieſt part of claſſing and arranging my ſpecimens, and though my preſs ſhould be completely ſeparated into its conſtituent parts, yet the whole can be arranged, in the ſpace of one minute, to remain until the next leiſure time that may occur; ſo that, in fact, with this contrivance, botany is a ſtudy that can be entered into or given up as it may ſuit the convenience of a moment; which is not the caſe where any of the plans deſcribed in Dr. Withering's Introduction to the Study of Botany * are followed.

Method of uſing it.
The plants are laid between blotting-paper, and the preſſure regulated by the number of boxes above each, &c.

* Third Edition.

It

Remarks on the effects and advantages of this contrivance.

It may be neceſſary to explain ſome parts of the ſtructure of the preſs. The holes in the bottomeſt and uppermoſt boxes are to admit of the circulation of air and the evaporation of moiſture, which will be conſiderable when the preſs without its cap is placed within the influence of the ſun or fire, as it always ought to be: and the ſmall triangular pieces of wood in the corners of each box, are to prevent the upper boxes from preſſing ſolely on the canvas bottom of the loweſt box, when any number are lifted together: theſe may at firſt ſight appear likely to impede the preſſure of the ſand when placed in the preſs, but this objection will vaniſh when it is recollected that each box contains an inch depth of ſand: and it is evident, on examination of the preſs, that the ſides of the boxes ſerve no other purpoſe than to keep the ſand together; for they are perceptibly moveable in a perpendicular direction, and ſuſtain no preſſure from the ſuperior boxes; therefore the plant is placed as if in a heap of ſand.

The beſt method of preſerving dried plants is to ſtitch them to a paper.

I am afraid I have already treſpaſſed too long on the attention of the reader, but I cannot diſmiſs the ſubject without offering a few obſervations on the manner of preſerving plants after they are dried. There have been at leaſt three methods uſed: firſt, fixing them to paper by the aid of gum-water: ſecond, placing them looſely in a book: third, ſtitching them with thread to a ſheet of paper. The firſt plan appears to me the leaſt eligible, for it cannot be neatly executed unleſs the plant be preſſed nearly flat, and by fixing it to the paper it can never admit of an accurate examination in future, but becomes a mere picture, and a very imperfect one; for almoſt every diſtinguiſhing character, eſpecially the parts of fructification, are injured or defaced; even the general habit is often ſo much altered that the plant could not be aſcertained, were it not for the name generally ſubſcribed at its root. The ſecond plan is alſo objectionable; for though the plant may at any time be ſubjected to examination, yet by lying looſe in a book, it is very apt to be broken by the leaſt motion, particularly when kept dry, which all preſerved plants ought to be. The third appears to me the leaſt liable to objection; for if the plant be neatly ſtitched down, it will not be ſubject to ſuch motion as to injure its parts; and by cutting the threads

threads it may be detached from the paper, and subjected to any future investigation.

I am, Sir,

Your's, &c.

Kendal, July 8, 1805. THOMAS HARRISON.

P. S. I cannot refrain from calling to the recollection of your readers a valuable paper in the 9th Vol. p. 230, of your Journal, recommending the use of sand in the blasting of rocks. The effects there related of this simple agent, I confess did astonish me more than any thing I ever read: and as very serious and even fatal accidents have happened by the method of ramming blasts with stone in the limestone and slate quarries in this neighbourhood, I was determined to repeat the experiment the first opportunity, and thus shew its effects to the men who work in those places. Accordingly no long period elapsed before I had occasion to be in the neighbourhood of the slate quarries in Longsliddale, about ten miles from Kendal; when I went to one of those quarries, taking with me a bag of small sand, consisting of powdered freestone used in this country to scour pans with, and strew upon the stone floors. I mentioned my business to one of the workmen who was then engaged in boring a hole 24 inches deep and about an inch diameter in a slate rock; the stratum was about 27 inches thick, and reclined from the perpendicular about 20 degrees; and this being the first blast, it was consequently firmly surrounded and fixed in on all sides by solid rock: The part that he wanted to throw out by the blast was supposed to be five ton weight: The direction of the hole which he was boring was perpendicular to the stratum, and therefore elevated above the horizon about 20 degrees. He smiled at my proposal, but said he would try the experiment to satisfy me, provided I would pay for the powder if it failed; but he seemed to think that I had a very poor idea of blasting, to believe that a little light sand would answer the purpose of the laborious and dangerous process of ramming which he had been accustomed to use. However, he charged his blast with powder in the manner he had been used to do; *i. e.* into this hole of

Account of the successful application of the method of blasting rocks indicated in this Journal by W. Jessop, Esq.

Safe and eafy method of blafting rocks.

of 24 inches in depth, he put as much powder, together with two or three pieces of broken fticks, of half an inch diameter and about four or five inches long, as reached 12 inches upwards: The fticks he introduced for the purpofe of keeping the powder loofe in the hole, fo that it might take fire more nearly all at the fame inftant: This done, he put in a charged ftraw, into the outer end of which was fixed a match-paper; and laftly, he (with a fmirking countenance, anticipating the joy he expected to feel at my difappointment) filled up the hole with fand. The match was then fired, upon which we removed to a diftance, and the powder foon exploded; when he immediately exclaimed, from the peculiarity of the report, though the effect was unfeen by him, that the rock was as completely fhattered as if the hole had been rammed with ftone; and a view of the place verified the prediction. Since that time the plan has been adopted in that and moft of the neighbouring quarries, with the ufe of fmall fand which is wafhed out of the river in floods, and the method anfwers as well as the old one which was attended with fo much danger. In one cafe the fand was thrown out twice without any effect on the rock; the third time, the hole was rammed in the ufual manner with ftone, which was thrown out likewife; fo that the new procefs fortunately loft no reputation.

I have detailed thus much in confirmation of the valuable paper before mentioned, and in hopes that it may induce fome of your readers who refide in the neighbourhood of mines or quarries, to endeavour to introduce this fafe and eafy method in lieu of the dangerous and tedious one of ramming with ftone, in which procefs we have had repeated inftances of the lofs of limb, vifion, and even life itfelf: In the ufe of fand there is comparatively no danger, and by adopting this plan many a valuable life will be faved.

Defcription

III.

Defcription of a Safety Valve, containing a Vacuum Valve in the fame Hole of the Boiler. By Sir A. N. EDELCRANTZ.*

IN large boilers or coppers, where boiling fluids are enclofed, a *fafety valve* is generally ufed to prevent their burfting, from an unexpected exceffive force of the elaftic fteam, and, befides, a *vacuum valve*, to prevent their being compreffed or crufhed by the weight of external air, in the cafe of a fudden condenfation of the vapours. Thefe two valves are commonly fitted in two different holes in the boiler; but as a more fimple, and confequently more eligible, method feems to be that of joining them together, I take the liberty to fubmit to the Society for the Encouragement of Arts, &c. the following contrivance for that purpofe:— Large boilers require a fafety valve and a vacuum valve.

a b, Plate XII. Fig. 2. is a common conical fafety valve, fixed in the boiler *c d*, having four openings, *i i*, which are reprefented in a plan view in *Fig* 3: *e f* is the metallic rod, bearing the weight K K, with which the fafety valve is loaded, and extending itfelf under that valve to *f*: *g h* is the *vacuum valve* confifting in a plane circular plate, with a brafs tube fliding along the rod, and preffed by a fpiral fpring to the fafety valve *a b* (againft which it has been well ground in making it), clofing in that fituation the openings *i i*. Defcription of a valve that performs both offices.

Such being the conftruction of the whole, it is evident, that when the elafticity of the fteam increafes, the two valves, joined together, with the holes *i i* fhut, make but one, oppofing to the elafticity of the fteam an united refiftance, which is regulated by the weight *k k*, in the common way; but, on the contrary, when by condenfation of the vapours a vacuum is produced, the external air in preffing through *i i*, upon the vacuum valve *g h*, forces it down, and opens to itfelf a paffage into the boiler. It is a loaded valve opening upwards; having in its plate a fpring-valve opening downwards.

The valve *g h* may eafily be made conical, like the other, if that form fhould be preferred; but in different trials, I have found planes, if well turned and ground together, join as per-

* From the Memoirs of the Society of Arts for 1804. The filver medal was awarded for this invention.

fectly as can be desired, being pressed by the united elasticity of the spring and the steam.

The same applied to a former contrivance.

Fig. 4. is the same contrivance adapted to a new kind of *safety valve* or *piston*, which, though I originally intended it for the use of Papin's digesters of a new construction *, has been, in a larger size, applied by me even to steam engines, and is described in the Philosophical Magazine of December, 1803.†

Other experiments.

I have lately begun, and shall pursue, a set of experiments, with the intention of regulating by this safety piston, the quantity of admitted air to fire-grates, and to effect, by that means, a new mode of regulating the fire, and the elasticity of steam in boilers, with less expenditure of fuel and of force than usual; of which idea a hint is given in the work and place above mentioned. The result of these researches I shall at some future period do myself the honour of communicating to the Society.

IV.

Observations and Experiments for the Purpose of ascertaining the definite Characters of the primary Animal Fluids, and to indicate their presence by accurate chemical Tests. By JOHN BOSTOCK, *M. D.* *Communicated by the Author.*

Great want of precision in the animal analysis; particularly of the fluids.

THE precision which the analysis of mineral and vegetable substances has attained, does not appear to be yet extended to

* Nicholson's Journal, March, 1804.

† The description of this contrivance being already published, it would be superfluous to repeat it. I only beg leave to add the following practical remark. A metallic piston, if well turned and fitted into a cylinder of exactly the same kind of metal, will probably have the same degree of expansion, especially if hollow, and consequently will not increase its friction in any increased degree of temperature. But as in practice the cylinder is commonly exposed to a lower temperature than the piston, heated by the steam, a little increase of friction will take place by an increase of heat. To prevent the effect of this, I have found it useful to employ for the piston a metal of somewhat less expansive power than the cylinder: and the expansion of red copper being to that of brass nearly as 10 to 11, I prefer making the piston of the former metal, when the cylinder is made of brass.

the

the products of the animal kingdom. This remark may be applied both to the solids and fluids which compose the animal body, but it is the most applicable to the latter class of substances. The terms serous, mucilaginous, gelatinous, &c. are employed, even by the most esteemed medical and physiological writers *, in a vague and indeterminate manner, without attending either to the original import of the word, or to the restricted meaning, which it is necessary to impose upon popular expressions when they are adopted in scientific researches. The object of the present paper is to ascertain a definite character for what I propose to call the primary animal fluids, and to discover delicate and accurate tests, by which their presence may be easily and certainly indicated. By primary animal fluids, † I mean those into which the compound fluids existing in the animal body are capable of being resolved by the application of different re-agents, without decomposing them into their ultimate elements. Primary animal fluids.

Albumen.

The first of the animal fluids which I propose to make the subject of my investigation is the albumen. With the exception of water, no fluid appears to enter so largely into the composition of the animal body. It forms a very considerable proportion of the blood, and is found in greater or less quantity in nearly all the secretions. It is also capable of assuming the solid form, without undergoing any change in its chemical properties; in this state it constitutes the basis of all the membranous substances, which are so extensively dispersed through every part of the system; it composes the cellular tissue into which the earth of the bones, and the fibrous matter of the muscles are deposited, while it enters largely into the structure of the skin, the glands and the vessels. At present, however, we shall direct our attention to it while in the liquid form. Albumen, a large component part of animals.

* Even Mr. Abernethy, in his late valuable work on tumors, speaks of the gelatinous part of the blood, where I conceive from the context, there can be no doubt that he intended to designate the fibrine.

† All the animal fluids, both primary and compound, are merely solutions of a solid body in water; but those substances which are most frequently seen in a state of solution, have very generally obtained the title of fluids.

In

Moſt conveniently obtained from white of egg.

In order to obtain it in a ſtate of purity, I had recourſe to the white of the egg, a ſubſtance to which the name of albumen was originally applied, and which is ſtill conſidered by the moſt eminent chemiſts * as compoſed entirely of this ſubſtance. In order to aſcertain how far this opinion was correct, I kept a quantity of the white of the egg in a temperature of 212°, until it was firmly coagulated. It was then cut into ſmall pieces and placed in the upper part of a narrow-necked funnel, when a few drops of a browniſh viſcid fluid were ſeparated from it. Other pieces of the ſame coagulum were kept for ſome time in boiling water, the fluid being then paſſed through a filtre, had acquired a light browniſh colour, and a faint odour; when agitated it was ſlightly mucilaginous. By ſlow evaporation, a ſmall quantity of a brittle, ſemi-tranſparent ſubſtance was obtained. It appeared therefore evident, that the white of the egg contains a ſmall quantity of a ſubſtance incapable of coagulation, and therefore eſſentially different from albumen. I ſtill, however, continued to employ it for the purpoſe of aſcertaining the properties of albumen, as it affords this ſubſtance in a ſtate of greater purity than it can be obtained from any other ſource.

White of egg is not pure albumen; it contains a little of matter not coagulable.

White of egg contains 80 parts water; 15,5 ſolid albumen and 4,5 uncoagulable matter.

It was an object of ſome importance to aſcertain the proportion which this foreign ingredient bore to the albumen itſelf; 100 grains of firmly coagulated albumen were kept for ſome time in boiling water, and this being poured off, a freſh quantity was added, and this proceſs repeated until the water appeared to contain no farther impregnation. The whole of the fluid was then evaporated, and a reſiduum was left which amounted to $4\frac{1}{2}$ grains. Beſides the admixture of this peculiar ſubſtance, the white of the egg contains a conſiderable quantity of water, not only in its liquid ſtate, but after it is coagulated. By a gentle heat the water may be evaporated, and the ſolid matter is left behind in the form of a hard, brittle, tranſparent ſubſtance; I have found that upon an average, $\frac{4}{5}$ of recently coagulated albumen may be conſidered as conſiſting of water. Hence it will appear, that 100 grains of the white of egg conſiſt of 80 grains water, 4.5 grains of uncoagulable matter, and 15.5 grains only of pure albumen.

* Hatchett, Phil. Tranſ. 1800, p. 375.
Thomſon's Chemiſtry, IV. p. 484.

The

The moſt diſtinguiſhing characteriſtic of albumen is the property of being coagulated by heat, which forms an obvious and eaſy teſt of its preſence, when it exiſts in a compound animal fluid in any conſiderable proportion. In order to aſcertain how ſmall a quantity of albumen could by this means be rendered viſible, I added 13 grains of the white of the egg to 87 grains of water, and thus formed a ſolution, one grain of which contained $\frac{1}{50}$ grain of pure albumen. Five grains of this ſolution were then added to 95 grains of water, ſo that 100 grains of water contained $\frac{1}{10}$ grain only, or $\frac{1}{1000}$ of its weight of pure albumen. This was expoſed to the heat of boiling water, and a perceptible opacity was produced in the fluid.

Diſtinguiſhing character of albumen coagulated by heat.

One thouſandth part in water will coagulate;

The effects of the oxymuriate of mercury were next tried. One drop of a ſaturated ſolution of this ſalt, added to 100 grains of water, containing $\frac{1}{1000}$ of its weight of albumen, produced a very evident milkineſs; after ſome hours a curdy precipitate ſeparated and fell to the bottom. A ſolution of half the ſtrength, containing only $\frac{1}{2000}$ of its weight of albumen, was then tried by the ſame re-agent, and even in this inſtance, a ſufficiently obvious effect was produced.

—oxymuriate of mercury precipitates it;

The nitro-muriate of tin is a powerful coagulater of albumen in its unmixed ſtate, but I found it not to be ſo delicate a teſt as the oxymuriate of mercury. One hundred grains of water, containing $\frac{1}{5}$ grain of albumen, *i. e.* $\frac{1}{500}$ of its weight, was not affected by this re-agent until after ſome hours, when the fluid exhibited a degree of milkineſs.

—as does nitromuriate of tin,

In order to aſcertain the effects of tannin upon albumen, I macerated half an ounce of powdered galls in half a pint of water for ſome hours, and filtered the fluid. A deep brown tranſparent liquor was produced, 100 grains of which I found by evaporation to contain $2\frac{1}{2}$ grains of ſolid reſiduum. Equal parts of this preparation of galls and of a ſolution of albumen, in the proportion of one part to 1000 parts of water, were mixed together; at firſt no effect could be perceived, but after ſome time an evident precipitate was formed and ſlowly ſubſided.

—and tannin.

The aqua lithargyri acetati, or extract of Goulard, is an active precipitant of ſeveral animal fluids; when dropped into a ſtrong ſolution of albumen, a very copious and denſe precipitate is immediately formed.

Goulard throws it down;

It

—but doubtfully.

It is, however, somewhat difficult to determine how far this effect depends upon the albumen itself, as Goulard has been considered to be the appropriate test of the uncongelable part of the serum of the blood, which, it may be supposed, resembles the uncoagulable part of the white of the egg. The aqua lithargyri acetati is likewise decomposed by several of the saline bodies which are found to exist in almost all the animal fluids. I have sometimes found it to yield a precipitate even when added to distilled water, and in all cases, after exposure to the atmosphere for a few hours, the water to which it has been added becomes turbid, and is covered with a thin film. In order to try the effect of this re-agent on albumen, I added one drop of it to 200 grains of water, and upon observing that the transparency of the fluid was not affected, a single drop of the solution of albumen, of the same strength with that mentioned above, was added. It formed a dense precipitate as it fell through the fluid, and upon agitation, the whole was rendered slightly milky. In this case the proportion of the water to the albumen was as 10.000 to 1; to the uncoagulable part of the white of the egg, it would be about as 30,000 to 1.

Aqua lithargyri acetati plentifully precipitates it.

Nitrate of silver also has a like effect;

The next re-agent which I employed was the nitrate of silver. A single grain of a saturated solution of this salt produced an evident turbidness in 100 grains of water, containing $\frac{1}{10}$ gr. of albumen, and after some hours a small quantity of a curdy precipitate fell to the bottom of the vessel. It might, however, be suspected that in this case, the effect produced depended upon a quantity of muriate of soda contained in the albumen. I found that 100 grains of water, containing only $\frac{1}{100,000}$ gr. of common salt was rendered evidently turbid by one drop of the nitrate of silver; * but the precipitate which

* I weighed very exactly a grain of salt, and dissolved it in 200 grains of water. One grain of this solution was afterwards added to 99 grains of water, and by repeating the operation for three successive times, I obtained 100 grains of water, containing only $\frac{1}{20,000}$ of its weight of salt. I then took 99 grains of distilled water, and poured into it one drop of the nitrate of silver: after waiting for some time, until I was satisfied that no effect would take place, I added a single drop of the last solution of salt, thus making it $\frac{1}{200,000}$ part of the mixture; a faint but perceptible opacity was almost immediately produced.

is

is formed by the nitrate of silver acting on the muriate of soda is in the form of a greyish powder, and subsides more rapidly than it did in the former instance, where it produced a white, flaky precipitate.

I found that a solution of albumen of the same strength with that employed in the last experiment, was immediately decomposed by the nitro-muriate of gold. One drop of this metallic solution instantly produced a dense white precipitate in 100 drops of water, containing only $\frac{1}{10}$ of a grain of albumen. —and nitro-muriate of gold;

Albumen in a concentrated state is powerfully coagulated by alum; I found however, that this re-agent is not so accurate a test of its presence when in a diluted state, as some of those which I had already employed: $\frac{1}{5}$ grain of albumen, dissolved in 100 grains of water, was indeed rendered slightly turbid by the addition of a few drops of a saturated solution of alum, but no precipitate was formed. —and alum.

Before I conclude my account of these experiments I must observe, that the strength of the solution of albumen was in all cases rather less than my estimate. When I added the albumen to the water, a small portion of it always remained insoluble, and this was separated from the fluid by filtration before the experiments were performed. This insoluble part I supposed to consist of the membranous matter, with which it is said that the white of the egg is intersected. The quantity was indeed almost too small to be appreciated, but where it is desirable to attain as much accuracy as possible, I think it necessary to mention every circumstance which may in the smallest degree affect the result. Obs. on the solution of albumen.

The experiments related above will, I conceive, indicate with a sufficient degree of accuracy, the presence of albumen as a constituent of an animal fluid. The property of being coagulated by heat is a characteristic of this substance, which will always serve as a mark of discrimination, and we have found that this property is not destroyed by dilution with 1000 times its weight of water. This therefore may be considered as a test of its presence minute enough for all practical purposes. We have also found that there are several re-agents which possess the power of precipitating it from its solution in water, while existing only in the same proportion. It will be necessary, however, to observe their operation upon the other animal substances before we can determine their use in the analysis or compound fluids. Remarks. The coagulation by heat is a good distinctive character of albumen.

Jelly.

Jelly.

Jelly. It is liquifiable by heat and becomes concrete by cold; and is a large component part of animals.

The next ſubſtance which I propoſe to examine is jelly. The peculiar characteriſtic of this body is its property of becoming concrete by cold, and being liquified by the application of a gentle heat. It enters into the compoſition of the blood, though leſs largely than the albumen. It is alſo an ingredient in the ſkin, membranous texture, ligaments, cartilages and tendons. By boiling it is eaſily extracted from theſe ſubſtances, and by evaporation and cooling the whole is reduced to a maſs of greater or leſs ſolidity, in proportion to the previous degree of concentration. By a proceſs of this nature, iſinglaſs is prepared from the bones and cartilages of fiſh; as this ſubſtance has been conſidered to conſiſt of jelly nearly in a ſtate of purity, I employed it for the following experiments.

Obtained by the ſolution of iſinglaſs. One fiftieth part in water will coagulate.

Four grains of iſinglaſs were diſſolved in 200 grains of water, and thus a ſtandard fluid was formed, one grain of which contained $\frac{1}{50}$ gr. of jelly. This ſolution became perfectly concrete by cooling. In the firſt place I wiſhed to aſcertain how ſmall a proportion of jelly diſſolved in water was capable of aſſuming the concrete ſtate. Equal parts of the ſtandard fluid and of water, *i. e.* one part of jelly to 100 parts of water, produced a compound which was completely ſtiffened by cooling; but I found that two parts of water to one of the ſtandard, *i. e.* one part of jelly to 150 parts of water, produced a compound, which though evidently gelatinous, did not aſſume the concrete form.

It is actively precipitated by tanin.

One of the moſt active precipitants of jelly is the tanning principle. I found that a mixture of 5 grains of the ſtandard ſolution and 95 of water, produced a copious precipitate when added to an equal quantity of an infuſion of galls of the ſame ſtrength with that employed in the experiments upon albumen. In this inſtance the jelly was to the water as 1 to 1000. I afterwards reduced the quantity of jelly until it compoſed $\frac{1}{5000}$ part only of the ſolution, and in this caſe a conſiderable precipitate was ſtill produced by the infuſion of galls.

—but not by aq. lith. acet.

A quantity of the ſtandard ſolution had a few drops of the aqua lithargyri acetati added to it, but no more effect appeared to be produced than would have enſued from mixing it with an equal quantity of pure water.

No effect was produced by adding the oxymuriate of mercury to the standard solution. The nitrate of silver and the nitro-muriate of tin were each employed, and produced a very slight and almost imperceptible opacity. —nor by ox. m. of merc. nor n. of silv. nor n. m. of tin.

The addition of the nitro-muriate of gold caused a small quantity of a dense precipitate when added to the standard solution; but when this was so far diluted as to contain one grain of jelly in 500 of water, the effect was no longer perceptible. N. m. of gold gave precip.

Mucus.

Animal mucus or mucilage enters largely into the constitution of many parts of the body, and forms a considerable proportion of several of the secretions. The term mucus had been generally employed in a vague and unrestricted sense, until Mr. Hatchet, in his valuable paper on the membranous parts of animals, inserted in the Phil. Transf. for 1800, attempted to assign to it a more appropriate and definite meaning. He conceives that jelly and mucus are only modifications of the same substance, and do not essentially differ from each other; he considers it to be entitled to the appellation of mucus, when it is soluble in cold water and cannot be brought to the gelatinous state *. Dr. Thomson adopts in general the idea of Mr. Hatchett, and lays down the following as the characteristic properties of animal mucilage. It is soluble in cold water, insoluble in alcohol, neither coagulable by heat nor generating into a jelly, precipitable by tan and the nitro-muriate of tin †. I have been induced from the results of my observations, to form a different opinion respecting the relation of jelly and mucus, but I shall defer the statement of it until I have related the experiments which have led me to dissent from such high authority. Mucus; a term not distinctly applied by chemists.

By agitating for a short time some recent saliva in cold water, part of it was dissolved, and after being passed through a filter, was made the subject of experiment, being, as I conceived, a solution of nearly pure mucus. By a careful evaporation I found that the water had dissolved $\frac{1}{240}$ part of its weight. Mucus obtained from saliva.

* Hatchett, Phil. Transf. 1800. 369 and 381.

† Thomson, IV. 503.

No

Effect of reagents. No coagulation by moderate heat nor did it gelatinise by cooling.

No effect was produced by the addition of the oxymuriate of mercury, and the nitro-muriate of tin caused only a slight opacity. No effect was produced by the addition of equal parts of this solution and the infusion of galls. The aqua lithargyri acetati added to the solution produced an immediate opacity, and after some time a white, fleaky precipitate fell to the bottom of the glass. No appearance of coagulation was produced by exposing the fluid for some time to the heat of boiling water, nor was there any tendency to gelatinize by evaporating and afterwards cooling the fluid.

Mucus from an oyster.

I afterwards endeavoured to obtain mucus in a state of purity from another source. For this purpose an oyster was agitated for a few minutes in cold water; the fluid was filtered and appeared slightly opake and glutinous.

By evaporation it appeared that it had dissolved about $\frac{1}{50}$ of its weight of animal matter. A quantity of this solution, diluted with an equal bulk of water, was employed in the following experiments.

Reagents.

The oxymuriate of mercury being added to it produced no effect. The infusion of galls after some time produced a slight degree of turbidness, and at length a precipitate was formed in small quantity. The aqua lithargyri acetati caused an immediate opacity, and after some time a dense precipitate.

The Goulard indicates its presence.

These experiments nearly coincide with the former. In both cases no effect was produced by the oxymuriate of mercury, thus proving the absence of albumen. The small precipitate caused by the galls indicate the existence of only a very minute quantity of jelly. The effect was scarcely as great in this instance, where the animal matter composed $\frac{1}{100}$ part of the weight of the solution, as was produced by the same reagent upon a solution of jelly, where it composed only $\frac{1}{5000}$ part of the weight of the fluid. Very nearly the whole therefore of the animal matter probably consisted of mucus, the presence of which was indicated by the Goulard.

Exclusive tests; tan for jelly, aq. lith. acet. for mucus, ox. m. of merc. for albumen.

I apprehend that these experiments will be deemed sufficient to establish a decided and essential difference between mucus and jelly, independent of the gelatinizing property of the latter, the effects produced upon them by the tanning principle and by the aqua lithargyri acetati are exactly opposite. Tan is a most delicate test of jelly, but does not in any degree affect mucus. Aqua lithargyri acetati is a delicate test of mucus

cus, but does not in any degree affect jelly. The oxymuriate of mercury, on the contrary, which is one of the most accurate tests of albumen, does not appear to be affected either by jelly or by mucus.

Albumen, jelly, and mucus, I am inclined to consider as the only primary fluids which are dispersed through the different parts of the body. Particular vessels or glands contain and secrete particular fluids, which cannot be resolved into other fluids without decomposition, as the fibrine of the blood, the resin of the bile, the urée of the kidney, &c. but these are in all instances confined to their appropriate organs, and do not necessarily enter into the present investigation.

These are the primary and general fluids.

From the above experiments I think we may be entitled to lay down, with a considerable degree of accuracy, the leading characteristics of the three primary animal fluids, and to establish tests by which their presence may be minutely ascertained. The most remarkable property of albumen is its becoming coagulated by heat, a property which it retains so far as to communicate a degree of opacity to its solution in water, when it forms only $\frac{1}{1000}$ part of its weight. A solution of the same strength has its albumen precipitated by the oxymuriate of mercury, and this test will indicate its presence when composing no more than $\frac{1}{2000}$ of the mixture. The tanning principle, the aqua lithargyri acetati, the nitrate of silver, and the nitro-muriate of gold, are all tests of the presence of albumen nearly as minute as the oxymuriate of mercury, but they are less valuable, because their effects are not confined to albumen. The nitro-muriate of tin and alum are also precipitants of albumen, but they are less delicate in their operation than the reagents enumerated above.

Refumption. Albumen is known by its coagulability, and precip. by ox. m. merc.

The peculiar characteristic of jelly is its property of becoming concrete by cold, and being again rendered fluid by a gentle heat: we have found that its solution in water retains this property when it composes $\frac{1}{100}$ part only of the weight of the fluid. Tan is a still more minute test of jelly than of albumen, but jelly is not in the least degree affected by the oxymuriate of mercury, and may thus in all cases be easily distinguished from it. No effect is produced in jelly by Goulard, and scarcely any by the nitrate of silver, and the nitro-muriate of tin, when it is in a state of such dilution. By means of tan, jelly may be easily detected in a fluid of which it forms only $\frac{1}{1000}$ part.

Jelly is known by its concretion on cooling and its precip. by tan.

Mucus is negative as to the preceding characters; but precip. by aq. lith. acet.

The properties of mucus are principally negative; it is not coagulable by heat, nor capable of becoming gelatinized; it is not precipitable either by the oxymuriate of mercury or by tan, but it may be detected with considerable minuteness by the aqua lithargyri acetati.

Other tests.

It appears therefore that the oxymuriate of mercury, tan, and the aqua lithargyri acetati are the three most valuable tests. The nitro-muriate of tin is a less delicate test of albumen than the oxymuriate of mercury, and is also in some degree affected by jelly. The nitrate of silver appears to be a very nice test of albumen, but it is objectionable in consequence of its being decomposed by the muriate of soda, a salt which is supposed to exist in most of the animal fluids. The nitro-muriate of gold is a delicate test of albumen, but it likewise precipitates jelly.

Order of analysis.

In the analysis of a fluid which is supposed to contain either albumen, jelly, or mucus, the first step is to observe the effect of the oxymuriate of mercury; if this produce no precipitate, we may be certain that the fluid in question contains no albumen. We must next employ the infusion of galls, and if this also cause no precipitate, we may conclude that the animal matter held in solution consists of mucus alone.

Remark.

I have before remarked, that the ideas which I have formed of the nature of jelly and mucus, and the relation which these substances bear to each other, differ materially from those of Mr. Hatchett. It is not indeed without a degree of diffidence that I dissent from so distinguished a chemist; but I conceive that I am justified by the experiments related in this essay.— Mr. Hatchett, in the valuable paper to which I have already referred, speaks of the white of the egg as consisting of pure albumen; but I believe that in this particular he will be found not perfectly accurate.

Animal mucus resembles veget. gum.

There is a great resemblance between the mechanical properties of animal mucus and vegetable gum, and I found that they strongly resemble each other also in their chemical qualities. A solution of gum arabic, containing one grain of gum to 200 grains of water, was not affected either by the oxymuriate of mercury or by tan. With the nitro-muriate of tin and with the nitrate of silver there was only a slight degree of opacity, but with the aqua lithargyri acetati there was a dense precipitate instantly formed.

On

V.

On muſcular Motion. By ANTHONY CARLISLE, *Eſq. F. R. S. being the Croonian Lecture, read before the Royal Society, November 8, 1804.*

(Concluded from Page 201.)

THE loſs of motion and ſenſation from the influence of low temperature, accompany each other, and the capillaries of the vaſcular ſyſtem appear to become contracted by the loſs of animal heat, as in the examples of numbneſs from cold. Whether the ceſſation of muſcular action be owing to the impeded influence of the nerves, or to the lowered temperature of the muſcles themſelves, is doubtful; but the known influence of cold upon the ſenſorial ſyſtem, rather favours the ſuppoſition that a certain temperature is neceſſary for the tranſmiſſion of nervous influence, as well as ſenſation.

Cold deſtroys mobility and ſenſation.

The hybernating animals require a longer time in drowning than others. A full grown hedge-hog was ſubmerſed in water at 48°, and firmly retained there; air-bubbles began inſtantly to aſcend, and continued during four minutes; the animal was not yet anxious for its liberty. After ſeven minutes it began to look about, attempting to eſcape; at ten minutes it rolled itſelf up, only protruding the ſnout, which was haſtily retracted on being touched with the finger, and even the approach of the finger cauſed it to retract. After fifteen minutes complete ſubmerſion, the animal ſtill remained rolled up, and withdrew its noſe on being touched. After remaining thirty minutes under water, the animal was laid upon flannel, in an atmoſphere of 62°, with its head inclined downwards; it ſoon began to relax the ſphincter muſcle which contracts the ſkin, ſlow reſpirations commenced, and it recovered entirely, without artificial aid, after two hours. Another hedge-hog ſubmerſed in water at 94°, remained quiet until after five minutes; about the eighth minute it ſtretched itſelf out, and expired at the tenth. It remained relaxed, and extended, after the ceſſation of the vital functions; and its muſcles were relaxed, contrary to thoſe of the animal drowned in the colder water.

Hybernating animals not eaſily drowned. Experiment with the hedge-hog.

The irritability of the heart is inſeparably connected with reſpiration. Whenever the inhaled gas differs in its properties from

Connection of irritability with reſpiration, &c.

from the common atmofphere, the mufcular and fenfible parts of the fyftem exhibit the change: the actions of the heart are altered or fufpended, and the whole mufcular and fenforial fyftems partake of the diforder: the temperature of animals, as before intimated, feems altogether dependant on the refpiratory functions, although it ftill remains uncertain in what manner this is effected.

Diftribution of heat by the blood.

The blood appears to be the medium of conveying heat to the different parts of the body; and the changes of animal temperature in the fame individual at various times, or in its feveral parts, are always connected with the degree of rapidity of the circulation. It is no very wide ftretch of phyfiological deduction to infer, that this increafed temperature is produced by the more frequent expofure of the mafs of blood to the refpiratory influence, and the fhort time allowed in each circuit for the lofs of the acquired heat.

Coagulation of that fluid.

The blood of an animal is ufually coagulated immediately after death, and the mufcles are contracted; but, in fome peculiar modes of death, neither the one, nor the other of thefe effects are produced: with fuch exceptions, the two phenomena are concomitant.

Heat delays the laft coagulation and the contractions of the mufcles, &c.

A preternatural increafe of animal heat delays the coagulation of the blood, and the laft contractions of the mufcles: thefe contractions gradually difappear, before any changes from putrefaction are manifefted; but the cup in the coagulum of blood does not relax in the fame manner; hence it may be inferred, that the final contraction of mufcles is not the coagulation of the blood contained in them; neither is it a change in the reticular membrane, nor in the blood-veffels, becaufe fuch contractions are not general throughout thofe fubftances. The coagulation of the blood is a certain criterion of death. The reiterated vifitations of blood are not effential to mufcular irritability, becaufe the limbs of animals, feparated from the body, continue for a long time afterwards capable of contractions, and relaxations.

The chemical combinations of living matter are tranfient, and not renewable.

The conftituent elementary materials of which the peculiar animal and vegetable fubftances confift, are not feparable by any chemical proceffes hitherto inftituted, in fuch manner as to allow of a recombination into their former ftate. The compofition of thefe fubftances appears to be naturally of tranfient duration, and the attractions of the elementary materials which form

form the grofs fubftances, are fo loofe and unfettled, that they are all decompofed without the intervention of any agent, merely by the operation of their own elementary parts on each other.

An extenfive difcuffion of the chemical properties attaching to the matter of mufcle would be a labour unfuited to this occafion; I fhould not, however, difcharge my prefent duty, If I omitted to fay, that all fuch inveftigations can only be profitable when effected by fimple proceffes, and when made upon the raw materials of the animal fabric, fuch, perhaps, as the albumen of eggs, and the blood. But, until by fynthetical experiments the peculiar fubftances of animals are compofed from what are confidered to be elementary materials, or the changes of organic fecretion imitated by art, it cannot be hoped that any determinate knowledge fhould be eftablifhed upon which the phyfiology of mufcles may be explained. Such refearches and inveftigations promife, however, the moft probable ultimate fuccefs, fince the phenomena are neareft allied to thofe of chemiftry, and fince all other hypothefes have, in their turns, proved unfatisfactory.

Difficulties attending chemical refearch into thefe objects.

Facts and Experiments tending to fupport and illuftrate the preceding Argument.

An emaciated horfe was killed by dividing the medulla fpinalis, and the large blood-veffels under the firft bone of the fternum.

Temperature of the primary fluids in different animals. The horfe.

The temperature of the flowing blood was 103°

Spleen - -	103
Stomach - -	101
Colon - -	98
Bladder of urine	97
Atmofphere -	30.

Three pigs, killed by a blow on the head, and by the immediate divifion of the large arteries and veins, entering the middle of the bafis of the heart, had the blood flowing from thefe veffels of 106, 106½, and 107°; the atmofpheric temperature being at 31°.

Pigs.

An ox, killed in a fimilar manner, the blood 103; atmofphere 50°.

An ox.

Three fheep, killed by dividing the carotid arteries, and internal jugular veins: their blood 105, 105, 105½°; atmofphere 41°.

Sheep.

Frogs.

Three frogs, kept for many days in an equable atmofphere at 54°; their ftomachs 62.°

Fluid of dropfy.

The watery fluid iffuing from a perfon tapped for dropfy of the belly 101°: the atmofphere being 43°, and the temperature of the fuperficies of the body at 96°.

Thefe temperatures are confiderably higher than the common eftimation.

Experiment to fhew that the volumes of mufcles are increafed during action.

A man's arm being introduced within a glafs cylinder, it was duly clofed at the end which embraced the head of the humerus; the veffel being inverted, water at 97° was poured in, fo as to fill it. A ground brafs plate clofed the lower aperture, and a barometer tube communicated with the water at the bottom of the cylinder. This apparatus including the arm, was again inverted, fo that the barometer tube became a gage, and no air was fuffered to remain in the apparatus. On the flighteft action with the mufcles of the hand, or fore-arm, the water afcended rapidly in the gage, making librations of fix and eight inches length in the barometer tube, on each contraction and relaxation of the mufcles.

Crimping of fifh cannot be effected after rigidity of death.

The remarkable effects of crimping fifh by immerfion in water, after the ufual figns of life have difappeared, are worthy attention; and whenever the rigid contractions of death have not taken place, this procefs may be practifed with fuccefs. The fea fifh deftined for crimping are ufually ftruck on the head when caught, which, it is faid, protracts the term of this capability; and the mufcles which retain this property longeft are thofe about the head. Many tranverfe fections of the mufcles being made, and the fifh immerfed in cold water, the contractions called crimping take place in about five minutes; but, if the mafs be large, it often requires thirty minutes to complete the procefs.

Experiment. Two fifh were fcored and one of them crimped. Its fpecific gravity, and alfo its abfolute weight, was increafed by crimping.

Two flounders, each weighing 1926 grains, the one being in a ftate for crimping, the other dead and rigid, were put into water at 48°, each being equally fcored with a knife. After half an hour, the crimped fifh had gained in weight 53 grains, but the dead fifh had loft 7 grains. The fpecific gravity of the crimped fifh was greater than that of the dead fifh, but a quantity of air-bubbles adhered to the furfaces of the crimped mufcles, which were rubbed off before weighing; this gas was not inflammable.

The

The ſpecific gravity of the crimped fiſh - - 1,105
of the dead fiſh, after an equal
immerſion in water - 1,090.

So that the acceſſion of water ſpecifically lighter than the muſcle of fiſh, did not diminiſh the ſpecific gravity of crimped muſcle, but the contrary: a proof that condenſation had taken place.

Other ſubſtances crimped with like effect.

A piece of cod-fiſh weighing twelve pounds, gained in weight, by crimping, two ounces avoirdupois; and another leſs vivacious piece, of fifteen pounds, gained one ounce and half*.

The hinder limb of a frog, having the ſkin ſtripped off, and weighing $77\frac{1}{10}$ grains, was immerſed in water at 54°, and ſuffered to remain nineteen hours, when it had become rigid, and weighed $100\frac{1}{4}$ grains. The ſpecific gravity of the contracted limb had increaſed, as in the crimped fiſh.

Six hundred and thirty grains weight of the ſubſcapularis muſcle of a calf, which had been killed two days from the 10th of January, was immerſed in New River water at 45°. After ninety minutes, the muſcle was contracted, and weighed in air 770 grains: it had alſo increaſed in ſpecific gravity, but the quantity of air-bubbles formed in the interſticial ſpaces of the reticular membrane made it difficult to aſcertain the degree.

Fibrils of muſcles contract by application of water.

Some of the ſmalleſt faſciculi of muſcular fibres from the ſame veal, which had not been immerſed in water, were placed on a glaſs plate, in the field of a powerful microſcope, and a drop of water thrown over them, at the temperature of 54°, the atmoſphere in the room being 57°. They inſtantly began to contract, and became tortuous.

On confining the ends of another fibril with little weights of glaſs, it contracted two-thirds of its former length, by ſimilar treatment. The ſame experiment was made on the muſcular fibres of lamb and beef, twelve hours after the animals had been killed, with the like reſults. Neither

* I am informed that the crimping of freſh water fiſhes requires hard water, or ſuch as does not ſuit the purpoſes of waſhing with ſoap. This fact is ſubſtantiated by the practice of the London fiſhmongers, whoſe experience has taught them to employ pump water, or what is commonly called hard water.

but not by acid or faline water.

vinegar, nor water faturated with muriate of foda, nor ftrong ardent fpirit, nor olive oil, had any fuch effect upon the mufcular fibres.

Cold renders mufcles torpid.

The amphibia, and coleopterous infects, become torpid at 34°. At 36° they move flowly, and with difficulty; and, at a lower temperature their mufcles ceafe to be irritable. The mufcles of warm-blooded animals are fimilarly affected by cold.

Mufcles of frogs irritable after freezing.

The hinder limbs of a frog were fkinned and expofed to cold at 30°, and the mufcles were kept frozen for eight hours, but on thawing them, they were perfectly irritable.

The fame procefs was employed in the temperature of 20°. and the mufcles kept frozen for twelve hours, but that did not deftroy the irritability.

Heat deprives mufcles of their irritability;

In the heat of 100°, the mufcles of cold-blooded animals fall into the contractions of death; and at 110°, all thofe of warm blood, as far as thefe experiments have been extended. The mufcles of warm-blooded animals, which always contain more red particles in their fubftance than thofe of cold blood, are foon deprived of their irritability, even although their relative temperatures are preferved; and refpiration in the former tribe is more effential to life than in the latter.

as do poifons,

Many fubftances accelerate the ceffation of irritability in mufcles when applied to their naked fibrils, fuch as all the narcotic vegetable poifons, muriate of foda, and the bile of animals; but they do not produce any other apparent change in mufcles, than that of the laft contraction. Difcharges of electricity paffed through mufcles, deftroy their irritability, but leave them apparently inflated with fmall bubbles of gas; perhaps fome combination obtains which decompofes the water.

and electricity.

Experiments on mufcles immerfed in fluids.

The four feparated limbs of a recent frog were fkinned, and immerfed in different fluids; *viz.* No. 1, In a phial containing fix ounces by meafure of a faturated aqueous folution of liver of fulphur made with potafh; No. 2, In a diluted acetic acid, confifting of one drachm of concentrated acid to fix of water; No 3, in a diluted alkali, compofed of cauftic vegetable alkali one drachm, of water fix ounces; No. 4, in pure diftilled water.

The

The phials were all corked, and the temperature of their contents was 46°.

The limb contained in the phial No. 1, after remaining twenty minutes, had acquired a pale red colour, and the muſcles were highly irritable.

The limb in No. 2, after the ſame duration, had become rigid, white, and ſwollen; it was not at all irritable. By removing the limb into a diluted ſolution of vegetable alkali, the muſcles were relaxed, but no ſigns of irritability returned.

No. 3, under all the former circumſtances, retained its previous appearances, and was irritable, but leſs ſo than No. 1.

No. 4 had become rigid, and the final contraction had taken place.

Muſcular irritability deſtroyed by other cauſes.

Other cauſes of the loſs of muſcular irritability occur in pathological teſtimonies, ſome examples of which may not be ineligible for the preſent ſubject. Workmen whoſe hands are unavoidably expoſed to the contact of white lead, are liable to what is called a palſy in the hands and wriſts, from a torpidity of the muſcles of the fore arm. This affection ſeems to be decidedly local, becauſe, in many inſtances, neither the brain, nor the other members, partake of the diſorder; and it ofteneſt affects the right hand. An ingenious practical chemiſt in London has frequently experienced ſpaſms and rigidity in the muſcles of his fore arms, from affuſions of nitric acid over the cuticle of the hand and arm. The uſe of mercury occaſionally brings on a ſimilar rigidity in the maſſeter muſcles.

Leſs blood flows through a contracted muſcle.

A ſmaller quantity of blood flows through a muſcle during the ſtate of contraction, than during the quieſcent ſtate, as is evinced by the pale colour of red muſcles when contracted. The retardation of the flow of blood from the veins of the fore arm, during venæſection, when the muſcles of the limb are kept rigid, and the increaſed flow after alternate relaxations, induces the probability, that a temporary retardation of the blood in the muſcular fibrils takes place during each contraction, and that its free courſe obtains again during the relaxation. This ſtate of the vaſcular ſyſtem in a contracted muſcle, does not, however, explain the diminution of its bulk, although it may have ſome influence on the limb of a living animal.

When

A contracted muscle is less sensible.

When muscles are vigorously contracted, their sensibility to pain is nearly destroyed; this means is employed by jugglers for the purpose of suffering pins to be thrust into the calf of the leg, and other muscular parts with impunity: it is indead reasonable to expect, *a priori*, that the sensation, and the voluntary influence, cannot pass along the nerves at the same time *.

Moral causes influence the muscles in the human species.

In addition to the influences already enumerated, the human muscles are susceptible of changes from extraordinary occurrences of sensible impressions. Long continued attention to interesting visible objects, or to audible sensations, are known to exhaust the muscular strength: intense thought and anxiety, weaken the muscular powers, and the passions of grief and fear produce the same effect suddenly: whilst the contrary feelings, such as the prospect of immediate enjoyment, or moderate hilarity, give more than ordinary vigour.

Mental as well as muscular actions may by habit become automatic,

It is a very remarkable fact in the history of animal nature, that the mental operations may become almost automatic, and, under such habit, be kept in action, without any interval of rest, far beyond the time which the ordinary state of health permits, as in the examples of certain maniacs, who are enabled without any inconvenience, to exert both mind and body for many days incessantly. The habits of particular modes of labour and exercise are soon acquired, after which, the actions become automatic, demand little attention, cease to be irksome, and are effected with little fatigue: by this happy provision of nature, the habit of industry becomes a source of pleasure, and the same appears to be extended to the docile animals which co-operate with man in his labours.

and then give little fatigue.

Voluntary and involuntary muscles.

Three classes of muscles are found in the more complicated animals. Those which are constantly governed by the will, or directing power of the mind, are called voluntary muscles. Another class, which operate without the consciousness of the mind, are denominated involuntary; and a mixed kind occur in the example of respiratory muscles, which are governed by the will to a limited extent; nevertheless the exigencies of the animal

* I have often observed that a small electric shock may be received without pain through the muscles of the fore arm; but I imagined it to be owing to the want of power in such a shock to increase the contraction.—N.

animal feelings eventually urge the refpiratory movements in defpight of the will. Thefe laft mufcles appear to have become automatic by the continuance of habit.

Voluntary actions require education; the involuntary are caufed by ftimuli.

The ufes of voluntary mufcles are attained by experience, imitation, and inftruction: but fome of them are never called into action among Europeans, as the mufcles of the external ears, and generally the occipito-frontalis. The purely involuntary mufcles are each acted upon by different fubftances, which appear to be their peculiar ftimuli; and thefe ftimuli co-operate with the fenforial influence in producing their contractions: for example, the bile appears to be the appropriate ftimulus of the mufcular fibres of the alimentary canal below the ftomach, becaufe the abfence of it renders thofe paffages torpid. The digefted aliment, or perhaps the gaftric juice in a certain ftate, excites the ftomach. The blood ftimulates the heart; light the iris of the eye, and mechanical preffure feems to excite the mufcles of the œfophagus. The laft caufe may perhaps be illuftrated by the inftances of compreffion upon the voluntary mufcles, when partially contracted, of which there are many familiar examples. Probably the mufcles of the officula auditûs are awakened by the tremors of found; and this may be the occafion of the peculiar arrangement obfervable in the chorda tympani, which ferves thofe mufcles.

Stimuli feem to excite fenforial power in fome refpect refembling the paffions.

Thefe extraneous ftimuli feem only to act in conjunction with the fenforial power, derived by thofe mufcles from the gangliated nerves, becaufe the paffions of the mind alter the mufcular actions of the heart, the alimentary canal, the refpiratory mufcles, and the iris; fo that probably the refpective ftimuli already enumerated, only act fubferviently, by awakening the attention of the fenforial power, (if that expreffion may be allowed,) and thereby calling forth the nervous influence, which, from the peculiar organization of the great chain of fympathetic nerves, is effected without confcioufnefs: for, when the attention of the mind, or the more interefting paffions prevail, all the involuntary mufcles act irregularly, and unfteadily, or wholly ceafe. The movements of the iris of the common parrot is a ftriking example of the mixed influence.

Lower tribes of animals act by external excitements.

The mufcles of the lower tribes of animals, which are often entirely fupplied by nerves coming from ganglions, appear of this clafs; and thus the animal motions are principally regulated

lated by the external ftimuli, of which the occurrence feems to agree with the animal neceffities: but the extenfive illuftrations which comparative anatomy affords on this point, are much too copious for any detail in this place.

The nervous power feems conftantly active through life.

There are two ftates of mufcles, one active, which is that of contraction, the other, a ftate of ordinary tone, or relaxation, which may be confidered paffive, as far as it relates to the mind; but the fenforial or nervous power feems never to be quiefcent, as it refpects either the voluntary or involuntary mufcles during life. The yielding of the fphincters appears to depend on their being overpowered by antagonift mufcles, rather than on voluntary relaxation, as is commonly fuppofed.

I have now finifhed this endeavour to exhibit the more recent hiftorical facts connected with mufcular motion.

Conclufion.

It will be obvious to every one, that much remains to be done, before any adequate theory can be propofed. I have borrowed from the labours of others, without acknowledgement, becaufe it would be tedious to trace every fact, and every opinion to its proper authority: many of the views are perhaps peculiar to myfelf, and I have adduced many general affumptions and conclufions, without offering the particular evidence for their confirmation, from a defire to keep in view the remembrance of retrofpective accounts, and to combine them with intimations for future refearch. The due cultivation of this interefting purfuit cannot fail to elucidate many of the phenomena in queftion, to remove premature and ill-founded phyfiological opinions, and eventually to aid in rendering the medical art more beneficial, by eftablifhing its doctrines on more extenfive and accurate views of the animal economy.

VI.

On the Meafure of Mechanic Power. In a Letter from Mr. J. C. Hornblower.

To Mr. NICHOLSON.

Dear Sir,

Meafure of mechanic power or effect.

I AM glad to find that fomebody has feconded my motion concerning *horfe power*, and I hope the fubject will not be dif-miffed miſſed until it has paſſed the unanimous aſſent of both theoretic and practical mechanics; and I muſt here expreſs my acknowledgments to Mr. Gregory for his improved method of deciding the queſtion. It is of abſolute importance, that the draught be in a circular direction, and alſo that the radius of the circle be given; for no poſition was ever more demonſtrable than that the leſs the radius the leſs can a horſe (whoſe ſides are equal) exert his faculty of traction.

Meaſure of mechanic power or effect.

But I muſt beg leave to uſe a little freedom (in no wiſe unbecoming, I hope,) in adverting to what Mr. Gregory in conjunction with Profeſſor Robiſon has advanced on the ſubject of Mr. Smeaton's mode of defining *mechanic powers* and *mechanical effect*; and I am ſurpriſed that among men of talent and aſſiduity there can be a difference of opinion. It ſeems to me, that if any reaſon can be found, it muſt be that we do not underſtand the ſubject, and, perhaps I may give a decided proof of it in what I ſhall advance concerning it.

However, I am ſure that when a ball of caſt iron, of twenty inches diameter is elevated by means of a pinion and wheel connected to the ſides of a pair of ſheers, and left to hang there a little while,—if I cut the rope that ſuſtains it, it will fall freely a certain height, in a certain portion of time, and would daſh a faulty cylinder of a ſteam engine in pieces. And I am ſo well ſatisfied that there was a certain tendency in this ball to fall towards the centre of the earth, that I need not take a moment to examine the truth of it; but that the deſtruction of the cylinder was occaſioned by the ball falling from the point to which it was raiſed, (how it was raiſed is no part of the ſubject.) Then I ſay, that if the ball had not fallen from the height it was, or if the ball had not been ſo heavy as it was, or if it had had its velocity retarded by any means, the cylinder would not have been broken. I will add, that had this ball been a true ſphere, and the cylinder had not been there, but a certain curved ſurface in its place, which ſhould have received the ball to prevent it impinging in the line of deſcent, it would have been turned out of its courſe conformably to the nature and poſition of the curve: and all that is above common apprehenſion in this matter, is, that while the ball was falling, its velocity was increaſing every inſtant, and that when it met with the curve (if it gave it an horizontal courſe) it would proceed with uniform velocity juſt

Meafure of mechanic power or effect.

juft twice the fpace through which it fell, allowing the fame time for this horizontal courfe as it had in falling its perpendicular height, and that its tendency is to continue that courfe for ever.

From this ftatement of the law of bodies in motion, (fo far as it goes) I think we need not be very diffident in faying, that it is from fimilar facts, (though lefs philofophically obferved) that we obtain our primary ideas of motion. The apple falling from the tree is a very good inftance to the point, and it would not require a very extraordinary ftretch of genius to apply fuch an accident as that to any thing like the pile engine, ftamping prefs, &c.

But there are certain fpeculative mechanics, who in their mode of accounting for effects like what are here ftated, have chofen to adopt terms of a very different import, and for fome latent reafon wifh to keep gravitation out of fight. The writer of the article Dynamics in the Supplement of the Encyclopedia Britannica, calls it preffure, and by his way of philofophifing in the explanation of the meafure of mechanic power, has (in my opinion) laboured to make it as myfterious as poffible.

I muft for the fake of thofe of your readers who have not the work to refer to, quote a few paffages now and then from the above popular work, and here I would refer them to the article on Machinery, where he begins by ftating, that different notions have been entertained on this fubject by Leibnitz, des Cartes, and other eminent mechanics of the laft century, and adds, "*that fome of the moft eminent practitioners of the prefent times (for we muft include Mr. Smeaton in the number) have given meafures of mechanical power in machinery, which we think inaccurate, and tending to erroneous conclufions and maxims.*"

He then proceeds to explain and demonftrate the true meafure of mechanic power, and he begins by fuppofing a man preffing uniformly on a mafs of matter for a certain time, and going on with the fubject takes occafion to diftinguifh between *the weight of a body* and *its heavinefs!* and, towards the latter end of that fection he comes to fome fort of a conclufion of the fubject, fo far as to fay what is the real meafure of mechanic power: I fee I muft make endlefs quotations if I regard the very letter of his argument, but I hope I fhall be excufed; however I will quote the laft paragraph verbatim.

Relating

Relating to the preſſure of the man juſt mentioned, he ſays, "but farther we know, that this preſſure is the exertion *, we have no other notion of our own force, and our notion of gravity, of elaſticity, or any other natural force is the ſame. We alſo know, that the continuance of this exertion fatigues and exhauſts our ſtrength as completely as the moſt violent motion. A dead pull as it is called of a horſe at a poſt fixed in the ground, is a uſual trial of his ſtrength. No man can hold out his arm horizontally for much more than a quarter of an hour, and the exertion of the laſt minutes gives the moſt diſtreſſing fatigue, and diſables the ſhoulder for action for a conſiderable time after. *This is therefore an expenditure of mechanical power in the ſtrict primitive ſenſe of the word.* Of this expenditure we have an exact and adequate effect and meaſure in the quantity of motion produced, that is in the product of the quantity of matter by the velocity generated in it by this exertion. And it muſt be particularly noticed, that the meaſure is applicable even to caſes where no motion is produced by the exertion; that is, if we know that the exertion which is juſt unable to ſtart a block of ſtone lying on a ſmooth pavement, but would ſtart it if increaſed by the ſmalleſt addition, and if we know that this would generate in a ſecond 32 feet of velocity in 100 pounds of matter, we are certain that it was a preſſure equal to the weight of this 100 pounds. It is a good meaſure, though not immediate, and may be uſed without danger of miſtake when we have no other."

Meaſure of mechanic power or effect.

I ſhould not have quoted ſo much of this ſection, if it had not been that I think it contains an unequivocal interpretation of the writers notion of the true meaſure of mechanic power, and at once exhibiting, in my proud opinion, the fallacy of the doctrine in *toto*. What! ſhall mere muſcular exertion, whether of horſe or man, be eſteemed even an auxiliary to get the conception of the nature of the thing; how much leſs then ſhall it be ſet forth as the thing itſelf? In perfect conſonance with this writer, Mr. Gregory, page 152, Philoſophical Journal, Vol. XI. ſays, ſuppoſe that a horſe while ſtanding ſtill, ſuſtains by means of a rope and ſimple fixed pulley, a maſs of an hundred weight, and thus keeps it ſuſpended at the top of a well for the ſpace of a minute; neither the animal nor the weight moves: but ſhall we ſay, in conformity

* Article Machinery, Supplement, 4th ſec.

as

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as it would feem with Mr. Smeaton's meafure, that there is no power expended, no effect produced. On the contrary we know there is a power expended, and that effect, if fufficiently long continued, would completely tire the horfe. Then let us have a *poft* inftead of the horfe, and furely that will not tire, and what will be the confequence then? why then there will be no power expended, and no effect produced; and I beg leave to afk my opponents, what is the power expended when a horfe or other animal is placed there to fuftain the weight? is it any more than the expence of nervous or mufcular action; and has that any analogy with a weight defcending through a given fpace, either uniformly or accelerated? and, I afk again, what is the effect produced more than what is produced by the poft? the horfe does but keep the weight from dropping into the well, and the poft will do the fame; indeed you may fay, that when you hang up your hat, that the pin which fuftains it, prevents it from falling, as does the horfe the mafs in the well, and that therefore there muft be fome power expended on the pin.

It is really difficult to be grave on this occafion; but I feel myfelf reftrained by the magnitude of the fubject, and its importance to the community. Profeffor Robifon fays, when a man holds out his arm horizontally, the exertion towards the end of a quarter of an hour gives the moft diftreffing fatigue, and then fays this is an expenditure of mechanical power, which I fhall take the liberty to deny for the prefent. But is it fuch a mechanical power as Smeaton's, or in fine, is it a power made up of a mafs of matter moving with any determinate velocity either uniform or accelerated? If the learned Profeffor intended to familiarize the doctrine to people of common fenfe, he could not have chofen a more indirect and perplexing example.

But let us attend a little further to the fubject in the fourth fection of the article Machinery, Sup. Ency. Brit. There he fays, that "when a man fupports a weight for a fingle inftant, he certainly balances the preffure or action of gravity on that body," by the way here is a great want of precifion in the expreffion, "*preffure or action of gravity*," as if they were fynonimous terms, whereas *preffure* certainly denotes *repulfion*, if the term will bear any definition at all, and to explain the term *gravity* if it will not admit of *attraction*, I am fure ſure it cannot be called repulſion, but to proceed, " and he continues this action as long as he continues to ſupport it, and we know, that if this body were at the end of a horizontal arm turning round a vertical axis, *the ſame effort* which the man exerted in merely carrying the weight, if now exerted on the body by puſhing it horizontally round the axis, will generate in it the ſame velocity which gravity would generate by its falling freely."

Meaſure of mechanic power or effect.

A more erroneous propoſition was never introduced to the theory or practice of mechanics. What, is there no difference in a man carrying a load on his ſhoulder, and putting it into a truck? or to come nearer the Profeſſor's propoſition, let the man who has to carry two hundred pounds for one mile be permitted to take the weight from his ſhoulders, and reſt it on the arm of any thing like a horſe wheel, perfectly detached from the mill gear, let the gudgeons be oiled, and then let him "*puſh*" it horizontally round its axis until he has travelled a mile.

Now without aſking the man which he likes beſt, let us ſee what he does by placing his load on the arm of the horſe-wheel, and puſhing it round. Why, he certainly overcomes the additional friction which his load has added to the weight of the wheel, and that is all, and if you will let us have gudgeons which have no friction, the man need not to walk far to puſh the horizontal arm into perpetual motion.

But now for the monſtrous concluſion by this propoſition. " If the man's exertion was employed to generate motion inſtead of counteracting gravity, he would generate during that minute the ſame motion that gravity would; that is 60×32 feet velocity per ſecond in a maſs of 30 pounds. There would be 30 pounds of matter moving with the velocity of 1920 feet per ſecond. We would expreſs this production or effect by 30×1920, or by 57600 as the meaſure of the man's exertion during the minute."

Here is evidently a typographical error, *ſecond* for *minute*, but when we admit ſuppoſitions for the ſake of illuſtration, there ought to be ſome conformity in the ſuppoſition to the fact it is intended to illuſtrate: then I would aſk, where's the man who can generate 32 feet of velocity in a maſs of 30 pounds in one ſecond? to be ſure he can let it fall a ſecond, but he cannot *carry* it 32 feet in a ſecond; but he ſays, " we

Measure of mechanic power or effect.

would express the production or effect by 30 × 1920 or 5760 as the measure of the man's exertion during the minute." Sir, it is more than even one of Boulton and Watts' horses can do.—Well may he say, "such an exertion will completely exhaust a man's strength."

He then goes on to consider "*more narrowly* what a man *really does*, when he performs what Mr. Smeaton allows to be the production of a measurable mechanical effect. Suppose a weight of 30 pounds hanging by a cord which passes over a pulley, and that a man taking this cord over his shoulder, turns his back to the pulley, and walks away from it, we know that a man of ordinary force will walk along raising this weight at the rate of about sixty yards in a minute, or a yard in every second, and that he can continue to do this for eight or ten hours from day to day, and that this is all he can do without fatigue. Here are 30 pounds raised uniformly 180 feet in a minute, and Mr. Smeaton would express this by 30 × 180 or 5400, and would call this the measure of the mechanical effect, and also of the expenditure of power. This is very different from our measure 57600."—Yes, but I hope not the less conclusive on that account.

It is wholly incomprehensible to me why those men (who have certainly a right to controvert any proposition which appears to them erroneous) should take up the subject, assuming points which the doctrine advanced by Mr. Smeaton has nothing to do with. It is clear that all this animal exertion comes at last to the law of bodies falling in accelerated velocity, which Smeaton allows to be a distinct consideration, as he says, "if the weight descends quickly, it is sensibly compounded with another law, viz. the law of acceleration by gravity." But how inconsistent is it to go about to elucidate the laws of bodies in motion by the action of a man or a horse. What is the expenditure of animal power but a waste of what has been usually termed by anatomists nervous spirits, or perhaps an inceptive disorganization of the constituent parts of the ligaments and muscles? in short, we may compare it to contractions, inflammation, and gangrene, but never to the momentum acquired by a body moving through a certain space in a certain time. It is remarkable, that all this reasoning is about that of which Mr. Smeaton has never said a word, except in his illustration of the mechanic power

necessary

neceſſary to give velocity to heavy bodies, where he ſuppoſes a man puſhing an iron ball on an extended plane, and this not to try his muſcular force, or to ſee what he can do without tiring, but merely in elucidation of the doctrine he ſets out with. Meaſure of mechanic power or effect.

I have not time at preſent to purſue the ſubject to the extent I wiſh, and to enter on the ground of the miſtake of this great man: nor let it be imagined for a moment that I have availed myſelf of his everlaſting abſence, to call in queſtion what he has advanced in refutation of a ſuppoſed error. It would have remained as it was unto a diſtant period, had it not been, that I ſee Mr. Gregory advancing the ſame opinion, which no doubt he will defend or deſert.

I am, Dear Sir,

Your obedient Servant,

J. C. HORNBLOWER.

If Mr. Gregory ſhould read this, I ſhall be glad if he will ſet me right as to the identity of *animal exertion* and *mechanic power*.

VII.

An Inveſtigation of all the Changes of the variable Star in Sobieſki's Shield, from five Years Obſervations; exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures reſpecting unenlightened heavenly Bodies. By EDWARD PIGOTT, *Eſq. Abridged from the Philoſophical Tranſactions for* 1805.

THE author begins his memoir with an inveſtigation of the periods of change in the variable ſtar in Sobieſki's ſhield, of which the right aſcenſion was 279° $9\frac{1}{2}'$, and its declination ſouth 5° 56′, for end of June 1796. Its rotation on its axis in 1796 was eſtimated at $62\frac{3}{4}$ days, from a mean of ſix obſervations of its greateſt and leaſt brightneſs. In the preſent paper he gives about 26 ſimilar determinations, moſt of them the reſults of very accurate obſervations made in the year 1796, 1797, 1798, 1799, and 1801. From all theſe reſults it The variable ſtar in Sobieſki's ſhield, revolves on its axis in $61\frac{1}{2}$ days.

it was found that the disagreements between the periods of change, as deduced from its full brightness, were much greater than those deduced from its least illumination. The former gave the mean period 63 days, and the latter $59\frac{3}{4}$; and the mean of these, namely $61\frac{1}{2}$ days, agreeing with the former determination to $\frac{1}{4}$ day, is as near as could be expected in observations of this nature.

The places of full and least brightness do not equally divide the star's circumference; they divide it as 7 to 8.

The author in the next place proceeds to examine some other of the changes to which this star is subject. By tabulated observations through the same series of years, he finds that the time of decrease, from the middle of its full brightness to the middle of its least, is on a mean of 34 days; and that the time of its increase, from the middle of its least brightness to the middle of its full, is in like manner only 27 days. The sum of these numbers amounting to the period 61, shews their probable exactness. These compared and combined with the former determinations of 1796, give a mean of the whole 33 + and 29 − days. As it thus appears that the time of the decrease is longer than that of the increase, it follows of course, that the places of the full and least brightness are not situated at the distance of half the circumference from each other; and the like circumstance Mr. P. affirms is found to be the case with most, if not all the variable stars.

Other variable stars are similarly affected.

The luminous parts are themselves variable.

The next particulars to be reviewed were the durations of its brightness without any perceptible change, while at its maximum and minimum. These determinations required a tolerable succession of observations; where that is not the case he has omitted them in his tables From these it is found in general, that when the degree of brightness at its maximum is less than usual, and its minimum not much decreased, the changes take place but very slowly, and cannot be settled with much accuracy unless the observations have been made frequently and with great attention. He accordingly passes again over the series of years, shewing the dates of its magnitudes when at its full brightness, and also when at its least brightness, and he tabulates at the end of the first part of his paper, all the different changes that have been examined. The words in the first column or compartment describe them; in the second column the present results are exhibited; in the third are the results of the former observations; and in the last column is placed a mean of both computed proportionally, according to the number of observations of each.

TABLE

TABLE VIII.

Table of the general affections of the variable star.

	Days.	Days.	Days, on a mean.
Rotation on its axis - - -	61½	62¾	62—
Duration of brightnefs, at its maximum, without any perceptible change - - -	8+	14	9½
Ditto, when it does not attain its ufual brightnefs - - -	20—	—	—
Duration of brightnefs at its minimum, without any perceptible change - - - -	9—	9	9
Ditto, when it does not decreafe fo much as ufual - - -	20—	—	—
Decreafe in time, from the middle of its full brightnefs to the middle of its leaft - - - -	34	28	33+
Increafe in time, from the middle of its leaft brightnefs to the middle of its full - - -	27+	35	29—
Extremes of its different degrees of brightnefs; with a mean of its ufual variations -	5+ 9 or 0	5+ 7.8	5. 6

The changes in the luminous parts occafion the periods fingly taken to differ from each other.

The author having thus fettled with confiderable precifion thefe effential variations of the ftar, proceeds to examine fome of its other phenomena, particularly one which is common to moft of the changeable ftars, and likewife in fome degree to our fun, namely, that the times of their periodical returns of brightnefs are in general irregular,—a circumftance fo interefting as to engage our attention, and which induced Mr. P. to make the fucceffion of obfervations, in the hope of difcovering its nature and caufe. With this view he has proceeded to tabulate the feries of years, fo as to afcertain the apparent rotations in days from the obferved middle times of its full brightnefs, and alfo from the obferved middle times of its leaft brightnefs, for fingle periods. From thefe it appears, that the periodical returns of brightnefs are uncommonly fluctuating, and that the differences between the extremes are very confiderable; to account for which he offers the following

ing explanations, ſuggeſting previouſly a few plauſible conjectures, and ſome inferences ariſing from the obſervations themſelves *.

Aſſumed poſitions: 1. The ſtars are opake. 2. They have regular rotations. 3. Their luminous appearance is cauſed by an atmoſphere like that of the ſun. 4. The luminous parts are ſparingly diſperſed in the ſtar here treated of:

1ſt. That the body of the ſtars are dark and ſolid.

2d. Their real rotations on their axes are regular.

3d. That the ſurrounding medium is by times generating and abſorbing its luminous particles in a manner nearly ſimilar to what has been lately ſo ingeniouſly illuſtrated by the great inveſtigator of the heavens, Dr. Herſchel, with regard to the ſun's atmoſphere.

4th. That theſe luminous particles are but *ſparingly diſperſed* in the atmoſphere ſurrounding the variable ſtar of Sobieſki, appears from the ſtar being occaſionally diminiſhed to the 6.7 magnitude, and much leſs. July 4, 1799, it was of the 7th; September 15, 1798, and Auguſt 9, 1803, *of the 9th, if not inviſible.* (See Table VII †.) Does not this indicate a very ſmall portion of light on its *darkened hemiſphere?*

5. Probably ſmall patches:

5th. And may we not with much plauſibility conſider them as ſpots, ſomewhat circular, or of no great extent? for even on its *brighteſt hemiſphere* the *duration* of its full luſtre is, on a mean, only 9½ days of the 62, or about one-ſixth and ½ of its circumference. (See Table VIII. page 140.) The dimenſions therefore of the parts enlightened ſeem much circumſcribed, and can be tolerably eſtimated, and conſequently may be repreſented very ſmall, particularly if the *powerful effect of a little light* and *the length of time* a bright ſpot is remaining in view be taken into conſideration.

6. Changeable in their nature:

6th. And a further ground of preſumption that thoſe principal bright parts are but ſlight patches is, that they undergo *perpetual changes*, and alſo that ſuch changes are very viſible to us, for moſt probably they would be imperceptible, were not the bright parts contraſted by conſiderable intervals or diminutions of light.

7. And deducible from the phenomena.

7th, and laſt. We may obtain ſome idea of the *relative ſituation or intervals between* theſe bright parts, by the obſerva-

* The reſt of the paper is given without abridgement, and the author himſelf ſpeaks in the firſt perſon.

† The author refers to his tables in the Tranſactions, of which the abridged reſult has been here given.

tions

tions of the increafe and decreafe of brightnefs, as thereby the changes and times elapfed are pointed out. (See Table V. page 136; and Phil. Tranf. for 1797.)

I have tried practically the effect of the above fuppofitions, by placing fmall white fpots on a dark fphere, which being revolved round reprefented the various changes as nearly as could be expected: proceeding therefore with thefe and other confiderations, I fhall make ideal drawings of the ftar with the fmall illuminated parts in its atmofphere, and apply to them fome of the actual obfervations from both the preceding tables, having always in view that each period may, more or lefs, require a different difpofition of fpots, in confequence of their conftant changeability. Experiment with a fphere.

1ft View.

Plate XIII. Fig. 1, A B, the ftar's polar axis, round which its rotation takes place in 62 days from C to D. View of the ftar at its greateft brightnefs.

C D, its equator, the 360 degrees of which being revolved in 62 days, gives nearly $5\frac{3}{4}$ degrees for each day's motion; the brighteft part or fpot is reprefented as centrally facing us, and accordingly fhewing the ftar in its greateft luftre. Were this bright fpot and the other parts to remain *unchangeable*, they would after having completed the revolution of 360 degrees, or 62 days, (the ftar's rotation on its axis,) appear again as at firft, and at every return continue to give exact periodical times, as was nearly the cafe in 1799, between Auguft and October, (fee Table IX. p. 142); but if the fpot becomes obfcure, and another brightens up in a different place, this latter will make the ftar appear at its next full fplendour either fooner or later than the real rotation according to its pofition, thus,

2d View.

Fig. 1. A full brightnefs having been fhewn by the fame fpot, it afterwards lofes its light, and another as bright is produced 5 days motion (or 29 degrees) preceding it at E, fee Fig. 2. This latter, when turned centrally to the earth, will appear 5 *days fooner* than the former one, now obfcured, (here marked P,) and fhow the ftar at its full luftre, making the rotation 57 days inftead of 62, which was the cafe in 1796, the obferved revolution between September 17 and November 13. (See Table IX.) Anomaly from another bright fpot caufing an apparently fhort period.

3d View.

Another caufing a period apparently long.

Fig. 3. We will now apply a cafe of an interval of too great length, that of 72 days: the fpot *m* alone having fhewn us the ftar in its full luftre, its light difappears during the revolution, and another brightens forth ten days (or 58 degrees) *following it* at H; when *m* returns to face us again in 62 days it being obliterated, the ftar will appear obfcured, and not recover its fplendour until the new brightened part H becomes central, which being *ten days later* than the pofition in which *m* was feen, makes the revolution 72 days inftead of 62, as was obferved between July 14 and September 24, 1801. (See Table IX.) In the above cafe the alterations took place while behind the ftar, otherwife fome irregularities would have been perceived, as will later be noticed. The fame reafoning with proper alterations will, I apprehend, account for the other revolutions, yet I fhall foon again refume the fubject with regard to a *feries* of the greateft irregularities; at prefent let us proceed to take a few views of the intervals of its *leaft brightnefs*, which, contrary to my expectation, I find much more difficult to explain than thofe of the full, although the refults difagree lefs among themfelves. The darkened face of the ftar is here reprefented with a few fmall changeable bright fpots, placed in general at a proper diftance, fo as to keep up an uninterrupted increafe and decreafe of light with regard to us, and are alfo made to correfpond with feveral other obfervations.

4th View.

Greateft period explained; between intervals of leaft brightnefs.

Fig 4 is to explain the greateft interval of 74 days, between July 4th, and September 16th 1799. (See Table X.) The darkened hemifphere here exhibited in its *minimum* July 4th, with the following fpots, *w* nearly gone off, next a fmall one *l*, then another P of a fimilar fize, preceding the centre a day or two, (or a few degrees, (and laftly a bright one at D, juft appearing. During the rotation, D lofing its light and the P becoming *much brighter*, the ftar at its next return in 62 days, when at its firft pofition, muft of courfe appear much brighter, (See fig. 5) but by the retiring of *l* and P continues to diminifh in luftre till the appearance of fome large fpot from the other hemifphere; which taking place 12 days afterwards, will, (when this time is added to the 62 already revolved) make the revolution

revolution of 74 days, as required; for a view of a ſhort interval, for the preſent let that of 56 days be taken between Auguſt 21ſt and October 16th 1801. (See Table X.)

5th View.

The leaſt brightneſs or *minimum* is repreſented by fig. 6, when the bright ſpots *y* and *x* at each extremity of the equatorial diameter are mutually but juſt in ſight and a minute one, *r* alone on its ſurface preceding *y* by 6 days motion: *n n*, are other middling ſized ſpots near *x*, but preceding it; they cannot for the preſent be ſeen, being on the oppoſite or bright hemiſphere. The ſpot *x* during the ſtars revolution having loſt its light, and *r* being conſiderably increaſed, the next *minimum* will be between *n n* and *r*, (inſtead of *x* and *y*.) See fig. 7; and by the retiring of *n n* the *diminution* of the ſtar's light will continue to take place only until the re-appearance of *r*, at the place where *y* was, which being 6 days ſooner than the former poſition, (See fig. 6,) reduces the rotation to 56 days. All the foregoing views are from unconnected periods, where only the ultimate returns of each appearance have been attended to; but now, I ſhall examine a long interval with many intermediate changes, that between June 18th, and September 17th 1796, wherein are included the moſt intricate irregularities and viciſſitudes: theſe obſervations are already pointed at full length in the Philoſophical Tranſactions for 1797, and therefore can at any time be inſpected: indeed, I then little thought they would ever become of further uſe, but that of ſtating facts, to which, however, I have always been very partial, and particularly ſo, after having experienced the advantage of Maraldi's printed obſervations on the variable ſtar in Hydra, as it was partly by them that I aſcertained the periodical returns of brightneſs of that ſtar, and which flattered me the more, as Maraldi himſelf had been leſs ſucceſsful in the attempt; See Phil. Trans. for 1786. Yet in the preſent Paper I have omitted all ſuch details, being aware they might be thought too voluminious, but hope at ſome future time the Society will honour them with a place in their library. Leaſt brightneſs.

The firſt ſketch, Plate XIV. repreſents, for June 13, 1796, the comparative ſize of the bright ſpots ſuppoſed to ſurround the ſtar, but here extended at full length; the next eight following

Explanation by the figures of the periodical variations and irregularities of light in the ſtar in Sobieſki's ſhield.

are ſpherical views, on an enlarged ſcale, for each quarterly, rotation or leſs, ſhewing the principal changes, as expreſſed in the adjoining remarks, and correſponding with the obſervations; theſe being taken from my printed paper, as already mentioned, are marked in italics. It will be ſeen that the ſpots by which the changes are principally regulated, are placed at equal diſtances, yet intermediate ones might alſo frequently be inſerted without occaſioning any objection, but that of rendering the explanations more complex.

REMARKS ON PLATE III.

Fig. 2. "*June 18th. Full brightneſs Mag. bright 5th,*" before or after which date the ſtar would appear leſs bright, by the ſpot E being removed from the centre, and one of the others out of view.

Fig. 3. "*July 3d, 15 days or ¼ rotation being elapſed ſince June 18th, 5th Mag, a little decreaſed*" by the removal of the brighteſt ſpot E, the *h* being much leſs.

Fig. 4. "*July 19th, 16 days or ¼ rotation 5.6 Mag. ſtill decreaſed,*" N being much leſs than *h*, now gone off. *A ſlight minimum.*"

Fig. 5. "*July 27th, 8 days of the rotation, 5 Mag. rather increaſed*" by the conſiderable increaſe of N ſince four days, with the addition of F, *a ſlight full brightneſs.*

Fig. 6. "*Aug. 3d, 7 days of rotation, 5,6 Mag. decreaſed* by the going off of N, the E, which is now reappearing, being reduced to much leſs than F.

Fig. 7. "*Aug. 19th, 16 days or ¼ rotation, 5.6 Mag. again decreaſed,*" by the removal of F, by E being much leſs, and by the *h* alſo being conſiderably diminiſhed.

Fig. 8. "*Sept 3d, 15 days or ¼ rotation, 6 Mag. ſtill more decreaſed,*" by the *h* being much leſs than E, which is now going off, and N ſcarcely reappearing, *another minimum.*

Fig. 9. "*Sept. 17th, 14 days or near ¼ rotation, 5 Mag. full brightneſs conſiderably increaſed,*" by N having retained its increaſed brightneſs of July 27, and now facing us centrally.

1ſt, Thus are exhibited, the two ſhort intervals of its full brightneſs, one between June 18 and July 27, of 39 days, and the other between July 27 and Sept. 17, of 52 days. See Table IX.

2dly,

2dly, The interval of 46 days between the two *minima* of July 19 and Sept. 3; See Table X.

Explanation by the figures of the periodical variations and irregularities of light in the ſtar in Sobieſki's ſhield.

3dly, The long decreaſe of 38 days between July 27 and Sept. 3, and

4thly, The rapid increaſes of 3 and 14 days between the 19 and 27th of July, and the 3d and 17th of September,

As alſo the other intermediate changes, yet I muſt again repeat, particularly as a few days error may occaſionally proceed from the obſervations, that by theſe ſketches it is not meant to give exact drawings of the ſize, diſtances or alterations of the ſpots, but merely to ſhew how the changes may take place, as, I believe, nothing of the kind has hitherto been offered to the public, either with or without corroborating obſervations; nor do I preſume to think, that the explanations are the only ones or beſt can that be imagined, the more ſo, as they ſolely refer (for greater ſimplicity) to the ſtar's equator, while poſſibly, were the ſpots placed in a northern or ſouthern latitude, or permanent ones near the poles, or were a proper inclination, given to the polar axis, they might be more ſatisfactory: however, the materials themſelves, the *obſervations* and *deductions* will I flatter myſelf ever be acceptable, and contribute to facilitate future conjectures, which from an allowable analogy may extend to ſimilar parts of the ſtarry ſyſtem, with regard to the probability of eſtabliſhing whether any of the moſt *irregular or particular* changes may not *return at fixt periods*, or after a certain number of rotations. I think we can entertain but ſlight hopes of it, owing to the *great fluctuation* of the luminous matter, as ſhewn by the *perpetual varying* of the *apparent* revolutions, magnitudes, &c. See Tab. IX. X. and VII. Still it is natural to ſuppoſe, that ſome parts of the atmoſphere of this ſtar may have a leſs tendency than others to become luminous, ſo as to promote at different times, ſimilar appearances; and indeed that is ſtrongly indicated by the *intervals* of the *minima* being far *more regular* than thoſe of the *full brightneſs*, which, with other reaſons induce us to ſuſpect even that one of its hemiſpheres is leſs favourably conſtituted or qualified, than the other for the generating of theſe particles, although they do occaſionally encroach on both ſides, as appears by the obſervations between June and Auguſt, See Phil. Trans. for 1797, or the eight ſketches of 1796, and likewiſe in 1797, ſee Tab. VII. when during *three months* it was only reduced to the

5 or

5 or 6 Mag. by which the degree of brightneſs that ſurrounded it, muſt have been nearly equal: had the cauſes of varying its light then ceaſed, it would ever have continued to appear as an unchangeable ſtar of the 5 or 6 Mag. and ſuch is the caſe of ſeveral others that *formerly have been variables*, but for many years retain a ſteady brightneſs, as β Geminorum, δ Urſæ majoris, α Draconis, and perhaps that in the Swan's breaſt, while others, after *ſhewing their changes*, have *entirely diſappeared*, owing to a total abſorption of light, as the famous one in Caſſiopea, in Serpentarius of 1604, that near the Swan's head, and doubtleſs many more. Does not this induce us to preſume that there are alſo others, that have *never ſhewn* a glimpſe of brightneſs? Laſtly, *new variables* may become ſo at different periods, by an unuſual and partial increaſe or diminution of their bright parts, as not unlikely was the caſe of ο Ceti, Algol α Herculis, &c. for theſe ſtars being by times very conſpicuous, their changes, had they been always equally great, might have been eaſily noticed by the ancient aſtronomers, who obſerved only with the *naked eye*. A few lines above, I mentioned the probability that there exiſted *primary* inviſible bodies or *unenlightened ſtars* (if I may be allowed the expreſſion) that have ever remained in *eternal darkneſs*; how numerous theſe may be, can never be known. Would it then be too daring or viſionary to ſuppoſe their numbers equal to thoſe endowed with light? particularly when we take into contemplation the ample ſet of bodies viſible only by reflected rays, that appertain to our own ſyſtem, ſuch as the planets, aſteroides, comets, and ſatellites. Do not theſe, although but of a ſecondary nature, lead us to venture on the foregoing more enlarged conjecture; and moreover to ſuſpect, that the *enlightened ſtars* are thoſe that have already attained the higheſt degree of perfection? granting, therefore, ſuch multitudes do really exiſt, cluſters of them, by being collected together as in the milky-way, muſt intercept all more diſtant rays, and if free from any intervening lights, they would appear as *dark ſpaces* in the heavens, ſimilar to what has been obſerved in the Southern Hemiſphere. That ſo few of theſe obſcure places are perceived, may be attributed to their being obliterated by the preſence either of ſome ſcattered ſtars, or of other ſlight luminous appearances.

Changeable ſtars may become permanent; and others diſappear. There may be dark ſtars, perhaps equal in number to the viſible ones.

I have thus fully inveſtigated the nature of this diſtant ſun, a ſingle one among many millions, and ſcarcely perceptible

Our ſun, though variable, is ſtill very luminous;

to the fight, yet of no lefs importance than our own grand luminary. But ours is ftill fupplied abundantly with refplendent particles, while Sobiefki's variable ftar has them moft fparingly difperfed over its fphere: a fcantinefs that apparently muft occafion to its furrounding planets, conftant viciffitudes of uncertain darknefs, and repletion of light and heat. How far more enviable feems our fituation; I mean that which we enjoy at prefent; there being ftrong reafons to believe, that the fun's luminous appearance has been at times confiderably diminifhed; and I have little hefitation in conceiving that it may alfo be reduced at fome future period to fmall patches, and then the apparent irregularities of its periodical rotations, which at prefent are only perceived by the obfervations of trifling dark fpots, would become evidently confpicuous, particularly when feen at a diftance as remote as the variable ftars are from us. But fuch conjectural flights of fancy cannot too foon be dropt. I therefore fhall conclude with obferving, that thefe inquiries on the alterations of light of the ftars have been fo little difcuffed, that it is to be hoped they will not be difcontinued; and although I have already troubled the Society with many papers concerning fuch changes, I neverthelefs propofe, ere long, having the honour of prefenting them with one more, moft probably my laft, on this fubject.

though it may become reduced in future ages.

EDW. PIGOTT.

VIII.

Account of a Luminous Meteor. By a CONSTANT READER.

To Mr. NICHOLSON.

SIR,

HAVING found it frequently ftated, that it might be ufeful, fhould every one who has a fair opportunity of noting with reafonable accuracy, the courfe and altitude of meteors, defcribe their appearances as well as they are able; I fend you the following account to make what ufe of you pleafe.

Defcription of the appearance, courfe, and duration of a luminous meteor.

Laft night (Sunday the 21ft) paffing along the Strand, I ftopped at the door of the Crown and Anchor, the vacant fpace before it lately caufed by the pulling down of houfes

offering

Defcription of the appearance, courfe, and duration of a luminous meteor.

offering a confiderable view of the heavens, at that time fplendid with ftars; I was looking with attention towards the N. W. when fuddenly a meteor from about 35° of height, fhot from the W. by N. It was apparently about the fize of a tennis ball, perhaps hardly fo large, it was followed by a ftream of light which feemed in fpecks, the length of the train was about a degree, that is about twice the apparent diameter of the moon. Its courfe was from North of Weft towards the North, paffing about 10° below the of the Great Bear, which I judge was then about 45° above the horizon. Its motion was majeftic, by no means rapid, I am fure it was full ten feconds in motion, the light not fo piercing as that of a ftar of the firft magnitude, but exceeded that of the fecond, with which I had full opportunity of comparing it. It ran through 30° of the heavens, defcribing an arch of great diameter, its path was convex above, and declining downwards. The extinction of it was at an altitude of about 25° having fallen certainly not more than 10°, I do not think fo much. It very vifibly ftopped before it was extinguifhed. It burft at laft with very few fparks, and its train and itfelf together difappeared in a moment. I had perfect leifure and fpace to obferve its whole courfe, it expired below the fecond pointer, I inftantly drew out my watch, and comparing it this morning with the clock of St. Paul's Cathedral, it was exactly at thirty-one minutes after eleven that I obferved the end of the phenomenon.

I am, Sir,

Your moft obedient humble Servant,

A Constant Reader.

Monday, July 22d, 1805.

IX.

Precipitation of Platina as a Covering or Defence to polifhed Steel, and alfo to Brafs. In a Letter from Mr. J. Stodart.

To Mr. NICHOLSON.

Dear Sir,

Platina is taken from its folution by ether.

YOU kindly favoured me, by inferting in the laft Number of your excellent Journal, an account of a method I have used with succefs, for gilding polifhed fteel with gold; perhaps it may be worth knowing, that a very fimilar procefs may be performed with platina. That metal, in a ftate of folution, is taken up from the acid by agitation with ether, in the way that gold is, though certainly with lefs avidity. The ethereal folution of platina afforded by this procefs, is, like that of gold, depofited on the furface of polifhed iron, or fteel, forming a coat of defence from ruft. It is perhaps a fact of equal importance, that the furface of polifhed brafs is coated with platina by the fame operation that fteel is; namely, by plunging the brafs for an inftant into the ethereal folution. As far as I know, thefe facts have not hitherto been noticed: on the contrary, authors highly refpectable, have from ingenious and well conducted experiments, been led to conclufions very oppofite to thofe I have advanced. Dr. Lewis, to whofe genius and induftry the arts are much indebted, fays, "gold is the only one of the known metals which the ether takes from acids; and hence this fluid affords a ready method of diftinguifhing gold, contained in acid folutions." The fame author gives the following experiment. "Sulphuric ether was poured into a folution of platina, and into a compofition of platina and gold. The vials were ftopt and fhaken, the ether received no colour from the folution of platina, but became inftantly yellow from that of the platina and gold." The only way in which I can account for thefe refults, fo contrary to my experience, is by fuppofing that the platina with which Dr. Lewis made his experiments, was not fo pure as that with which we are now furnifhed. What I ufed was part of an excellent malleable bar, its fpecific gravity I do not exactly know. I am inclined to think it was quite pure. The ether was furnifhed by my friend Mr. Hume, whom I am again happy to thank for his kind and able affiftance. The ethereal folution of platina is of a beautiful pale yellow colour, does not at all ftain the hand, and is precipitated by volatile alcali. The precipitate I have not examined. It may be fulminating, and I have no relifh for explofions. The coat of platina on fteel is of a dull white colour. I have no doubt of its proving quite as good a defence from ruft, as the coat of gold. It is, however, by no means fo beautiful; for which reafon a preference will probably be given to the laft named metal. I have ufed both the gold and platina in coating different

and coats iron or fteel;

and alfo brafs.

Dr. Lewis's experiments gave a contrary effect;

probably becaufe his platina was impure.

The platina coating is lefs beautiful than gold.

different parts of the ſame inſtrument. The effect produced by the contraſt of colour is very beautiful. Whether any of theſe obſervations may be worth communicating through the medium of your moſt uſeful Journal, is a queſtion I beg leave to ſubmit entirely to your judgment. I have not tried any of the eſſential oils with ſolution of platina; further experiments will probably be made with theſe metallic ſolutions, by thoſe who have more time, and a better knowledge of theſe ſubjects. Such purſuits, when the reſults are frankly communicated, promiſe to benefit ſcience, and muſt ultimately prove uſeful to ſociety.

I remain with much reſpect,

Dear Sir, your obliged Servant,

J. STODART.

Strand, July 24, 1805.

X.

Experiments on Wootz. By Mr. DAVID MUSHETT. *From the Philoſophical Tranſactions*, 1805.

(Concluded from Page 204.)

Forging No. 2.

Appearances on forging the cake of wootz, No. 2.

ONE half of this cake was heated to a ſcarlet ſhade, and put under the cutting chiſſel; it was at firſt ſtruck lightly, then reheated, and cut comparatively ſoft; but a ſmall crack had over-run the progreſs of the chiſſel. Its ſoftneſs in cutting was attributed to an evident want of ſolidity. The other half cake felt harder under the hammer, but proved afterwards ſpongy throughout the maſs. In the act of cutting, a looſe pulveriſed matter was diſengaged from ſome of the cells, poſſeſſed of a ſhining appearance.

The fractures obtained in conſequence of the diviſion of the half cakes, preſented a flattiſh cryſtallized appearance, more reſembling very white caſt iron, than ſteel capable of being extended under the hammer. One of the middle cuts was entirely cellular with cryſtallized interiors, and incapable of drawing; the correſponding cut of the other half cake was drawn

drawn into a ſtrait bar three quarters of an inch in breadth, and three-eighths thick, but was covered with cracks and flaws from end to end. The colour of the break was one ſhade lighter than No. 1, it tore leſs out, was equally yolky, and poſſeſſed on the whole an aſpect very unfavourable for good ſteel.

The other two outſide quarters were alſo drawn into ſhape; one under the tilt hammer, and the other by hand. Theſe were more ſolid in the fracture, poſſeſſed fewer ſurface-cracks, ſtood a higher degree of heat, tore out more, and exhibited a ſilky gloſſy grain, at leaſt two ſhades lighter in the colour than the centre pieces.

Forging 3d Cake.

Appearances on forging the cake of wootz, No. 3.

One half of this cake, firſt ſubjected to be cut, was found ſofter than any of the preceding, and exhibited no ſymptom of cracking. The other half was cut at three heats, but found looſe and hollow in the extreme. A conſiderable portion of the ſame brilliant powder, formerly noticed, was here again diſengaged. It was carefully taken up for examination, and found to be very fine ore of iron in a pulvereſcent ſtate, very obedient to the magnet, and without any doubt an unmetallized portion of that from which wootz is made.

This curious circumſtance led me to examine every pore and cell throughout the whole fragments. On the upper ſurface of two of them I found ſmall pits containing a portion of the ore, which had been ſlightly agglutinated in the fire, but ſtill highly magnetic. The upper ſurface of the preſent cake, cloſe by the gate or feeder, contained a large pit filled with a ſtratum of ſemi-fuſed ore, ſurmounted by a maſs of vitrified matter, which bore evident marks of containing calcareous earth.

Thoſe who have devoted ſufficient attention to the affinities of iron and earths for carbon, will be ſurpriſed to find that, on this particular ſubject, the rude fabricators of ſteel in Hindoſtan have got the ſtart of our more poliſhed countrymen in the manufacture of ſteel.

Two bars of wootz were formed from this cake, and theſe in point of quality inferior to any of thoſe formerly produced. The appearance of the metal was more varied, leſs homogeneous, and contained more diſtinct laminæ with ruſty ſurfaces, than either of the two former cakes.

It

It appeared highly probable, from the obfervations that occurred in forging, and in the examination of the cake, that the original proportion of mixture was fuch as would have formed a quality of fteel fofter than No. 1 and 2; but as fteel of fuch foftnefs requires a greater heat to fufe it, than when more fully faturated with carbonaceous matter, it is probable that the furnace had not been fufficiently powerful to occafion complete fufion of the whole mafs, and generate a fteel homogeneous in all its parts.

Forging 4th Cake.

Appearances on forging the cake of wootz, No. 4.

Both halves of this cake cut pleafantly, and with a degree of tenacity and refiftance, mixed at the fame time with foftnefs beyond what was experienced in any of the former cakes. Two quarters of this cake were drawn under the tilt hammer, and one by hand. The refulting bars were nearly perfect. A flight fcale was obfervable upon the bar, from that quarter which contained the figure. The fracture was folid, though not homogeneous as to quality and colour, and it appeared pretty evident, that a confiderable portion of one fide through the whole bar was in the ftate of malleable iron, and of courfe not capable of being hardened. It was a fubject of confiderable regret, that the cake the moft perfect and the moft tenacious of the whole, in the procefs of forging, fhould get an imperfection which rendered it ufelefs for the perfect purpofes of fteel.

Forging 5th Cake.

Appearances on forging the cake of wootz, No. 5.

The firft half of this cake cut uncommonly foft for wootz, but by cracking before the chiffel ftill exhibited a want of proper tenacity. The next half cut equally foft, but with more tenacity. Two quarters of this cake drew readily out under the tilt hammer, and a third was drawn by hand at a bright red, fometimes approaching to a faint white heat. None of the bars thus obtained were uniformly free from cracks and fcale, although the fracture exhibited a fair break of a light blue colour, and the grain was diftinctly marked, and free from yolks.

General Remarks.

Remarks. Wootz appears to be the product of a pecu-

The formation of wootz appears to me to be in confequence of the fufion of a peculiar ore, perhaps calcareous, or rendered highly

highly ſo by mixture of calcareous earth along with a portion of carbonaceous matter. That this is performed in a clay or other veſſel or crucible, is equally preſumable, in which the ſeparated metal is allowed to cool; hence the cryſtallization that occupies the pits and cells found in and upon the under or rounded ſurface of the wootz cakes.

liar ore fuſed, and ſuffered to cool in a clay veſſel;

The want of homogenity, and of real ſolidity in almoſt every cake of wootz, appears to me to be a direct conſequence of the want of heat ſufficiently powerful to effect a perfect reduction; what ſtrengthens this ſuppoſition much is, that thoſe cakes that are the hardeſt, *i. e.* that contain the greateſt quantity of carbonaceous matter, and of courſe form the moſt fuſible ſteel, are always the moſt ſolid and homogenious. On the contrary, thoſe cakes, into which the cutting chiſſel moſt eaſily finds its way, are in general cellular, replete with laminæ, and abound in veins of malleable iron.

by an heat not ſufficient for good fuſion;

It is probable, had the native Hindoſtan the means of rendering his caſt ſteel as fluid as water, it would have occurred to him to have run it into moulds, and by this means have acquired an article uniform in its quality, and convenient for thoſe purpoſes to which it is applied.

which is the reaſon why it was not caſt in moulds.

The hammering, which is evident around the feeder and upon the upper ſurface in general, may thus be accounted for. When the cake is taken from the pot or crucible, the feeder will moſt probably be ſlightly elevated, and the top of the cake partially covered with ſmall maſſes of ore and ſteel iron, which the paucity of the heat had left either imperfectly ſeparated or unfuſed. Theſe moſt probably, to make the product more marketable, are cut off at a ſecond heating, and the whole ſurface hammered ſmooth.

I have obſerved the ſame facts and ſimilar appearances in operations of a like nature, and can account ſatisfactorily for it as follows.

The firſt portions of metal, that are ſeparated in experiments of this nature, contain the largeſt ſhare of the whole carbon introduced into the mixture. It follows of courſe, that an inferior degree of heat will maintain this portion of metal in a ſtate of fluidity, but that a much higher temperature is requiſite to reduce the particles of metal, thus for a ſeaſon robbed of their carbon, and bring them into contact with the portion

portion first rendered fluid, to receive their proportion of the steely principle. Where the heat is languid, the descent of the last portions of iron is sluggish, the mass below begins to lose its fluidity, while its disposition for giving out carbon is reduced by the gradual addition of more iron. An accumulation takes place of metallic masses of various diameters, rising up for half an inch or more into the glass that covers the metal; these are neatly welded and inserted into each other, and diminish in diameter as they go up. The length, or even the existence of this feeder or excrescence, depends upon the heat in general, and upon its temperature at different periods of the same process. If there has been sufficient heat, the surface will be convex and uniformly crystalline; but if the heat has been urged, after the feeder has been formed and an affinity established between it and the steelified mass below, it will only partially disappear in the latter, and the head or part of the upper end of the feeder will be found suspended in the glass that covers the steel.

The same or similar phenomena take place in separating crude iron from its ores, when highly carbonated, and difficult, from an excess of carbon, of being fused.

The division of the wootz cake by the manufacturers of Hindostan, I apprehend is merely to facilitate its subsequent application to the purposes of the artist; it may serve at the same time as a test of the quality of the steel.

Experiments to ascertain the comparative measure of carbon in wootz, by the quantity of lead it reduces from flint glass.

To ascertain, by direct experiment, whether wootz owed its hardness to an extra quantity of carbon, the following experiments were performed with various portions of wootz of common cast steel, and of white crude iron, premising that in operations with iron and its ores, I have always found the comparative measure of carbon best ascertained by the quantity of lead which was reduced from flint glass.

1st Cake.	Grains.
Fragments of wootz - - - - - -	65
Pounded flint glass three times the weight -	195

This mixture was exposed to a heat of 160° Wedgwood, and the wootz fused into a well crystallized spherule of steel. A thin crust of revived lead was found below the wootz, which weighed 9 grains, or $\frac{139}{1000}$ the weight of wootz.

2d Cake

Experiment to ascertain the comparative measure of carbon on wootz, by the quantity of lead it reduces from flint glaſs.

2d Cake.

	Grains.
Fragments of wootz - - - - - -	80
Flint glaſs, ſame proportion as above - -	240

The fuſion of the mixture in this experiment was productive of a maſs of lead weighing 10 grains, equal to $\frac{1}{8}$th the weight of the wootz.

3d Cake.

Fragments of wootz - - - - - -	75
Flint glaſs - - - - - - -	225

The maſs of lead precipitated beneath the ſteel in this experiment, amounted to 9 grains, or $\frac{12}{100}$ the weight of the wootz employed.

4th Cake.

Fragments of wootz - - - - - -	93
Flint glaſs - - - - - - - -	279

Lead obtained, precipitated from the glaſs by means of the carbon of the wootz $14\frac{1}{2}$ grains, equal to $\frac{156}{1000}$ the weight of the wootz.

5th Cake.

Fragments of wootz - - - - - -	69
Flint glaſs - - - - - - -	207

The lead revived in this experiment amounted to 7 grains, which is equal to $\frac{102}{1000}$ the weight of the wootz.

6th. Caſt Steel formed with $\frac{1}{60}$th part of its Weight of Carbon.

Fragments - - - - - - -	90
Cryſtal glaſs - - - - - -	270

Lead revived $8\frac{1}{2}$ grains equal to $\frac{94}{1000}$ the weight of the ſteel introduced.

7th. White caſt Iron dropt while Fluid into Water.

Fragments - - - - - - - -	103
Cryſtal glaſs - - - - - - -	309

The fuſion of this precipitated $23\frac{1}{2}$ grains of lead which is equal to $\frac{228}{1000}$ the weight of the caſt iron.

 Recapitulation

Recapitulation of these Experiments.

Recapitulation of the experiments.

1st cake of wootz revived of lead		,139
2d	ditto	,125
3d	ditto	,120
4th	ditto	,156
5th	ditto	,102
Steel containing $\frac{1}{60}$ of its weight of carbon		,094
Cast iron		,228

Wootz contains more carbon than steel does, and less than cast iron.

It would appear to result from these experiments, that wootz contains a greater proportion of carbonaceous matter, than the common qualities of cast steel in this country, and that some particular cakes approach considerably to the nature of cast iron. This circumstance, added to the imperfect fusion which generally occurs in the formation of wootz, appear to me to be quite sufficient to account for its refractory nature, and unhomogeneous texture.

Its ore is probably very excellent.

Notwithstanding the many imperfections with which wootz is loaded, it certainly possesses the radical principles of good steel, and impresses us with a high opinion of the ore from which it is formed.

The possession of this ore for the fabrication of steel and bar iron, might to this country be an object of the highest importance. At present it is a subject of regret, that such a source of wealth cannot be annexed to its capital and talent. Were such an event practicable, then our East India Company might, in their own dominions, supply their stores with a valuable article, and at a much inferior price to any they send from this country.

XI.

A Memoir on the Webs of Spiders. By C. L. CADET.*

On the natural and medical history of spiders.

SPIDERS have often excited the curiosity of naturalists and the attention of physicians. The former have successfully studied the habits and conduct of these insects; and notwithstanding the repugnance they naturally inspire, these accounts

* Abridged from the Journal de Physique, LVIII. 463.

have

have become interefting, from the induftry with which they extend their webs for feizing their prey, and from obfervations on the multiplicity and arrangement of their eyes, which are geometrically difpofed on a motionlefs head, in a manner conformable to their neceffities. Their combats, the fingularity of their amours, their fenfibility for mufic, and their patience, all conftitute fubjects of wonder in the hiftory of fpiders. Phyficians have examined whether their bite be really venomous, as is generally thought; and they have found only two fpecies productive of danger, namely, the tarantula and avicularia of Cayenne. Swanmerdam, Roffi, and Baglivi have left us little to wifh for in this matter, as the effects of their bite and the remedies are both known.

Spiders' webs a popular remedy for flight wounds.

The webs of fpiders are confidered by the common people as a remedy for wounds; country people often apply them on cuts or flight wounds, and apparently with fuccefs. This property was not of fufficient importance to induce chemifts to analyfe the material; but as there has alfo been attributed to them a febrifuge virtue, fuperior in fome circumftances to the bark, I have thought them entitled to a more particular examination. The following extract is taken from the *Journal d'Economie Rurale*, for Germinal, in the year XII.

Narrative of fevers cured by fpiders internally taken.

"We have feen upwards of thirty years ago, a good prior, the curate of Batheren in Franche Comté, cure all the fevers of his parifh, and of the neighbouring villages, by pills of a ftrange compofition. He went into his barn and formed fmall pills with fpiders' webs, by rolling them between his hands in the ftate he found them. He adminiftered this remedy to his patients in white wine, and very feldom failed to cure. M. Marie de St. Urfin being chief phyfician of the Hotel de Dieu, of Chartres, treated a very obftinate fever in that hofpital. He had employed bitters, the bark, and all the remedies of medical art without fuccefs, when one of the female attendants offered to undertake the cafe with a certainty of cure. When fhe was interrogated concerning her remedy, fhe refufed to mention it. M. de St. Urfin therefore continued to attend his patient for fome days; after which, having a good opinion of the attendant, he determined to put his patient under her care. There was no return of the fever after the firft dofe of the remedy. The phyfician fuppofed that the imagination of the patient, his confidence in a new remedy, and particularly the secrecy, might have suspended the attack, and he waited, but to no purpose, for its return. The attendant encouraged by her success, consented to mention the remedy, which proved to be the same as that of the curate of Batheren."

Supposed to be effected by gelatine.

The editor of the Journal here quoted, being struck with the new experiments of Seguin upon gelatine applied to the treatment of intermitting fevers, suspects that spiders' webs may contain a principle resembling animal jelly. The experiments of Cadet, while they overthrow this supposition, appear to him entitled to the attention of medical men.

Analysis of spiders' webs.

Experiment 1. Spiders' webs triturated in the cold with quick-lime, emit a slight ammoniacal smell. 2. Cold water by digestion on the webs, becomes of a red-brown colour; is slightly precipitated by infusion of nut-galls; is precipitated by acids; and this precipitate is again dissolved when the acids are saturated with ammonia. 3. Spiders' webs cleaned as much as possible from dust and foreign matters, were boiled in distilled water. The decoction smelled like champignons, and lathered by agitation. The undissolved matter was boiled in additional waters, until it gave out nothing more. All these waters being put together and evaporated, let fall their contents in successive pellicles; and at length, by gentle evaporation, a solid extract was had, nearly equal to half the weight of the spiders' webs. 4. The residue not dissolved in boiling water, was digested in alcohol. It gave a very deep orange-coloured tincture, which did not lather. Water being added, threw down a grey flaky precipitate, of a brown colour when dry, and little more than one hundred and seventieth part of the original webs. On hot coals it swelled up, smoaked, and took fire; and from its habitudes in these respects, and with the alkalies, it resembled a resin. The diluted alcoholic solution being then evaporated, afforded a residue slightly deliquescent, of a taste at first sweetish, and afterwards bitter, and in quantity nearly three times that of the resinous precipitate. 5. The insoluble residue after this treatment with water and alcohol, burned without swelling up, and emitted a small quantity of white fumes having the smell of burned wood. Neither the oxigenated muriatic, nor the sulphureous acids, discoloured it. It was soluble with effervescence in muriatic acid, which took up two-thirds and left

left a black paſte. Ammonia ſeparated a brown matter in ſmall quantity from the clear ſolution; and this matter, when calcined, did not loſe its colour. It was almoſt totally ſoluble in muriatic acid, and this ſolution gave a black precipitate with nut-galls, and a blue with alkaline pruſſiate. The fluid to which the ammonia had been added, gave a grey precipitate by pot-aſh. This retained its colour when ignited, and was again ſoluble in muriatic acid with efferveſcence. 6. Cauſtic pot-aſh poured on the reſidue of ſpiders' webs previouſly treated with water and alcohol, diſengages a little ammonia, and partly diſſolves the matter. An acid throws down from this ſolution a black pulvurelent taſteleſs precipitate, which ſlightly puffs up by heat, and leaves by deficcation a brittle and apparently reſinous matter. Its quantity is about one-twelfth of the exhauſted matter made uſe of. It is partly ſoluble in volatile oils.

Analyſis of ſpiders' webs.

7. The aqueous extract of No. 3 being digeſted with alcohol, gave out one-ſeventh part. This alcoholic extract was brown, conſiderably deliqueſcent, and of a ſharp taſte. It ſwelled conſiderably on the coals, and at a certain period it burned rapidly, as if a nitrate were preſent. It efferveſced briſkly with ſulphuric acid, giving out a white vapour of a muriatic ſmell. Potaſh and lime diſengaged from this extract a ſtrong ammoniacal ſmell, and the vapours were very ſenſible on the approach of muriatic acid. The extract having been incinerated, appeared by ſeveral experiments to contain muriate of lime and a ſulphate. What remained of the aqueous extract after treatment with alcohol, was leſs deep in colour than before, had a puryurelent appearance, and ſlightly pungent taſte. On hot coals it did not ſwell up, but left a very abundant precipitate. Strong ſulphuric acid poured on this extract produced no ſenſible ſmell, and there was no production of ammonia when it was triturated with quick-lime.

8. Spiders' webs ſubjected to deſtructive diſtillation, gave firſt water ſlightly coloured, but becoming deeper as the proceſs went on; and afterwards a black thick oil, with carbonated hydrogen and carbonic acid. A very ſenſible ſmell of ammonia was developed, and a reſidual coal was left, amounting to half the matter employed. The coal after incineration left two-thirds of its weight, half of which was taken up by muriatic acid, and the remainder ſeemed to be ſilex and coally matter.

Analyfis of fpiders' webs.

matter. The muriatic folution, during evaporation, depofited fulphate of lime. When fpiders' webs were incinerated in an open veffel, the afhes were found to contain fulphate of lime, muriate of foda, and carbonate of foda. Muriatic acid applied to the refidue took up more fulphate of lime; and when this folution was treated with ammonia and afterwards with pot-afh, it gave oxide of iron, a little alumine, and fome lime. The undiffolved part was filex.

9. Spiders' webs were almoft totally diffolved in nitric acid amounting to fix times their weight; carbonic acid and nitrous gas being difengaged. The folution when evaporated let fall cryftals of fulphate of lime, and by continuing the evaporation, the yellow, bitter, deliquefcent matter, which Welter calls *amer*, was afforded.

Component parts.

Hence the author concludes that fpiders' webs are compofed of, 1. A brown extract foluble in water, and not changeable in the air; 2. A refinous extract foluble in alcohol, and very deliquefcent; 3. A fmall quantity of alumine; 4. Sulphate of lime; 5. Carbonate of foda; 6. Muriate of foda; 7. Carbonate of lime; 8. Iron; 9. Silex. The author thinks that the earths and earthy falts may be derived from the local fituation of thefe infects, and that it is probable that the webs of garden fpiders may not afford them. The two conftant products to which he demands particular attention, are thofe obtained from the aqueous and alcoholic folutions. He thinks it defirable to try their medical powers feparately. He fuppofes the refinous matter to be the fame fubftance as under other circumftances forms the fpiders' filk, and the wax which Mr. Accum has elfewhere mentioned as one of their products.

XII.

Information on the Mines and Manufactures of the Eaft Indies, and other Subjects. By J. MACHLACHLAN, *Efq. of Calcutta.**

SIR,

Account of fome receipts for dying.

SHOULD you think the enclofed receipts for dying the beautiful reds of the Coromandel coaft can be of any ufe to

* Soc. Arts, 1804; for which the filver medal was given.

the

the dyers of the united Britiſh kingdom, be pleaſed to lay them before the Society for the Encouragement of Arts, &c. that they may be publiſhed in the volume of their Tranſactions; if not, I truſt you will excuſe my troubling you with them. They were ſent to me from Madras by a ſcientific friend, who had the ſeveral operations, detailed in them, performed in his own preſence. I forwarded a copy of them, and a ſmall quantity of the ingredients mentioned in them, to a friend at home, ſeveral years ago; but he dying about or ſoon after the time of their arrival, I never learned what became of them. It ſtrikes me, however, that there is a conſiderable coincidence between the thread proceſs and that which I have ſeen recommended by Mr. Henry, of Mancheſter, for dying the Adrianople or Turkey red.

I am not certain whether it is known at home, that many of the hills in Bahar, and other parts of India, contain immenſe quantities of mica, talc, or Muſcovy glaſs. The natives of this country and China make very ſplendid lanterns, ſhades, and ornaments of it, tinged of various fanciful colours; and it is alſo uſed by them in medicine. When burned or calcined, it is, I am told, conſidered as a ſpecific in obſtinate coughs and conſumptions. When powdered, it ſerves to ſilver the Indian paper, &c. uſed in letter-writing; and, in fact, it is applied to numberleſs purpoſes. The bazar price of that of the beſt quality, ſplit into ſheets of about two lines thick, is ſix rupees the maund of 84lb. avoirdupois. If it could be applied to any uſeful purpoſe at home, it might go in part ballaſt of ſhips, and at a trifling expenſe. I encloſe a ſmall ſpecimen of it, and am,

Immenſe quantities of talc found in the hills in Bahar.

Sir,

Your very obedient ſervant,

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

N. B. The chaya, or red dye root of the Coaſt, is, I believe, known at home: as alſo the caſhaw leaves, which are uſed as an aſtringent.

Charles Taylor, Eſq.

Directions

Directions for dying a bright Red, four Yards of $\frac{3}{4}$ broad Cotton Cloth.

Instructions for dying cottons bright red by the Indian method.

1st. The cloth is to be well washed and dried, for the purpose of clearing it of lime and congee, or starch, generally used in India for bleaching and dressing cloths; then put into an earthen vessel, containing twelve ounces of chaya or red dye root, with a gallon of water, and allow it to boil a short time over the fire.

2d. The cloth being taken out, washed in clean water, and dried in the sun, is again put into a pot with one ounce of myrabolans, or galls coarsely powdered, and a gallon of clear water, and allowed to boil to one half: when cool, add to the mixture a quarter of a pint of buffalo's milk. The cloth being fully soaked in this, take it out, and dry it in the sun.

3d. Wash the cloth again in clear cold water, and dry it in the sun; then immerse it into a gallon of water, a quarter of a pint of buffalo's milk, and a quarter of an ounce of the powdered galls. Soak well in this mixture, and dry in the sun. The cloth, at this stage of the process, feeling rough and hard, is to be rolled up and beetled till it becomes soft.

4th. Infuse into six quarts of cold water, six ounces of red wood shavings, and allow it to remain so two days. On the third day boil it down to two-thirds the quantity, when the liquor will appear of a good bright red colour. To every quart of this, before it cools, add a quarter of an ounce of powdered alum; soak in it your cloth twice over, drying it between each time in the shade.

5th. After three days wash in clean water, and half dry in the sun; then immerse the cloth into five gallons of water, at about the temperature of 120 degrees of Fahrenheit, adding 50 ounces of powdered chaya, and allowing the whole to boil for three hours; take the pot off the fire, but let the cloth remain in it until the liquor is perfectly cool; then wring it gently, and hang it up in the sun to dry.

6th. Mix intimately together, by hand, about a pint measure of fresh sheep's dung, with a gallon of cold water, in which soak the cloth thoroughly, and immediately take it out, and dry it in the sun.

7th.

7th. Wafh the cloth well in clean water, and fpread it out in the fun on a fand-bank (which in India is univerfally preferred to a grafs-plat) for fix hours, fprinkling it from time to time, as it dries, with clean water, for the purpofe of finifhing and perfecting the colour, which will be of a very fine bright red. Inftructions for dying cottons bright red by the Indian method.

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

CHARLES TAYLOR, Efq.

Directions for dying of a beautiful red, eight ounces of Cotton Thread.

1ft. Put one gallon and a half, by meafure, of fap-wood afhes, into an earthen pot, with three gallons of water, and allow the mixture to remain twenty-four hours to perfect it for ufe.

2. Put the following articles into an earthen pot, viz. Three-quarters of a pint of Gingelly oil; one pint, by meafure, of fheep's dung, intimately mixed by hand in water; two pints of the above ley.—After mixing thefe ingredients well, pour the mixture gradually upon the thread into another veffel, wetting it only as the thread, by being fqueezed and rolled about by the hand, imbibes it, continuing to do fo until the whole is completely foaked up, and allow the thread to remain in this ftate until next day.

3d. Take it up, and put it in the fun to dry; then take a pint and a half of afh-ley, in which fqueeze and roll the thread well and allow it to remain till next day.

4th. Squeeze and roll it in a like quantity of afh-ley, and put it in the fun to dry; when dry, fqueeze and roll it again in the ley, and allow it to remain till next day.

5th. Let the fame procefs be repeated three or four times, and intermit till next day.

6th. Ley the thread once, as the day before, and, when well dried in the fun, prepare the following liquor: One gill of Gingelly oil; one pint and a half of afh-ley.—In this fqueeze and roll the thread well, and leave it fo till next day.

7th. Repeat the procefs of yefterday, and dry the thread in the fun.

Inſtructions for dying cottons bright red by the Indian method.

8th. The ſame proceſs to be repeated.

9th. Firſt repeat the aſh-ley proceſs three or four times, as under the operations 3, 4, and 5, and then prepare the following mixture: On pint of ſheep-dung water; one gill of Gingelly oil; one pint and a half of aſh-ley.—In this ſqueeze and roll the thread well, and dry it in the ſun.

10th. Repeat the ſame proceſs.

11th. Do. Do.

12th. Do. Do.

13th. Do. Do.

14th. Do. Do.

15th. Waſh the thread in clean water, and ſqueeze and roll it in a cloth until almoſt dry; then put it into a veſſel containing a gill of powdered chaya root, one pint by meaſure of caſhan leaves, and ten pints of clear water; in this liquor ſqueeze and roll it about well, and allow it to remain ſo till next day.

16th. Wring the thread, and dry it in the ſun, and repeat again the whole of the 15th proceſs. leaving the thread to ſteep.

17th. Wring it well, dry it in the ſun, and repeat the ſame proceſs as the day before.

18th. Do. Do.

19th. Do. Do.

20. Wring and dry it in the ſun, and with the like quantity of chaya root in ten pints of water, boil the thread for three hours, and allow it to remain in the infuſion until cold.

21ſt. Waſh the thread well in clear water, dry it in the ſun, and the whole proceſs is complete.

J. MACLACHLAN.

Calcutta, Oct. 4, 1803.

SCIENTIFIC NEWS.

Imperial Academy of Sciences at Peterſburgh.

THE Vice Admiral Tchitchagoff, Miniſter of the Marine department, has forwarded to the Academy a queſtion on the reſiſtance of fluids, and its application to naval architecture, for the ſolution of which that department will beſtow a reward of 1000 ducats of Holland, or 462*l.* 10*s.* Prize of the Ruſſian Imperial Academy.

The academy being deſirous of ſeconding the patriotic views of the marine department, decided on the publication of a program in the following terms.

Prize propoſed by the department of the Marine, on the 1ſt of *July*, 1804.

It is propoſed, that of the two theories of the reſiſtance of fluids propoſed and applied to naval architecture by Don G. Juan, in his *Examen Maritime*, and by M. Romme, in his *Art de la Marine*, one or the other of them, for example, that of Don Juan ſhould be corrected and improved to ſuch a degree, as to afford reſults that ſhall differ from the reſults of experiment, by ſo ſmall a quantity, as may be practically neglected without ſenſible error:—Or otherwiſe, if theſe theories cannot be corrected, it is propoſed, that a new theory ſhould be eſtabliſhed and applied to naval architecture, which ſhall lead to concluſions of the ſame degree of accuracy;—Or otherwiſe, laſtly, if it ſhould be impoſſible to eſtabliſh ſuch a theory, it is propoſed, that from experiments at leaſt, there ſhould be deduced a formula reſembling thoſe which have been given by Meſſrs. Boſſut and Prony; and ſuch that it ſhall be not only more conformable to experiments than thoſe formulas, but that it ſhall lead as nearly as poſſible to the concluſions drawn from experiments, even when the formule ſhall be applied to naval architecture. The ſubject. To improve the theories of the reſiſtance of fluids; or to eſtabliſh a new theory; or to deduce a formula from experiment.

For the ſatisfactory ſolution of this problem the department of the marine has appointed a prize of 1000 Ducats of Holland, and has fixed a term of two years to be reckoned from the date of this program. After the expiration of that term, no memoirs addreſſed to the academy will be received on this ſubject, the time appointed being ſufficient for thoſe new experiments which the ſolutions in queſtion render indiſpenſably One thouſand ducats, or 462l. 10s.

bly

bly neceſſary. The memoirs forwarded to the academy muſt be written in a diſtinct legible character, either in the French Engliſh or Ruſſian language.

(Signed in the original) PAUL TCHITCHAGOFF.

Terms and conditions for receiving memoirs.

In the invitation accompanying this program, the Academy requires men of ſcience who intend to make application for this prize, to addreſs their memoirs to the perpetual ſecretary of that body, before the 1ſt of July, 1806, and that the writer ſhould clear the poſt charges as far as the regulations of their reſpective countries will allow. The cuſtomary mode of marking the memoirs with a device or motto, and ſending at the ſame time a ſealed letter, having the ſame device, and containing the name and reſidence of the author, is alſo to be adopted in the preſent inſtance. The memoirs will be examined before the expiration of the term of concurrence, by the Department of the Marine and by the Academy; the latter of whom will publiſh the judgment they ſhall adopt, and the Department of the Marine will crown by the payment of the prize, the labours of that author who ſhall have ſatisfied the conditions of the program.

Prize concerning Light, propoſed by the Imperial Academy of Sciences for the Year 1806.

Prize concerning light.

The uſual theories that light is projected matter; or mere undulation of a fluid;—

—or it is a chemical principle.

Five hundred roubles, £.112 10 0

The queſtion.

After an introduction, in which a conciſe ſtatement is given of two theories reſpecting the nature of light, the one aſcribed to Newton, which ſuppoſes light to conſiſt in the emanations of matter from luminous objects, and the other aſcribed to Euler, which deduces the effects from vibrations of a peculiar elaſtic fluid,—they proceed to ſtate rather more fully a chemical hypotheſis of Lavoiſier, who not only conſiders light as cauſed by a peculiar matter, but alſo that this matter is ſubject to the elective attractions, ſo as by its combinations and diſengagements to produce an extenſive ſeries of phenomena, which are thus accounted for. It is principally with a view to develope this laſt hypotheſis that they have propoſed a prize of 500 roubles (£112 10 0.)

" For the moſt inſtructive ſeries of new experiments on light, conſidered as matter; on the properties which may with juſtice be aſcribed to it; on the affinities which it ſhall appear to have with other bodies, whether organic or inorganic, and

on

on the modifications and phenomena which are manifested in thofe fubftances by virtue of the combinations in which the matter of light may have entered along with them."

After propofing this queftion, the Academy proceeds to explain, by obferving that without entering into any hiftoric difcuffion, or the objections which have been oppofed to this hypothefis, nor the refearches already made with a view to develope traces of chemical action between light and bodies in the different modifications of natural phenomena,—the enquiries here propofed may not be unufefully extended to the galvanic fire, of which the dazzling brilliancy when large piles are made to act upon coaily matters, in fome refpect imitates the folar light. The Academy has chofen to enunciate the fubject of their prize in a general way, in order that philofophers may not be in any refpect impeded as to the points of view from which they may be difpofed to contemplate and to treat fo difficult a fubject, which has fcarcely yet been entered upon, though fo eminently worthy of attention from the cultivators of natural fcience. Remarks by the academy.

The memoirs are to be written either in Ruffian, French, Englifh, German, or Latin, and forwarded to the perpetual fecretary of the Academy, fealed up, with device and indicatory billet, as mentioned with regard to the former prize. No memoirs will be received after the 30th of April, 1806, inclufive, and the author of that memoir which in the judgment of the Academy fhall have merited the prize, fhall be proclaimed in the public meeting of the following month of July. The fuccefsful memoir becomes the property of the Academy, and muft not be printed without their formal permiffion. The other treatifes will be delivered to the refpective authors, on application to the fecretary, either perfonally or by procuration. Conditions and terms.

Voyages of Meffrs. HUMBOLDT *and* BONPLAND.

Meffrs. Levrault, Schoell and Company circulated at the beginning of the prefent year a profpectus of the voyage of Meffrs. Humboldt and Bonpland, the publication of which is committed to them: "the travellers, they obferve, have in general re-written all their obfervations, whatever might have been the object, in the narrative of their voyage. Mr. Humboldt has thought it proper to follow another courfe, and to Voyages of Humboldt and Bonpland.

treat

Humboldt's travels.

treat feparately all thofe objects which confiderably differ in their nature. He has determined accordingly to publifh in detached collections, all that more particularly belongs to Aftronomy, Geology, Botany, Zoology, &c. and his voyage, properly fo called, will embrace all that relates to General Phyfics, the Origin of Nations, their Manners, their Intellectual Culture, Antiquities, Commerce, and Political Economy. Upon this part of his obfervations and the hiftory of his voyage, he will not at prefent publifh more than a narrative, under the title of *Relation abrégé*, &c. or an abridged Relation of a Voyage to the Tropics, performed in the interior of the new continent during the years 1799, 1800, 1801, 1802, and 1803.

It is agreed by Meffrs. Humboldt and Bonpland, who are connected by the moft intimate fentiments of friendfhip, and have fhared together in all the fatigues and dangers of this voyage, that the whole of their publications fhall be in their joint names. The preface to each work will announce to which of them the feveral parts are refpectively to be afcribed. The lift of works fpeedily to appear are as follows:

1. The abridged Relation of the Voyage, in quarto; promifed in July, 1805.

2. A Collection of Aftronomical Obfervations and Admeafurements made on the new continent; promifed in 1805.

3. An Effay on the Geography of Plants, or a Philofophical Sketch of the Equinoctial Regions; founded on obfervations made from the 18th degree of fouth latitude, in the years before mentioned, with one large plate, coloured; promifed in June, 1805.

4. Equinoctial Plants, collected in Mexico and the Ifle of Cuba, in the Provinces of Caracas, Cumena and Barcelona, on the Andes of New Grenada, Quito and Peru, and on the banks of Rio Negro, Oroonoko and the River of Amazons, with plates engraved by *Sellier*, in folio; the firft number to appear in April, 1805.

5. A Collection of Obfervations of Zoology and comparative Anatomy, made in a Voyage to the Tropics; with plates engraved by *Bouquet*, coloured or not, at the option of the purchafer. The firft number to appear in May.

N.B. All thefe works will bear the general title of *Voyage de M.M. Alexandre de Humboldt et Aimé Bonpland*, and will form

form a collection of the same size and type, except the Equinoctial Plants, which are larger. Subsequent notices of the price and publication are to appear in the journals.

Fish ejected from Volcanoes.

AMONG the great number of facts which Humboldt has collected in his voyage, the following lately communicated to the National Institute is very curious. Several volcanoes of the Cordilleras of the Andes occasionally throw out eruptions of mud mixed with large volumes of fresh water, and what is most remarkable, an infinite number of fishes. The volcano of Imbaburo, among others, threw out at one time so great a number near the town of Ibarra, that their putrefaction occasioned disorders. This phenomenon, astonishing as it appears, is not even extraordinary, but, on the contrary, of considerable frequency, so that the facts are authentically preserved in the public registers, along with those of earthquakes. It is more particularly singular that these fish are not at all injured, though their structure is very soft. They do not even appear to have been exposed to a high temperature; for the Indians assert that they sometimes arrive at the foot of the mountain still living. These animals are sometimes thrown out of the mouths of the crater and sometimes through lateral clefts; but always at the height of 12 or 1300 toises or fathoms above the surrounding plains. Humboldt thinks they are produced in lakes situated at that height within the crater; and it is a confirmation of this opinion that the same species are found in the brooks which run at the foot of the mountains. It is the only species which subsists at the height of 1400 toises. It is a new species to naturalists. Humboldt made a drawing of it on the spot, and gave it the name of *Pimelodrus Cyclopum* or *thrown by the Cyclops*. It will be found in the first number of his Zoology.

Volcanic eruption of fishes.

Water formed by Mechanical Pressure.

IN a sitting of the French National Institute at the commencement of the present year, M. Biot read a note on the formation of water by mere compression. The experiment of forming water out of its component parts oxygen and hydrogen by burning those gases by the electric spark is well known. M. Biot has succeeded in determining this combination independently of electricity, by rapidly compressing a mixture of the

Combustion of oxygen and hydrogen by mechanical pressure.

the two gaſes included in the ſyringe of an air-gun. The compreſſion which forces the particles of gas together, cauſes them to give out a ſufficient quantity of heat to ſet them on fire. Some caution is requiſite in repeating this experiment, which is not without danger. Out of three times that M. Biot made it, there were two in which the braſs cap of the pump and the barrel itſelf, which was iron, were broken by the exploſion.

Malleability of Zinc.

Zinc is malleable while heated between 210° and 300°.

A VERY curious and uſeful diſcovery has been made by Meſſrs. Charles Hobſon and Charles Sylveſter, both of Sheffield, that zinc is in fact a malleable metal. The laminability of this metal to a certain conſiderable degree, has long been known; but it was not ſuſpected that it is capable of being forged and drawn into wire. They have found that at a temperature between 210° and 300° of Fahrenheit zinc yields to the hammer, and alſo that it may be wire-drawn or laminated by keeping it at this temperature during the mechanical operation. An oven or a hollow metallic veſſel kept at a due heat may be uſed for the pieces, in the ſame manner as the Smith's forge is uſed for iron and ſteel. It appears that the zinc, after having been thus annealed and wrought, continues ſoft, flexible and extenſible, and does not return to its former partial brittleneſs, but may be bended and applied to the uſes for which zinc has hitherto been thought unfit, ſuch as the fabrication of veſſels, the ſheathing of ſhips, and numerous other important applications. I have ſeen a chaſed or ſtamped figure raiſed at one ſtroke in thin zinc; which is, I think, as much elevated as it could have been in copper.*

Palladium.

Palladium.

BY a letter from Meſſrs. Knight, of Foſter-lane, I am informed that the new metal, palladium, may be purchaſed at their warehouſe.

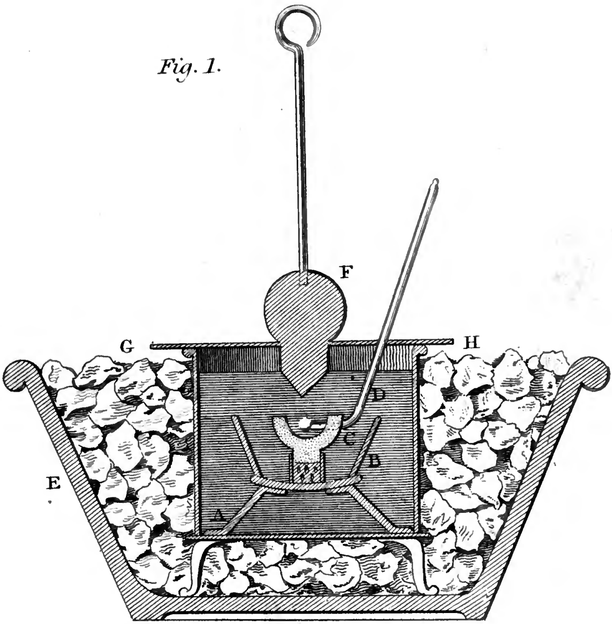
Erratum corrected.

Mr. GREGORY begs leave to correct a miſtake which occurs in his paper on horſe-powers, occaſioned by his inadvertently copying from ſome of his former obſervations on that ſubject the fraction $\frac{9}{4}$ inſtead of $\frac{9}{5}$ for the approximate value of the exponent n: this makes a change in the reſults of the computation at page 148; for $(9-3)^{\frac{9}{5}} : (9-4.4)^{\frac{9}{5}} :: 130 : 80{\cdot}58$, or nearly $80\frac{1}{2}$ lb. inſtead of $71\frac{1}{2}$, as there given. He alſo points out a preſs error at p. 153, where the ſymbols of multiplication in each of the theorems ſhould have been ſigns of addition.

* The inventors have obtained a patent.

New Experiments by Count Rumford, to prove the maximum of Density in Water above 32° fahr.

Fig. 1.



Chevalier Edelcrantz's Valves for Steam Engines.

Fig. 2.

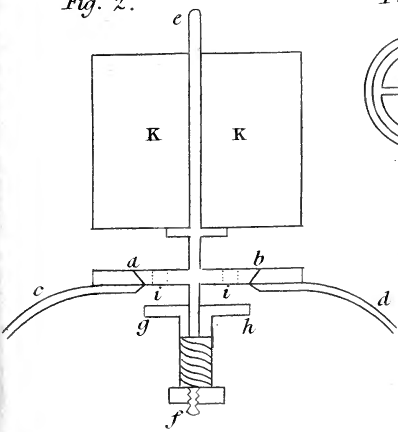


Fig. 3.

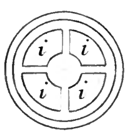
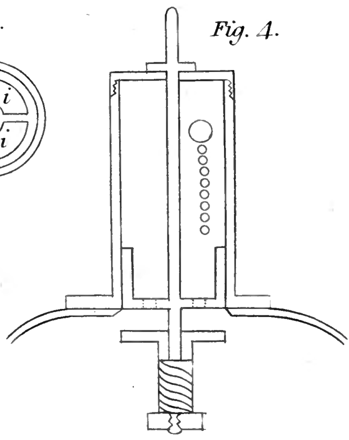
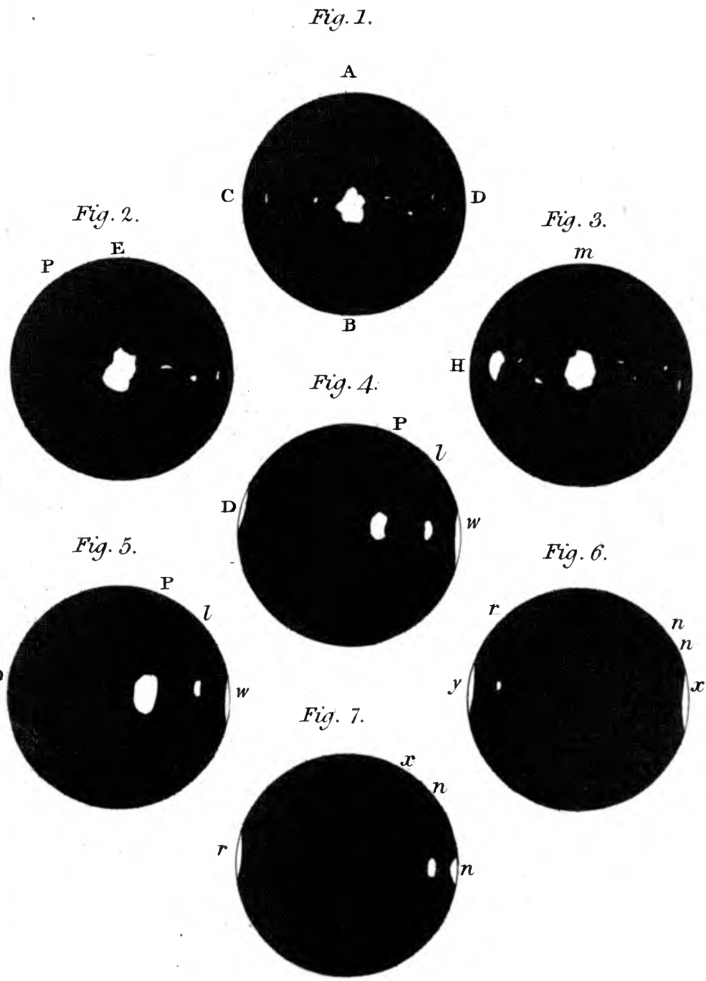


Fig. 4.



Variable Star in Sobieski's Shield.



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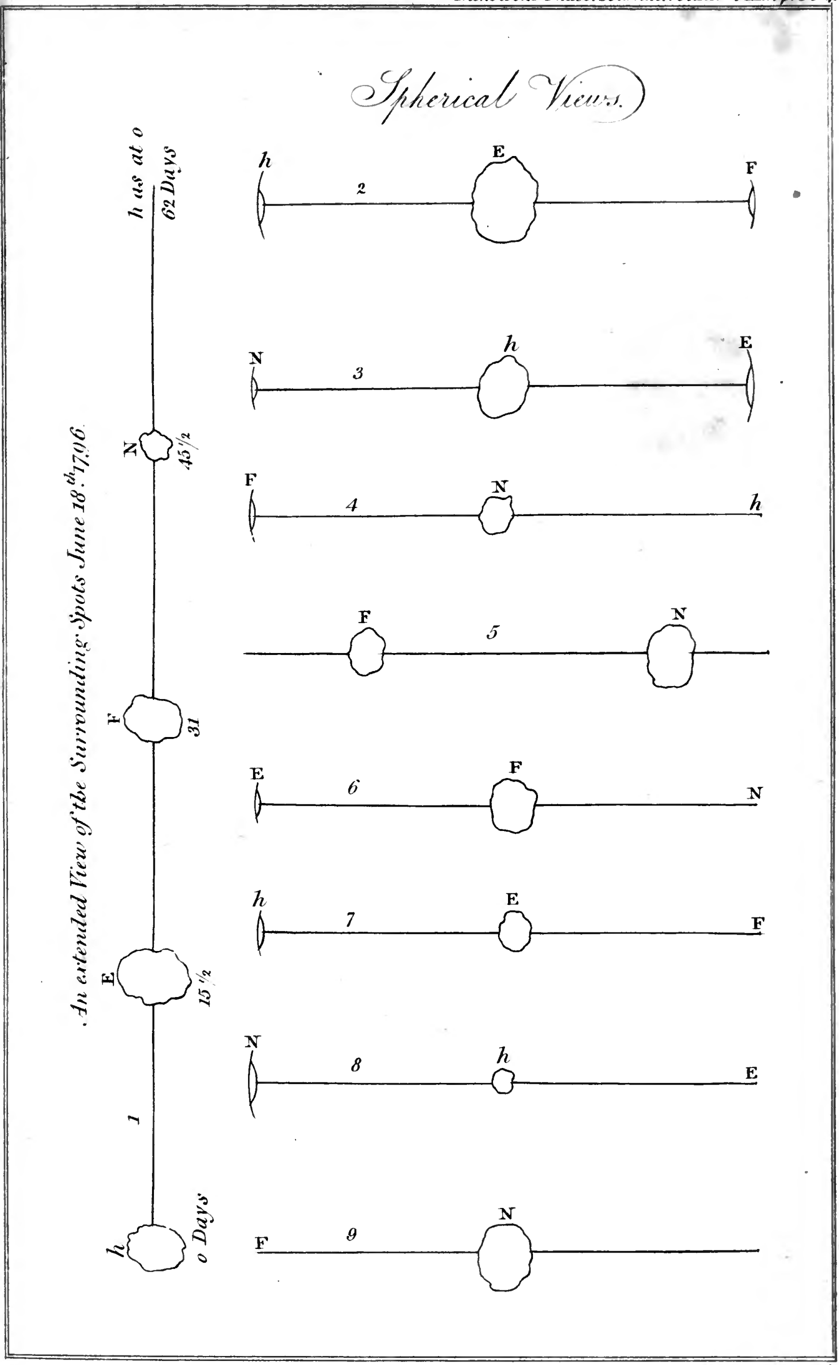


Mutlow Sc. Russell Ct.

Mr. Harrison's Press for Botanical Specimens.

Fig. 1.

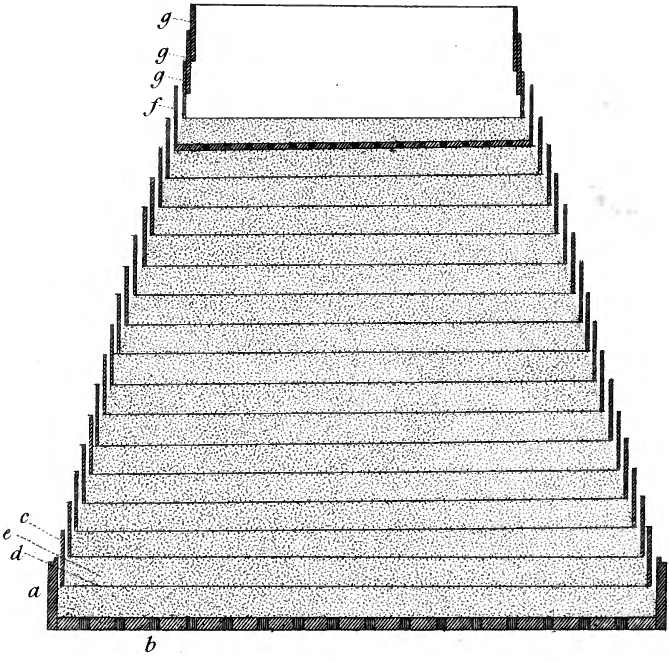
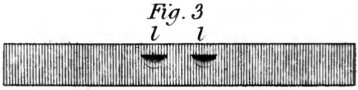


Fig. 2.

Fig. 3



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THE END OF THE ELEVENTH VOLUME.

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